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Three essays on public health insurance, quality, access and cost of health care

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Abstract

Medicaid and Medicare are two major public programs that help vulnerable groups of people to gain coverage of health care services. There are various ongoing debates on the Medicaid- and Medicare-related issues. Among those, some topics draw most of attentions.

First of all, how do we contain cost of Medicaid programs? In the 90's, Medicaid expenditures grew fast. In response to this, many states began to enroll large numbers of Medicaid patients in managed care programs. The first chapter examines the effect of Pennsylvania Medicaid mandatory HMO program, HealthChoices program on the outcomes of pregnant women. I utilize the Pennsylvania Health Care Cost Containment Council inpatient data file and American Hospital Association survey data to perform difference-in-difference-in-difference estimation and find robust results indicating that HealthChoices program helps Medicaid mothers reduce the incidence of preventable complications, utilization of C-section procedure and decrease the delivery charge.

Second, how do we improve the access to a certain health care service. In January 2006, Medicare introduced a new prescription drug benefit through Part D, therefore lowering the out-of-pocket cost of prescription drug for Medicare beneficiaries. The second chapter uses data from the National Ambulatory Medical Care Survey (NAMCS), Medical Expenditure Panel Survey (MEPS) and the National Inpatient Sample from the Healthcare Cost and Utilization Project (NIS-HCUP) 2002-2004 and 2006-2009 and a difference-in-discontinuity approach to estimate the differential discrete jumps in outcomes at 65 years old for the sample after 2006 and before 2006. We find a 33% increase in the number of prescription drugs and a 55% increase in the number of generic drugs prescribed in physicians' offices for each visit following policy implementation. We also find the existence of anticipatory effects for prescribing patterns before the adoption of Part D. We do not find evidence that Part D resulted in significant changes in medical expenditures

for other services or inpatient health outcomes.

Last but not the least, how do we assure the quality of health care for vulnerable groups of people while containing costs of the program? Centers for Medicare and Medicaid Services started "the Medicaid/CHIP Quality Initiative", and promoting Pay-for performance (P4P) program for Medicaid managed care plans as a part of this initiative. The third chapter studied the effect of Medicaid managed care P4P programs on the use of preventive care services and the different effectiveness of P4P incentive designs. We used data from the National Health Interview Survey (NHIS), and the National Immunization Survey (NIS). Results suggested that among those on Medicaid, state adoption of a P4P program is positively associated with the likelihood that adults have received mammograms, blood pressure checks and Pap smear tests. P4P adoption is also associated with increases in the probability that children on Medicaid are up-to-date on the Haemophilus influenza type B vaccine, the 4:3:1, and the 4:3:1:3:3 vaccine series, respectively. These average effect sizes are larger in states with higher Medicaid managed care penetration rates and in states that use negative financial incentives, such as withholds and penalties, rather than states with positive financial incentives and non-financial incentives.

Chapter 1

The Pennsylvania HealthChoices Program

Pregnancy Outcomes for Medicaid Patients in Mandatory Managed Care

1.1 Introduction

The literature on Medicaid managed care is now substantial, but has not reached a clear conclusion as to whether managed care can achieve the same or better health outcomes more efficiently than traditional fee-for-service (FFS) reimbursement systems. Given that one in five Americans now relies on Medicaid to pay for healthcare and that many states have turned to managed care as a strategy to control costs, determining the net impact of managed care on care utilization and on health outcomes for Medicaid enrollees is vital.[1] However, measuring this impact is difficult because a number of confounding factors may bias estimates of managed care's effect. In this paper we take advantage of Pennsylvanian's staggered adoption of mandatory managed care and use difference-in-difference (DD) and difference-in-difference-in-difference (DDD) methods to study its effect on health care and outcomes for pregnant Medicaid patients.

Pennsylvania began implementing its mandatory managed care program, HealthChoices, when it received a 1915(b) waiver from the Health Care Financing Administration (HCFA, now known

[1] See `http://www.medicaid.gov/Medicaid-CHIP-Program-Information/By-State/By-State.html`.

as Centers of Medicare and Medicaid Services or CMS) in 1997. Although Pennsylvanian's move to managed care had as explicit goals both efficiency and preservation of the quality of care, implementation of HealthChoices raised concerns among advocates for low-income Pennsylvanians. [2] The dual goals may be achieved if managed care can provide the same or better care at lower cost, but it is also possible that the incentives created by HealthChoices will cause managed care organizations to reduce costs by reducing the quality of care.

Under traditional FFS reimbursement programs, patients may seek covered services from any physician or hospital that participates in the Medicaid program, and providers are reimbursed by the state for the services provided according to a schedule of fees. FFS reimbursement thus gives physicians and hospitals an incentive to provide unnecessary services. Managed care comes in a variety of types, but the classic arrangement, and the focus of our interest, is care that is managed by a health maintenance organization (HMO). In this type of managed care, the state pays the HMO a flat fee for each Medicaid patient that the HMO enrolls (premia may be risk-adjusted to reflect the greater costs associated with specific patients). The HMO in turn identifies and reimburses the physicians and hospitals that its enrollees may use. The flat fee paid by the state gives the HMO an incentive to reduce the reimbursements it pays to doctors and hospitals for patient care.

HMOs pursue lower costs by reimbursing the providers in their networks on a capitated basis, giving the doctors and hospitals an incentive to eliminate unnecessary services. Further, HMOs may reduce costs and achieve better health outcomes by providing more preventive and more coordinated care to patients that may be ill-informed about recommended health measures. However, HMOs may also reduce costs by restricting provision of necessary care, a strategy that will also be more effective if patients are ill-informed.

We study these issues by examining the impact of Pennsylvania's switch to mandatory managed care on health and cost outcomes for pregnant women. Much of the literature on managed care and Medicaid examines the experiences of pregnant women and their babies. Pregnant Medicaid beneficiaries often lack a regular source of early and comprehensive prenatal care, and

[2] Both Pennsylvanian's Department of Public Welfare (DPW) and the federal government had these goals in mind. HCFA approved 1915 (b) waivers under the condition that states receiving the waiver demonstrate that their proposed program was "cost-effective and efficient" but also that its restrictions of Medicaid beneficiaries' choices not "substantially impair access to covered services of adequate quality where medically necessary."

have substantial rates of smoking and substance abuse, and may therefore be more likely to have pregnancy-related complications that place their health and their babies' health at risk, increasing the cost of their care (Goldfarb et al., 1991). Managed care plans have the potential to provide more comprehensive prenatal care to these women, encouraging healthy behaviors during pregnancy that reduce the chances of complications and help to identify potential problems before delivery. We measure the impact of Pennsylvania's managed care program on both health outcomes and utilization of care by measuring its effect on the rate of preventable complications, on the rate of Cesarean sections (C-sections), and on the total cost of hospital admissions associated with each pregnancy.

Managed care is rarely implemented for a random selection of Medicaid patients, so estimates of its effects based on comparing outcomes for patients covered by managed care to outcomes for those who are (or were) not may be biased by self-selection, by the influence of contemporaneous trends, or by heterogeneity among patient populations or individual patients (see Levinson & Ullman, 1998, for a succinct summary). However, in Pennsylvania the new managed care program was mandatory, reducing the bias that self-selection might otherwise introduce. Second, the state introduced managed care for Medicaid patients at different times for different regions, allowing us to use DD to net out the effects of contemporaneous changes that may be affecting all Medicaid patients, not just those subject to the policy change. Third, we use changes in the outcomes of commercially-insured mothers to net out any common shocks that may be affecting all mothers, whether Medicaid or commercially-insured, and so are not due to the effects of the policy change. Finally, we control for the potentially confounding effects of heterogeneity among mothers arising from differences in their health behaviors or attitudes by using mother fixed effects, thus measuring the impact of the policy change by measuring changes in outcomes for women who gave birth more than once during the study period.

Our results show that in Pennsylvania managed care is associated with a lower rate of preventable complications, a result that is robust to changes in the specifications. We also find that the reduction in preventable complications is greater for high risk mothers, while at the same time the probability of these mothers receiving a C-section is higher, results suggesting that better overall health outcomes may have been achieved through more intensive care of higher risk

patients. Finally, we find some evidence suggesting that these beneficial health outcomes are eventually matched by reductions in costs, although cost reductions take several years to develop.

The plan of the paper is as follows. We briefly review the literature in section 2, and explain how our paper contributes to the research, and in section 3 we then describe the implementation of HealthChoices in Pennsylvania in greater detail. In section 4 we explain our empirical strategy and describe our specifications. Section 5 discusses our data sources, our sample, and the variables used in the estimations, and we present the estimation results and robustness checks in section 6. We consider the possible effects of patient health status and of adjustment lags in section 7, and we conclude in section 8.

1.2 Literature Review

Most studies of the impact of Medicaid managed care use data from birth certificates on the health outcomes for infants delivered by women who are, or who, because of their income and education levels are likely to be, enrolled in Medicaid. In a number of papers the birth certificate files are linked to Medicaid files, giving researchers a way to connect Medicaid claims, for example for prenatal care, to the outcomes for infants. Researchers usually have information about the mother (age, race, income, sometimes education and marital status), and a few also have some controls for the quality of the hospital where the delivery takes place (e.g., whether the hospital is a teaching hospital; whether there is a neonatal intensive care unit). Many therefore examine whether, controlling for mother and some hospital characteristics, managed care improved utilization of prenatal care and/or improved health outcomes, usually the incidence of low birth weight babies (Goldfarb et al., 1991; Carey et al., 1991; Kreiger et al., 1992; Shulman, 1997; Levinson and Ullman 1998; Oleske et al. 1998; Griffin et al., 1999; Moreno, 1999; Conover et al., 2001; Koroukian et al. 2001; Tai-Seale et al. 2001; Howell et al, 2004; Kenney et al., 2005; Sommers et al., 2005, Aizer, et al., 2007). In some papers, changes in the rate of C-sections (Carey et al., 1991; Moreno, 1999; Koroukian, 2001) or changes in expenditures (Tai-Seale et al, 2001; Duggan, 2004) are examined.

Despite similarities in the data analyzed in these papers, they differ in important ways. First, studies of the impact of managed care are necessarily done at the state level, and the states

examined differ in the types of managed care arrangements they instituted (e.g., owner-type, degree of competition), in their prior experience with managed care for Medicaid patients, and in how quickly the new programs were implemented, all of which may have affected the performance of these plans.[3] Further, some states expanded eligibility for Medicaid for pregnant women at the same time as they were implementing managed care.

Second, papers differ in the stringency with which they are able to control for potentially confounding factors. For example, some studies compare outcomes of Medicaid women before and after managed care is adopted state-wide (Griffin et al., 1999; Moreno, 1999). However, their results may be affected by the influence of contemporaneous changes not related to the implementation of managed care: for example, changes in prenatal care use after managed care implementation could be due to shifts in the composition of the population, or to changes in the quality of care for all patients in the area because of new practice patterns or technology.

Other studies compare outcomes for Medicaid patients enrolled in managed care to those enrolled in FFS systems during the same period (Krieger et al., 1992; Goldfarb et al. 1991; Levinson and Ullman 1998; Koroukian et al. 2001; Carey et al., 1991; Oleske et al. 1998; Tai-Seale et al. 2001). These studies mostly rely on managed care being mandatory, or the result of administrative selection, to control for biases that would be introduced if patients were free to choose whether to enroll in managed care: for example, sicker women may be more likely to choose FFS over managed care if they have a choice. However, these papers are comparing outcomes for different groups of women, frequently in different counties: counties might have different economic and other conditions that may be correlated with the health status of their Medicaid patients.

A few studies adopt the DD strategy we use in this paper, comparing changes in outcomes for patients that switch to managed care to changes in outcomes for those that remain in a FFS control group (Conover et al., 2001; Sommers, et al., 2005). This strategy differences out the common trend experienced by both control and treatment groups, such as parity risk or changes in medical practice standards.[4] One such paper, in this case a study of outcomes in counties

[3]Kaestner et al. (2005) is an exception: they use a national sample of birth certificates to study the impact of managed care. Their approach allows them to analyze a large sample, but they must use proxies for a mother's insurance status because the birth certificates do not contain insurance information.

[4]This approach is subject to the criticism that the policy itself might be endogenous in that there may be social or political reasons why some counties use managed care program while others remain with FFS. If these socio-political circumstances are correlated with the health of their citizens, the results will be biased. We are less

with mandatory as opposed to voluntary managed care (Kenney et al., 2005), also includes a DDD estimation that uses commercially-insured mothers to control for factors that might affect all mothers, whether in the Medicaid program or not.

Finally, two studies attempt to control for the effects of heterogeneity in individual mothers that may be affecting outcomes by focusing only mothers that give birth before and after the switch to managed care. Howell et al. (2004) compare changes in outcomes for women that remained in FFS to those that switched from FFS to managed care using a sample of Medicaid enrollees that gave birth twice within a five year period. Aizer et al. (2007) uses mother fixed effects, estimating the impact of managed care by changes in the outcomes for women that gave birth before and after the regime change.

Perhaps as a result of both the different states studied and the different empirical approaches employed, the evidence about the impact of managed care is mixed. A number of studies found some differences in prenatal care but no difference in the birth weight for infants between Medicaid patients using managed care and those in FFS systems (Goldfarb et al., 1991; Carey et al., 1991; Kreiger et al., 1992; Levinson & Ullman, 1998; Duggan, 2004; Howell et al, 2004; Kenney et al; 2005; Sommers et al., 2005). Others found that health outcomes were worse (Oleske et al, 1998; Conover et al., 2001). In one disturbing study, managed care was associated with lower quality prenatal care and increases in the incidence of low birth weight, prematurity, and neonatal death (Aizer et al., 2007). C-section rates appeared to be either unaffected by managed care (Carey et al., 1991; Oleske et al., 1998; Koroukian et al., 2001) or reduced in some cases (Howell et al, 2005).

In this paper we use hospital in-patient records from Pennsylvania rather than birth certificates and Medicaid files to generate our outcome variables. The in-patient records contain detailed information about a mother's health status, her delivery experience, charges for any hospital stays during her pregnancy and delivery, and her insurance status, but do not contain information about prenatal care, nor about infant outcomes. We therefore use the occurrence of preventable complications as our measure of health outcome, the occurrence of C-section as

concerned about this problem because in Pennsylvania the original plan was for Medicaid patients in all counties to be switched to managed care, and the order in which different regions adopted managed care seems mostly to have been driven by population, with the most populous regions converting first.

a measure of utilization, and the information about hospital charges to develop a measure of cost. Using inpatient records for Medicaid patients in a number of counties over a 10 year period provides us with a much larger sample of pregnancies than most studies to estimate the effects of managed care, which may be small. Using these records also allows us to link patient outcomes to information about the hospital where the birth took place so that we can include more controls for variations in a hospital's quality and its competitive situation.

1.3 HealthChoices

Pennsylvania obtained permission from HCFA to require Medicaid recipients to use an HMO for their health care in January 1997, and almost immediately began a phased implementation of HealthChoices, its new mandatory managed care program. In this program, which is administered by the Pennsylvania Department of Public Welfare (DPW), Medicaid patients are provided with a choice of several HMOs, and each must select an HMO to manage their healthcare, or, if they have not done so, they are randomly assigned to one. (The state hired a separate company to provide information about the HMO choices and to facilitate enrollment.) When choosing an HMO, clients also choose a primary care physician, which choice they are able to alter at any time without cause. Pennsylvania reimburses the HMOs for their enrollees' care on a fixed-rate, pre-paid basis.

The state had several advantages that increased the likelihood of a successful switch to mandatory managed care. It already had experience working with HMOs to provide Medicaid services, principally to children, the elderly, and the disabled, as well as some experience with mandated managed care for Medicaid patients, having run a pilot program called HealthPASS in some areas of Philadelphia.[5] The state also had the benefits of an effective administrative structure to oversee the program, as well as a strong advocacy group to act as an independent monitor (Johnston, 2003).

There were some changes in the program over time. For example, the state originally planned to use HMOs that specialized in Medicaid patients only.[6] The state also planned that clients

[5] In 1996, the share of Pennsylvania's Medicaid beneficiaries enrolled in some form of managed care was already 32 %.

[6] While specialization might make an HMO more cognizant of the particular problems of Medicaid patients, such

would have at least four HMOs to choose among: implementation zones were organized so that each HMO could expect to enroll at least 35,000, the anticipated break-even point (Johnston, 2003). As the program expanded to different areas, however, these parameters changed: the state now ensures that at least three firms operate in each zone, some of the current HMOs providing services to Medicaid patients also serve other populations, and HMOs are now expected to enroll at least 10,000 patients.

Pennsylvania's original plan was to implement HealthChoices in phases, with the goal of moving the entire state to mandatory managed care by 2005 (Johnston, 2003). The program was first introduced in the five counties in and around Philadelphia (HealthChoices Southeast or Zone 1) in February 1997.[7] Implementation continued early in 1999 when the 10 counties in the southwest corner of the state around Pittsburgh (HealthChoices Southwest or Zone 2) adopted the program, and late in 2001, when the ten counties in and around the Lehigh Valley and Harrisburg (HealthChoices Lehigh/Capital or Zone 3) switched to the mandatory managed care program.

However, in 2003 the planned expansion was halted, leaving the state with three different types of Medicaid provision: the three zones, comprising 25 counties, with mandatory managed care ("HealthChoices counties"); 25 counties that reimbursed Medicaid providers mainly through FFS but that also had HMOs that Medicaid patients could elect to use voluntarily (Voluntary counties); and 17 counties that reimbursed providers of Medicaid services on a FFS basis (FFS counties).[8] See Table 1.1 and the Figure 1.1.

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as transportation needs, it also makes the organization more dependent on the state for its revenue, raising the possibility that budgetary pressures at the state level would lead to lower fees for HMOs, which in turn might cause reductions in necessary care to Medicaid patients.

[7]HealthChoices subsumed HealthPASS, the earlier pilot program of mandatory managed care for about half of the Medicaid patients in Philadelphia, so some patients in Zone 1 were already enrolled in managed care when HealthChoices was implemented. HealthPASS apparently had no impact on prenatal care or birth outcomes, possibly because women in HealthPASS could opt for FFS to cover their obstetric care (Goldfarb et al., 1991).

[8]Pennsylvania resumed expanding HealthChoices to all counties in July 2012, with a planned completion date for full conversion of March 2013 (Pennsylvania Health Law Project, 2012).

1.4 Empirical Strategy

1.4.1 Difference-in-Difference Estimation

We first estimate a DD specification using our subsample of Medicaid patients only. In this estimation, we assume that the common trends for Medicaid patients in all counties are similar, except for the effects of imposing managed care in HealthChoices counties, and we estimate the effect of the policy by comparing the change in outcomes occurring in the HealthChoices counties to the change in outcomes occurring in the control counties.

The pregnant Medicaid patients in the 25 HealthChoices counties therefore represent our treatment group, where the treatment is switching these patients from FFS to mandatory managed care. The pregnant Medicaid patients in the 17 FFS counties represent an obvious control group, and we use them in that way. However, the FFS counties are largely rural, and the patient populations have a different demographic profile. Although we include variables to control for variation that may arise from differing patient or geographic characteristics, our estimates may reflect the effect of uncontrolled variation associated with these differing populations. For example, if the policy's implementation would cause less substantive change in more rural counties, then estimates of the managed care effect found by comparing changes in outcomes in HealthChoices counties to changes occurring in the FFS counties would suggest a stronger effect than would be the case if Medicaid patients in FFS counties actually switched to managed care.

The Medicaid patient population in the 25 Voluntary counties, on the other hand, is more similar to that of the HealthChoices counties, suggesting these counties as an alternative control group. (See Table 1.2.) However, because managed care was available in these counties on a voluntary basis, the treatment of moving from FFS to mandatory managed care in the HealthChoices counties is only partial relative to the Voluntary counties, as some patients in those counties also enrolled in managed care. Estimates of the impact of managed care would therefore be biased downward, when measured relative to the Voluntary counties.

Consequently, we report estimates for the effect of managed care using the FFS counties and the Voluntary counties as alternative control groups. We expect estimates using the FFS counties as a control will represent an upper bound on the impact of managed care on patient outcomes,

while estimates using the Voluntary counties as a control will represent a lower bound, given the partial nature of the treatment.

Table 1.3 shows the percentage of pregnant Medicaid patients enrolled in managed care in each group of counties during the years of our study, with the first three rows showing the enrollment figures for the three HealthChoices zones, and the fourth and fifth rows showing enrollment in the Voluntary and FFS counties. The numbers for the HealthChoices counties show the jump in managed care enrollment in the year mandatory managed care was adopted. However, the numbers also indicate that some women in the mandatory zones were not enrolled in managed care after the counties switched. Some of these discrepancies, which shrank over time, may reflect coding error and learning on the part of hospitals. More importantly, women that were already pregnant at the time of the switch to HealthChoices were allowed to stay with their current OB-GYN until the end of their postpartum period even if that doctor was not part of the system the patient selected, which also delayed the point of complete conversion.[9] However, because we use the outcomes of all Medicaid patients in the HealthChoices counties to measure the effect of managed care, our estimate will be biased toward zero if some patients were still enrolled in the FFS system, that is, we estimate the effect of the "intent to treat" rather than actual implementation of HealthChoices if some fraction of the Medicaid patients in the HealthChoices counties were not covered by managed care.

The fourth and fifth rows of Table 1.3 show the percentage of patients enrolled in managed care in each of the two control groups. While the percent of such patients is much smaller, the numbers show that use of managed care increased in all counties over the period, even those that purportedly relied entirely on FFS, perhaps in anticipation of the planned state-wide implementation of HealthChoices.[10] Consequently, we include a group-specific linear time trend for each of these five groups of counties in our estimations to control for the general increase in enrollment in managed care by Medicaid patients.

[9] Existence of the exemption was verified through personal communication with Pennsylvania Health Law Project, Pittsburgh, PA, 2012.

[10] HMO participation by Medicaid patients living in two "FFS-only" counties was confined to only two of the 17 counties in this group.

1.4.2 Difference-in-Difference-in-Difference Estimation

The DD estimation assumes that the only different shock affecting Medicaid patients in the treatment counties stems from the switch to managed care; otherwise, forces causing changes in outcomes for Medicaid patients in treatment and control counties are similar. However, county-specific shocks, such as economic downturns, may also affect outcomes for Medicaid patients; if that is the case, the DD methodology will cause these effects to be included in the measurement of the effect of the policy change.

However, if we assume that mothers with commercial insurance experience these same idiosyncratic shocks, then we can use changes in the outcomes of commercially-insured mothers to control for their effect. In essence, we estimate a DD specification identical to the DD specification we estimate for the subsample of Medicaid mothers, but instead use the subsample of commercially-insured mothers in the treatment and control counties. The result is the estimated difference between changes in outcomes of commercially-insured mothers in the HealthChoices counties and changes in outcomes for commercially-insured mothers in the control counties. The DDD estimation procedure finds the difference in these two DD estimations, that is, the effect of the policy for Medicaid mothers will be estimated having controlled for changes that are affecting Medicaid mothers in both the treatment and control counties, and having controlled for shocks that also affect commercially-insured mothers in the treatment and control counties (and thus are not the result of the policy change).

The DDD specification includes the most extensive set of controls for unknown sources of variation that might bias our estimate of the effect of Medicaid managed care and thus is our main specification. However, in the DDD estimation outcomes for commercially-insured mothers in HealthChoices counties are assumed to be unaffected by the implementation of the HealthChoices program. If the HealthChoices program reduces the revenues that providers earn from Medicaid patients, they may increase charges or increase services provided to the commercially insured patients, in which case using commercially-insured patients as a control for region-specific trends in the regression may cause the effects of HealthChoices on cost and utilization to be overstated. We therefore present estimation results for both the DD and DDD specifications.

1.4.3 Mother Fixed Effects

We estimate all DD and DDD specifications with mother fixed effects included, because the health behaviors or attitudes of individual mothers may affect the outcomes of their pregnancies. Therefore, in the DD estimation we estimate the impact of managed care for Medicaid mothers by comparing changes in outcomes for Medicaid mothers in the HealthChoices counties who have more than one pregnancy during our sample period before and after managed care was implemented, to changes in outcomes for Medicaid mothers who have more than one pregnancy during our sample period but live in a county in the control group of counties. In the DDD estimation we compare the changes in outcomes for Medicaid mothers in the treatment counties who have more than one pregnancy before and after HealthChoices was implemented to those for commercially-insured mothers in the treatment counties who have more than one pregnancy before and after HealthChoices was implemented, relative to the difference in the change in outcomes (the DD) between Medicaid and commercially-insured mothers giving birth more than once in the control counties.

1.4.4 Specifications

The DD specification we estimate using the subsample of Medicaid mothers is:

$$Outcome_{ijkt} = \beta_0 + \beta_1 HealthChoices_k \times POST_t + \beta_2 YEAR_t + \beta_3 COUNTY_k + \beta_4 TREND_{kt} + \alpha_1 P_{ijkt} + \alpha_2 H_{jt} + MFE_i + \epsilon_{ijkt}, \quad (1.1)$$

where Outcome is one of the three measures of outcomes for patient i, who lives in county k and delivers in hospital j in year$t$, $YEAR$ and $COUNTY$ are fixed effects for the year of delivery and for the county where the patient lives, $TREND$ is a vector of four linear time trends, one for each group of counties in each HealthChoices zone and one for the control group counties, P is a vector of time-varying characteristics of the patient, H is a vector of characteristics of the hospital and the hospital market where delivery takes place, and MFE is mother fixed effects.[11]

[11] Note that all time-invariant individual characteristics will be excluded when mother fixed effects are included. $COUNTY_k$ is included because some mothers move between counties. We will consider mother's mobility in our robustness check.

The key variable is $HealthChoices \times POST$. The variable $HealthChoices$ equals one if the patient lives in one of the counties where mandatory managed care was adopted during the sample period, and zero if they live in one of the counties in a control group. The variable $POST$ equals one if the patient's delivery occurs after HealthChoices has been implemented in her county, and zero if the delivery occurs before that time.[12] Thus, the interacted variable $HealthChoices{*}POST$ identifies patients in HealthChoices counties after HealthChoices has been implemented, and its coefficient β_1 is an estimate of the change in outcomes for Medicaid mothers in HealthChoices counties, compared to their previous pregnancy, relative to changes in outcomes over pregnancies for Medicaid mothers in either the Voluntary counties or the FFS counties (depending on the control group used).

The DD specification that we estimate for the commercially-insured mothers is identical to equation (1).

We estimate the following DDD specification using the full sample of both Medicaid and commercially-insured births:

$$\begin{aligned} Outcome_{ijkt} = \beta_0 + \beta_1 Medicaid_{it} \times HealthChoices_k \times POST_t + \beta_2 YEAR_t + \beta_3 COUNTY_k \\ + \beta_4 Medicaid_{it} + \beta_5 YEAR_t \times COUNTY_k + \beta_6 YEAR_t \times Medicaid_{it} \\ + \beta_7 COUNTY_k \times Medicaid_{it} + \beta_8 TREND_{ikt} + \alpha_1 P_{ijkt} + \alpha_2 H_{jt} + MFE_i + \epsilon_{ijkt}, \quad (1.2) \end{aligned}$$

where $Outcome$ is one of the three measures of outcomes for patient i, who lives in county k and delivers in hospital j in year t, $Medicaid$ is a dummy variable that equals one if the patient is covered by Medicaid, and zero if they use commercial insurance, $TREND$ is a vector of eight insurance-zone specific linear time trends, and the other variables are as defined above.[13] Thus, this specification includes fixed effect controls for time-varying changes that affect outcomes of all deliveries, whether covered by Medicaid or commercial insurance (β_2), for time-invariant characteristics of counties that affect all deliveries (β_3), and time-invariant characteristics of all

[12] The calendar years for which $POST$ equals one are 1997 for the Southeast zone, 1999 for the Southwest zone, and 2002 for Lehigh/Capital region. Although the switch to HealthChoices actually occurred in 2001 in the Lehigh/Capital region, it occurred late enough in the year so that we use 2002 as the first year of implementation.

[13] We treat Medicaid as a time-variant variable in our main specification, but will alternate this specification in our robustness check.

Medicaid-insured mothers that affect their outcomes (β_4. A second set of interacted variables control for changes in outcomes over time in different counties for all deliveries (β_5), changes over time that affect the outcomes of all Medicaid-insured mothers (β_6), and finally time-invariant characteristics of Medicaid-insured mothers in different counties that affect their outcomes (β_7).

The key variable is $Medicaid * HealthChoices * POST$ because it identifies deliveries by Medicaid mothers in HealthChoices counties after HealthChoices has been implemented. Its coefficient, β_1, is thus an estimate of the change in outcomes for Medicaid mothers in HealthChoices counties, compared to their previous pregnancy, relative to changes in outcomes over pregnancies for commercially-insured mothers in the same counties, relative to the same outcome difference over pregnancies for Medicaid and commercially-insured mothers in the control counties.

1.5 Data, Sample, and Description of Variables

1.5.1 Data and Sample

The data for this paper are from the Pennsylvania Health Care Cost Containment Council (PHC4) inpatient data file for the years 1995-2004, a period that encompasses the implementation of the HealthChoices program in the three zones.[14] Each PHC4 inpatient record includes information about a patient's characteristics, zip code of residence, health status, procedure codes, and insurance, and also contains identifiers that allow us to link the individual's inpatient records over time. The PHC4 records also include a hospital identification number that we used to link the individual patient records to information about the hospital where the patient's delivery took place, drawn from the American Hospital Association Annual Survey of Hospitals.

We selected all cases of vaginal birth or Cesarean section (C-section) in the data for 1995-2004 based on ICD-9 procedure and DRG codes, a sample of 1,377,695 observations. We dropped patients that were covered neither by Medicaid nor commercial insurance, leaving 1,273,457 observations, and observations missing race, admission or zip code information, or that appeared too soon following a previous delivery (i.e., within the following quarter), leaving 1,267,637 ob-

[14]We end our sample in 2004 because in 2005 the DPW began implementing a primary care case management (PCCM) program in the Voluntary and the FFS counties (Lave, 2008). In a PCCM program, the state pays primary care physicians a flat fee per patient for serving as gate keepers to more specialized care.

servations. We then eliminated records where no live birth resulted for reasons unrelated to the patient's health care (ectopic or molar pregnancies, pregnancies with abortive outcomes, and pregnancies with intrauterine death), records for patients that were not Pennsylvania residents, and records that were missing hospital characteristics, leaving 1,224,667 observations. Finally, we eliminated all mothers who had only one pregnancy during 1995-2004, leaving a sample of 669,209 deliveries, 202,947 of which were covered by Medicaid and 466,262 of which were covered by commercial insurance, and all of which were deliveries by mothers who had at least two pregnancies during the sample period.

1.5.2 Outcomes

We estimate the impact of the program on three separate outcomes measures: the incidence of preventable complications as a measure of quality, total hospital charges as a measure of cost, and whether the patient had a C-section as a measure of utilization, all three based on data derived from the PHC4 inpatient database.

"Preventable complications" are complications that in many cases may be avoided with better care (Currie & MacLeod, 2008).[15] Managed care, with its emphasis on preventive care, specifically prenatal care in the case of pregnancy, may therefore result in improved outcomes by reducing the occurrence of preventable complications. The variable $OUTCOME$ equals one if a preventable complication requiring hospitalization occurred during the patient's pregnancy (that is, during the nine months preceding delivery) or during her delivery, and zero otherwise.

Our second outcome variable is the log of total charges, deflated to 1995 dollars using the CPI-U. Each inpatient record in the PHC4 database includes the total charges for that stay in the hospital.[16] We use the individual patient identifier to find all hospital admissions experienced by the patient in the nine months preceding their delivery for a pregnancy-related problem[17], and

[15]Preventable complications are: maternal fever, excessive bleeding, maternal seizure, precipitous labor, prolonged labor, dysfunction labor, anesthetic complications, fetal distress, and rupture of the uterus during labor, and choriamnionitis (an inflammation of fetal membranes most often occurring with prolonged labor).

[16]These charges are list prices, not actual costs nor actual reimbursements received by a hospital. Researchers frequently adjust list prices using the cost-to-charge ratios calculated for each hospital by CMS from Medicare data. However, there is no cost-to-charge ratio information available for Medicaid patients. We nevertheless tried adjusting the total charge variable using the Medicare cost-to-charge ratio for the hospital where the charges were incurred, but found it did not alter our results.

[17]Pregnancy-related admissions are identified by ICD9 Diagnosis Codes 640-679, excluding 656.4 (intra-uterine death), and ICD9 Obstetric Procedure Primary and Sub Codes 72, 73, 74.0, 74.1, 74.2, and 74.4.

sum the charges for all these admissions, plus the charges during the admission for delivery, to get our measure of total hospital charges for each pregnancy case[18].

Our third outcome variable equals one if the delivery is a C-section, and zero otherwise. Fees for C-sections are higher than for vaginal deliveries, and physicians respond by providing more of them (Gruber & Owings, 1996; Gruber, Kim & Mayzlin, 1999). Managed care plans, on the other hand, are paid a fixed premium by the state to provide care to each enrolled client, and thus have an incentive to control the utilization of unnecessary procedures. Typically HMOs do not reimburse physicians on a capitated basis for C-sections. However, they may use performance-based pay, for example a bonus for achieving some predefined level patient care quality that gives physicians an incentive to reduce the rate of C-sections (Armour et al, 2001). We therefore expect that the rate of C-section will be lower after the implementation of HealthChoices.

1.5.3 Patient Characteristics

Although using mother fixed effects controls for time-invariant characteristics of individuals that might affect their outcomes, we use the PHC4 inpatient records to create several variables to control for relevant characteristics or circumstances of a mother that might change from one pregnancy to the next. These, represented by P_{ijkt} in equations (1) and (2), include the patient's age, whether the patient was referred by a physician, clinic, or HMO, whether the patient was transferred from another hospital, and whether the delivery admission was an emergency or weekend admission. Using the ICD-9 diagnosis and procedure codes in the PHC4 inpatient record, we control for changes in health status by creating dummy variables that indicate whether a patient had at least one non-preventable complication, defined as birth complications that are unlikely to have been caused by a doctor at the time of delivery [19], or at least one pre-existing condition, defined as conditions that are not directly caused by pregnancy but that may affect

[18]Our total charges variable does not include charges for visits to clinics or primary care physicians, nor for visits to the hospital triage department, since the data we use does not include information on primary or outpatient care.

[19]Non-preventable complications are: a breech delivery, cephalopelvic disproportion (the baby's head is too big for the mother's pelvis), cord prolapse (the umbilical cord is delivered prior to the baby), placenta previa (the placenta is implanted too close to the cervical opening), abruption placenta (a premature separation of the placenta from the uterus), and a premature rupture of membranes.

the delivery outcomes [20], since either may affect treatment choices and total charges. Finally, we also include dummy variables to control for circumstances that might affect the choice of delivery procedure or the total change: whether the patient previously had a C-section and whether the delivery is a multiple birth. In estimations where the outcome is total cost or preventable complications, we also include dummies indicating whether the delivery is vaginal with instruments, or a C-section.

1.5.4 Hospital Characteristics

As noted above, each PHC4 inpatient record includes a numerical identifier for the hospital where the admission occurred, and we were able to attach hospital names and addresses to these identifiers. We then used the hospital name and zip code to link the PHC4 inpatient data to the American Hospital Association Annual Survey of Hospitals database and thereby add information about the hospital to the admission record. We use this information to control for the effects of hospital characteristics and hospital markets, represented by H_{jt} in equations (1) and (2), on patient outcomes.

We include dummy variables indicating whether the hospital has 200 to 400 beds (medium size), or more than 400 beds (large size), with the omitted group as hospitals with fewer than 200 beds; whether the hospital is not-for-profit (NFP), with the omitted group for-profit hospitals; whether the hospital is a member of the Council of Teaching Hospital of the Association of American Medical College (teaching); and whether the hospital has a level two or level three obstetrical unit, with the omitted group being hospitals with a level one obstetrical unit.

We also include a variable that measures the percentage of a hospital's patients that are covered by Medicaid in the year of delivery, as well as a Herfindahl-Hirschman index (HHI) for each hospital, because competition in hospital markets may influence the impact of managed care on patient outcomes (Gowrisankaran and Town, 2003). We construct hospital HHIs using the "variable radius" method (Gresenz et al., 2004), where a hospital's market is determined by the area from which it draws 75% of all of its patients.

[20] Pre-existing conditions are: anemia, herpes, eclampsia, incompetent cervix, Rhesus (anti-D) iso-immunization, uterine bleeding, hypertensive disorder, oligohydramnios, abnormality of vagina (congenItal or acquired), diabetes mellitus/abnormal glucose tolerance, and habitual aborter.

Table 1.4 presents descriptive statistics for the variables used in the estimations, for the entire sample and for the Medicaid and commercially insured subsamples, all by Medicaid policy areas.

1.6 Results

The DD and DDD estimation results are shown on Table 1.5: Panel A shows the DD results for Medicaid patients, Panel B shows DD results for commercially-insured patients, and Panel C shows the DDD results. Columns (1), (3), and (5) show the impact of HealthChoices on each outcome with the Voluntary counties as the control group the lower bound estimate and columns (2), (4), and (6) show the impact of HealthChoices on each outcome with the FFS counties as the control group the upper bound estimate. All estimations include the patient and hospital characteristics described in section 5, as well as year, county, and mother fixed effects, and a separate time trend for each HealthChoices zone and for the control group. Standard errors are clustered at the county level.

The results in Panel A suggest that HealthChoices reduced the probability of preventable complications relative to both of the two control groups. As anticipated, the estimated effect is smaller when the control group is the Voluntary counties as opposed to the FFS counties, but the difference is small. The estimated coefficients for the log of total charges and for C-section were negative but never significant. (Note that the county-group-specific trends are all positive and significant, which is the opposite impact on outcomes as the effect of the managed care policy.) Inspection of Panel B, on the other hand, shows no significant changes in the outcomes in these areas for commercially-insured patients after HealthChoices was adopted. The findings suggest that the commercially-insured patients were unaffected by the policy change experienced by Medicaid patients in the treatment counties, and that changes in their outcomes may be used in the DDD specification without concern.

Panel C shows that adding controls for regional variation affecting all pregnant patients does not change our results. The probability of preventable complications occurring in a pregnancy is 1.1 percentage points lower after HealthChoices begins, which, relative to the mean for Medicaid patients in the treatment counties in 1996, implies that the probability decreased by 10.5-10.7%. We continue to find that HealthChoices changed neither the log of total charge nor the probability

of a patient receiving a C-section,

1.6.1 Robustness Checks

The stability of our estimates under the DD and the DDD specifications, as well as relative to the two alternative control groups, suggests our results are robust. However, we also report a more formal set of robustness checks on the different panels of Table 1.6. In all cases we report results for the DDD specification only.

Clustering

We start by trying an alternative clustering rule. Instead of clustering standard errors at the county level, we cluster them at the zone-year level. The resulting DDD estimations are shown in Panel A of Table 1.6. Our results for preventable complications remain unchanged, but we also see some evidence that HealthChoices may also reduce the log of total charge.

Singleton Births

The literature studying the effect of managed care on low birth weight examines samples of 'singleton' births, because multiple births (e.g., twins) usually result in lower birth weight as a matter of course. Cases of multiple gestation are also more likely to be associated with complications, C-sections, and higher charges. We therefore re-estimated our DDD specification using only singleton births. Panel B of Table 1.6 indicates that restricting our sample in this way does not change our findings regarding the direction and significance of the effect of HealthChoices on preventable complications.

Mother Mobility

A Medicaid-insured mother may have managed care insurance either because it becomes mandated in the county where she lives, the assumption of our estimation strategy, or because she moves from one of the control counties to one of the HealthChoices counties. Because patients may move among policy areas, the estimated effect of HealthChoices reflects the combined effect of the policy change and of mothers' movements between the treatment and control groups. The

numbers of such patients were not large[21], but we tested whether they might be influencing our results by assigning to all such mothers the Medicaid reimbursement system that they would have experienced had they remained in the county where they lived when we first observed them. Although altering the data for mothers that move introduces measurement error in the county variables, it excludes from the estimates the effects of self-selection arising from mothers moving to a county with a different Medicaid reimbursement system. The results, presented in Panel C of Table 1.6, show that after making this adjustment we continue to find that HealthChoices is associated with a lower incidence of preventable complications.

Medicaid Enrollment

Some of the mothers in our sample were covered by Medicaid for only one of their deliveries[22]. We tested whether our results were driven by movements of mothers into and out of the Medicaid system by assigning each mother the insurance status that she had at her first delivery. The results of re-estimating the DDD are shown in Panel D of Table 1.6. The signs and significance of our variables are unchanged.

1.7 Effects of Patient Health Status and Adjustment Lags

1.7.1 Patient Health Status

We test whether the impact of HealthChoices varies with the health status of the mother: managed care, with its emphasis on preventive care, may be particularly effective for Medicaid mothers in poorer health. We divide the patients into high and low risk categories depending on whether the mother has a pre-existing condition or a non-preventable complication, and re-estimate the DDD specification, interacting the $Medicaid \times HealthChoices \times POST$ variable with HI, a dummy variable identifying high risk patients, and with LO, a dummy variable identifying low risk patients.

[21] During the sample period, 7995 of the 238,795 or 3.3% of the Medicaid deliveries represent deliveries that occurred to a mother who had moved from a county in a treatment group to a county in the control group, or vice versa, while 15,618 out of 534,413 or 2.9% of the commercially insured births were to mothers who had moved between county groups.

[22] During the sample period, 28,742 or 5.4% of the 534,413 commercially-insured births were by mothers whose first delivery in the sample period was covered by Medicaid, and 33,785 or 14.1% of the 238,795 Medicaid deliveries were by mothers whose first delivery was commercially-insured.

Our results are shown on Table 1.7. We continue to find evidence that preventable complications are significantly lower in HealthChoices areas. Inspection of the magnitudes shows that for high risk patients, preventable complications are reduced by 2.1-2.2 percentage points, while the impact on low risk patients, also negative although only significant relative the to the FFS control group, is .6-.7 percentage points. Relative to the mean value of preventable complications for each group in 1996, these coefficients imply that the probability of preventable complications fell by 15.9-16.7% for high risk patients, and by 8-9% for low risk patients.

Perhaps the most interesting results are those for C-sections. Our DDD estimations on Table 1.5 suggested that managed care had no separate impact on the rate of C-sections. However, the results on Table 1.7 show that the HealthChoices program affects the probability of C-section differently for the two risk categories: mothers in worse health, those with pre-existing conditions, were 1-1.3 percentage points more likely to have a C-section, which, relative to the mean in 1996, indicates an increase of 3.6-4.7%. The coefficients for the low-risk mothers, on the other hand, were negative but not significant. Our results thus show that the impact of HealthChoices was to increase the intensity of care for sicker patients, while having no effect on, or possibly reducing, the intensity of care for healthier patients. .

1.7.2 Adjustment Lags

In our estimations we assume that the impact of the managed care policy begins immediately with its implementation. It may be, however, that the impact is delayed, as patients and providers adjust to the new system, particularly since adjustment may involve patients choosing new doctors when they choose their HMO, and because patients already pregnant at the time of the implementation could continue to see their OB-GYN for that pregnancy. We test for lags in the impact of managed care adoption in the years subsequent to adoption by replacing the interacted policy variable, $Medicaid \times HealthChoices \times POST$, with three other variables: $Medicaid \times HealthChoices \times POST1$, $Medicaid \times HealthChoices \times POST2$, and $Medicaid \times HealthChoices \times POST3+$. $POST1$ and $POST2$ are dummy variables that equal one in the first or second year after adoption of HealthChoices in the patient's particular county. $POST3+$ equals one if HealthChoices was adopted three or more years ago in a county. (We do

not investigate longer lags because three years is the longest lag we can accommodate given the later switch to managed care in the Lehigh/Capital zone and the final year, 2004, of our data set.)

The results on Table 1.8 continue to show that the switch to HealthChoices is associated with a reduction in preventable complications, but suggest that the initial effect may more than double in magnitude within three years, reducing the probability of preventable complications by 23.8-25.2%. In addition, there is some evidence that the switch to HealthChoices may eventually reduce costs: the estimated coefficients on the log of total charge show that total charges are 19.7% lower (calculated as exp(0.18)-1) after three years of the new program. Given that the mean of total charges for Medicaid patients in treatment counties in 1996 (the year prior to the adoption of HealthChoices anywhere in Pennsylvania) was $5,542, this percentage translates into a $1,092 reduction per case.

1.8 Conclusion

The stated goals for switching from FFS to managed care in Pennsylvania included increasing the quality of care while controlling the cost. We found that the switch to managed care resulted in fewer patients with preventable complications, and, eventually, in lower charges. We also found that the reduction in preventable complications was larger among high risk patients, who also were more likely to receive C-sections. Thus, the HealthChoices program may have achieved lower costs and better health outcomes by providing better preventive care, particularly to higher risk patients. However, the full benefits from switching to managed care may take several years to develop, possibly because some pregnant women delay their switch until after their pregnancy is over. We reach these conclusions having employed a DDD specification, as well as mother fixed effects, so as to reduce the potential biases arising from uncontrolled variation and heterogeneity; the results were robust to several changes in the specification.

Our results support a more positive view of managed care than such studies as Aizer et al. (2007). However, differences in results for different states may reflect the real impact of different institutional arrangements (Marton, et al., 2012). Pennsylvania had the advantage of some experimental experience with mandatory managed care, a well-regarded management structure, and a

strong advocacy group for Medicaid patients (Johnston, 2003). Further, the state's requirement that at least three managed care companies be available for patients to choose among provided for at least some level of competition among the firms. More research on the effect that these or other differences may have had on the impact of switching Medicaid patients to managed care in different states would help us determine whether a well-designed Medicaid managed care program can achieve cost control and better care for its enrollees.

Figure 1.1: HealthChoices, Voluntary Managed Care and Non-Managed Care Counties, Pennsylvania

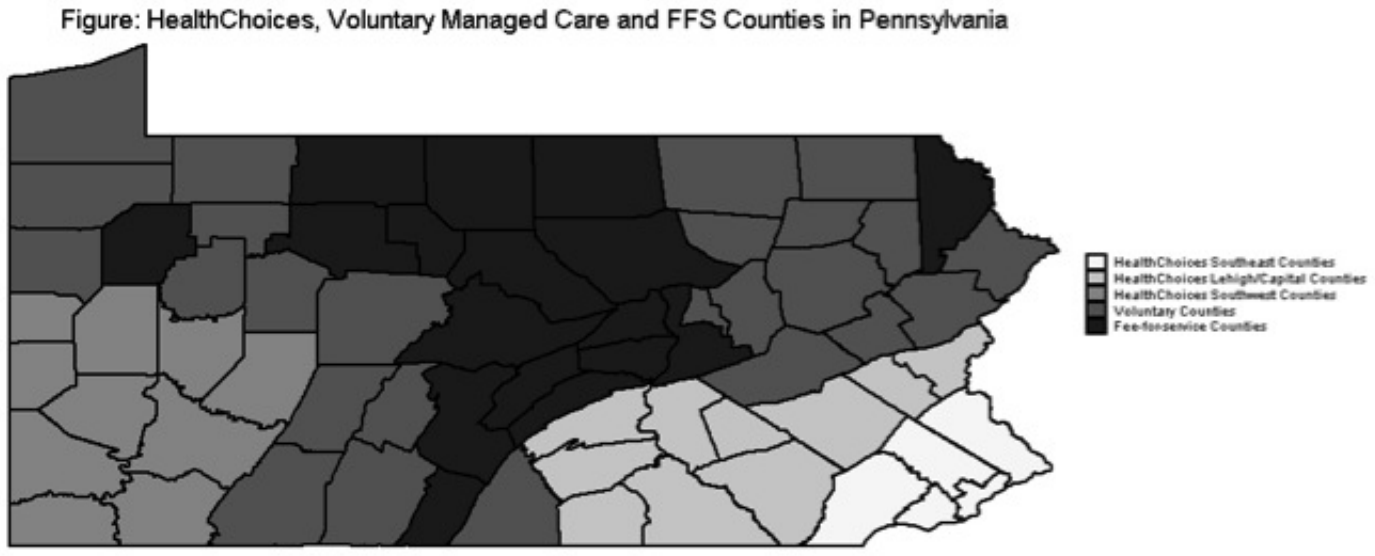


Table 1.1: Implementation dates and different zones in Pennsylvania

Zone	Date	Counties
HealthChoices Counties	1997	**Southeast Counties (Zone 1)** Bucks, Chester, Delaware, Montgomery and Philadelphia counties
	1999	**Southwest Counties (Zone 2)** Allegheny, Armstrong, Beaver, Butler, Fayette, Green, Indiana, Lawrence, Washington and Westmoreland
	2002	**Lehigh/Capital Counties (Zone 3)** Adams, Berks, Cumberland, Dauphin, Lancaster, Lebanon, Lehigh, Northampton, Perry and York
Voluntary Managed Care Counties	–	Bedford, Blair, Bradford, Cambria, Carbon, Clarion, Clearfield, Columbia, Crawford, Erie, Forest, Franklin, Jefferson, Lackawanna, Luzerne, Mercer, Monroe, Montour,Northumberland, Pike, Schuylkill, Somerset, Sullivan, Susquehanna, Warren, and Wyoming
None-Managed Care Counties	–	Cameron, Centre, Clinton, Elk, Fulton, Huntingdon, Juniata, Lycoming, Mckean, Mifflin, Potter, Snyder, Tioga, Union, Venango, and Wayne

[1] HealthChoices was implemented late in 2001 for zone 3, so we use 2002 as the first year of implementation for this zone.

Table 1.2: Summary Statistics for County Characteristics, by Coverage Type

Variables	All Counties	HealthChoices Counties	Voluntary Counties	FFS Counties
Poverty ratio	0.11	0.10	0.11	0.11
Population density	2225.95	2857.92	232.17	98.52
Median income	41122.87	43205.29	34515.74	34229.30
Unemployment rate	5.91	5.57	7.12	6.65

[1] Population density (persons/squ mile) is linearly imputed from the 1990 and 2000 census data for each county; median income ($) and poverty ratio(%) at the county and year level is from the small area income and poverty estimates from Census website. Unemployment rate(%) is from Bureau of Labor Statistics, Department of Labor website.

Table 1.3: Percentage of Pregnant Medicaid Patients Enrolled in Managed Care, by Year

	1995	1996	1997	1998	1999	2000	2001	2002	2003	2004
HealthChoices Counties										
Southeast (Zone 1)	0.12	0.27	0.66	0.76	0.83	0.89	0.9	0.9	0.91	0.93
Southwest (Zone 2)	0.08	0.15	0.15	0.26	0.71	0.95	0.94	0.95	0.95	0.95
Lehigh/Capital (Zone 3)	0.01	0.07	0.22	0.22	0.31	0.31	0.3	0.78	0.9	0.91
Voluntary Counties	0.00	0.01	0.03	0.04	0.21	0.31	0.32	0.33	0.37	0.38
FFS Counties	0.00	0.00	0.00	0.00	0.01	0.04	0.04	0.06	0.06	0.07

[1] Calculated from PHC4 inpatient database.

Table 1.4: Summary Statistics for Sample

		Medicaid Patients,by Coverage Type				Commercially Insured,by areas of Medicaid Coverage Type			
Variable	All births	All	HealthChoices	Voluntary	FFS	All	HealthChoices	Voluntary	FFS
Outcomes Variables									
Preventable complications	0.1	0.09	0.1	0.08	0.1	0.1	0.1	0.09	0.11
Total Charge	6385.77 [6845.47]	7067.83 [8740.03]	8308.43 [9760.53]	4051.58 [3952.18]	3633.35 [3843.16]	6088.9 [5806.69]	6709.62 [6186.35]	3950.8 [3301.75]	3553.79 [3290.50]
C-section	0.2	0.18	0.17	0.2	0.21	0.21	0.21	0.23	0.24
Patient Characteristics									
Emergency Admission	0.37	0.45	0.52	0.28	0.13	0.34	0.37	0.24	0.15
Weekend Admission	0.21	0.21	0.22	0.20	0.21	0.20	0.20	0.19	0.19
Vaginal Delivery with Instrument	0.73	0.76	0.77	0.73	0.76	0.72	0.72	0.69	0.73
Pre-existing conditionc	0.24	0.29	0.31	0.24	0.24	0.23	0.23	0.22	0.20
Non-preventable complicationsd	0.11	0.10	0.10	0.10	0.11	0.12	0.12	0.11	0.12
Previous C-section	0.13	0.13	0.12	0.13	0.14	0.14	0.14	0.14	0.15
Multiple births	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01
Patient referred by physician, clinic or HMO	0.97	0.94	0.93	0.96	0.97	0.98	0.98	0.97	0.98
Patient referred from another hospital	0.00	0.01	0.01	0.01	0.01	0.00	0.00	0.00	0.01
Age									
Between 16 & 20	0.13	0.30	0.30	0.30	0.32	0.06	0.06	0.07	0.07
Between 21 and 25	0.24	0.40	0.39	0.42	0.43	0.17	0.16	0.22	0.24
Between 26 and 30	0.30	0.18	0.18	0.17	0.16	0.35	0.34	0.37	0.38
Between 31 and 35	0.24	0.08	0.08	0.07	0.06	0.31	0.32	0.26	0.24
Between 36 and 40	0.08	0.02	0.03	0.02	0.02	0.10	0.11	0.08	0.07
Between 41 and 45	0.01	0.00	0.00	0.00	0.00	0.01	0.01	0.01	0.01
Between 46 and 50	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Age over 50	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00

Table 1.4: Summary Statistics for Sample(continued)

		Medicaid Patients,by Coverage Type				Commercially Insured,by areas of Medicaid Coverage Type			
Variable	All births	All	HealthChoices	Voluntary	FFS	All	HealthChoices	Voluntary	FFS
Hospital Characteristics									
HHI	0.50	0.53	0.47	0.64	0.86	0.48	0.43	0.60	0.84
	[0.29]	0.29	0.28	0.26	0.24	0.29	0.28	0.24	0.25
Teaching	0.31	0.35	0.46	0.06	0.08	0.29	0.34	0.09	0.09
NFP	0.99	0.99	0.98	1.00	0.99	0.99	0.99	0.99	1.00
Medicaid Share	0.12	0.16	0.17	0.14	0.13	0.11	0.10	0.13	0.12
	0.07	0.09	0.10	0.04	0.04	0.06	0.06	0.04	0.04
Medium Size	0.36	0.32	0.30	0.42	0.22	0.37	0.36	0.43	0.26
Large Size	0.39	0.42	0.52	0.16	0.19	0.38	0.43	0.19	0.21
Obstetric Level 2	0.31	0.26	0.23	0.37	0.24	0.33	0.32	0.37	0.28
Obstetric Level 3	0.36	0.33	0.43	0.10	0.10	0.37	0.43	0.15	0.13
Total Observations	669,209	202,947	145,120	44,401	13,426	466,262	364,994	75,984	25,284

[1] Standard deviations for continuous variables are reported in brackets.

[2] Preventable complications are: maternal fever, excessive bleeding, maternal seizure, precipitous labor, prolonged labor, dysfunction labor, anesthetic complications, fetal distress, and rupture of the uterus during labor, and choriamnionitis (an inflammation of fetal membranes most often occurring with prolonged labor).

[3] Pre-existing conditions are: anemia, herpes, eclampsia, incompetent cervix, Rhesus (anti-D) iso-immunization, uterine bleeding, hypertensive disorder, oligohydramnios, abnormality of vagina (congenItal or acquired), diabetes mellitus/abnormal glucose tolerance, and habitual aborter.

[4] Non-preventable complications are: a breech delivery, cephalopelvic disproportion (the babys head is too big for the mothers pelvis), cord prolapse (the umbilical cord is delivered prior to the baby), placenta previa (the placenta is implanted too close to the cervical opening), abruption placenta (a premature separation of the placenta from the uterus), and a premature rupture of membranes.

Table 1.5: DD and DDD Results

	Preventable Complications		Log of Total Charge		C-section	
Panel A: DD, Medicaid Only						
HealthChoices*POST	-0.0105**	-0.0107**	-0.0458	-0.0439	-0.0048	-0.0040
	[0.005]	[0.005]	[0.052]	[0.048]	[0.003]	[0.003]
Trend/Southeast	0.0084***	0.0089***	0.1070***	0.1070***	0.0150***	0.0148***
	[0.001]	[0.001]	[0.006]	[0.005]	[0.001]	[0.001]
Trend/Southwest	0.0055***	0.0060***	0.0612***	0.0595***	0.0178***	0.0176***
	[0.002]	[0.002]	[0.008]	[0.008]	[0.001]	[0.001]
Trend/Lehigh&Capital	0.0052***	0.0056***	0.0167**	0.0144*	0.0149***	0.0148***
	[0.001]	[0.001]	[0.007]	[0.008]	[0.001]	[0.001]
Trend/Voluntary	0.0035***		0.0225***		0.0189***	
	[0.001]		[0.006]		[0.001]	
Trend/FFS		0.0008		0.0224**		0.0194***
		[0.002]		[0.009]		[0.002]
Obs: No. of Deliveries	189,521	158,546	189,521	158,546	189,521	158,546
R-Squared	0.011	0.011	0.317	0.335	0.149	0.149
Number of Mothers	105,296	89,047	105,296	89,047	105,296	89,047
Control Group	VOL	FFS	VOL	FFS	VOL	FFS
Panel B: DD, Commercially-insured only						
HealthChoices*POST	0.0015	0.0010	-0.0045	-0.0046	0.0015	0.0008
	[0.003]	[0.003]	[0.023]	[0.023]	[0.002]	[0.002]
Trend/Southeast	0.0018*	0.0025**	0.0693***	0.0690***	0.0165***	0.0169***
	[0.001]	[0.001]	[0.004]	[0.004]	[0.001]	[0.001]
Trend/Southwest	-0.0020	-0.0013	0.0546***	0.0537***	0.0152***	0.0156***
	[0.001]	[0.001]	[0.006]	[0.006]	[0.001]	[0.001]
Trend/Lehigh&Capital	-0.0008	-0.0002	0.0095**	0.0085*	0.0120***	0.0122***
	[0.001]	[0.001]	[0.004]	[0.005]	[0.001]	[0.001]
Trend/Voluntary	0.0010		0.0179***		0.0174***	
	[0.001]		[0.005]		[0.001]	
Trend/FFS		-0.0030		0.0177***		0.0191***
		[0.002]		[0.006]		[0.002]
Obs: # of Deliveries	440,978	390,278	440,978	390,278	440,978	390,278
R-Squared	0.030	0.030	0.264	0.265	0.159	0.159
Number of Mothers	225,828	200,372	225,828	200,372	225,828	200,372
Control Group	VOL	FFS	VOL	FFS	VOL	FFS

Table 1.5: DD and DDD Results (continued)

	Preventable Complications		Log of Total Charge		C-section	
Panel C DDD, Medicaid & Commercially-insured						
Medicaid*HealthChoices*POST	-0.0110**	-0.0123***	-0.0898	-0.0893	-0.0010	-0.0008
	[0.005]	[0.004]	[0.061]	[0.061]	[0.004]	[0.004]
Medicaid*Trend/Southeast	0.0065***	0.0066***	0.0451***	0.0447***	0.0012	0.0012
	[0.001]	[0.001]	[0.009]	[0.009]	[0.001]	[0.001]
Medicaid*Trend/Southwest	0.0043***	0.0046***	0.0204***	0.0193***	0.0018**	0.0016**
	[0.001]	[0.001]	[0.005]	[0.005]	[0.001]	[0.001]
Medicaid*Trend/Lehigh&Capital	0.0046***	0.0047***	0.0006	-0.0008	0.0006	0.0005
	[0.001]	[0.001]	[0.008]	[0.008]	[0.001]	[0.001]
Medicaid*Trend/Voluntary	0.0019**		0.0064		0.0017**	
	[0.001]		[0.004]		[0.001]	
Medicaid*Trend/FFS		0.0033*		0.0059		0.0005
		[0.002]		[0.005]		[0.001]
Obs: # of Deliveries	630,499	548,824	630,499	548,824	630,499	548,824
R-Squared	0.023	0.023	0.275	0.279	0.153	0.153
Number of Mothers	283,618	249,469	283,618	249,469	283,618	249,469
Control Group	VOL	FFS	VOL	FFS	VOL	FFS

[1] Standard errors, reported in brackets, are clustered at the county level in Panel A and B. All regressions also include the patient and hospital characteristics described in section 5, as well as year and county and mother fixed effects; regressions in the first four columns also include a variable indicating whether the delivery was a C-section.

[2] c Standard errors, reported in brackets, are clustered at the county level in Panel C. All regressions also include the patient and hospital characteristics described in section 5, as well as year, county, and Medicaid fixed effects, the interactions of these fixed effects indicated in specification (2), and mother fixed effects.

[3] *** Significant at the 1 percent level (two-tailed test); ** Significant at the 5 percent level (two-tailed test); * Significant at the 10 percent level (two-tailed test).

Table 1.6: Robustness Checks

	Preventable Complications		Log of Total Charge		C-section	
Panel A: Cluster by Zone-Year						
Medicaid* HealthChoices* POST	-0.0110***	-0.0123***	-0.0898***	-0.0893***	-0.0010	-0.0008
	[0.003]	[0.003]	[0.029]	[0.030]	[0.003]	[0.003]
Observations: # of Deliveries	630,499	548,824	630,499	548,824	630,499	548,824
R-squared	0.023	0.023	0.275	0.279	0.153	0.153
Number of Mothers	283,618	249,469	283,618	249,469	283,618	249,469
Control Group	VOL	FFS	VOL	FFS	VOL	FFS
Panel B: Singleton Births						
Medicaid* HealthChoices* POST	-0.0124**	-0.0137***	-0.0910	-0.0906	0.0002	0.0004
	[0.005]	[0.004]	[0.061]	[0.061]	[0.003]	[0.004]
Observations: # of Deliveries	621,862	541,118	621,862	541,118	621,862	541,118
R-squared	0.024	0.023	0.260	0.265	0.137	0.137
Number of Mothers	283,411	249,220	283,411	249,220	283,411	249,220
Control Group	VOL	FFS	VOL	FFS	VOL	FFS
Panel C: Mobility						
Medicaid* HealthChoices* POST	-0.0078*	-0.0091**	-0.1114*	-0.1044*	0.0017	0.0020
	[0.004]	[0.004]	[0.057]	[0.060]	[0.003]	[0.003]
Observations: # of Deliveries	631,145	550,126	631,145	550,126	631,145	550,126
R-squared	0.023	0.023	0.259	0.264	0.152	0.152
Number of Mothers	280,773	245,158	280,773	245,158	280,773	245,158
Control Group	VOL	FFS	VOL	FFS	VOL	FFS
Panel D: Medicaid Insurance Status						
Medicaid* HealthChoices* POST	-0.0071**	-0.0076**	-0.0554	-0.0522	-0.0015	-0.0013
	[0.003]	[0.003]	[0.047]	[0.047]	[0.003]	[0.003]
Observations: # of Deliveries	630,499	548,824	630,499	548,824	630,499	548,824
R-squared	0.023	0.023	0.276	0.280	0.153	0.017
Number of Mothers	283,618	249,469	283,618	249,469	283,618	249,469
Control Group	VOL	FFS	VOL	FFS	VOL	FFS

[1] Standard errors, reported in brackets, are clustered at the county level except for Panel A, where they are clustered at the zone-year level. All regressions also include the patient and hospital characteristics described in section 5, year, county, and Medicaid fixed effects, the interactions of these fixed effects indicated in specification (2), and mother fixed effects. All estimations also include a separate time trend for each HealthChoices zone and for the control group; regressions in the first four columns also include a variable indicating whether the delivery was a C-section.

[2] *** Significant at the 1 percent level (two-tailed test); ** Significant at the 5 percent level (two-tailed test); * Significant at the 10 percent level (two-tailed test).

Table 1.7: DDD Estimations by Patient Health Status

	Preventable Complications		Log of Total Charge		C-section	
Medicaid* HealthChoices	-0.0208**	-0.0219**	-0.0918	-0.0899	0.0098*	0.0127**
POST* HI	[0.009]	[0.009]	[0.073]	[0.073]	[0.005]	[0.005]
Medicaid* HealthChoices	-0.0058	-0.0072**	-0.0888	-0.0891	-0.0068	-0.0080
POST* LO	[0.004]	[0.003]	[0.055]	[0.055]	[0.005]	[0.005]
Observations: # of Deliveries	630,499	548,824	630,499	548,824	630,499	548,824
R-squared	0.023	0.023	0.275	0.279	0.153	0.153
Number of Mothers	283,618	249,469	283,618	249,469	283,618	249,469
Control Group	VOL	FFS	VOL	FFS	VOL	FFS

[1] Standard errors, reported in brackets, are clustered at the county level. All regressions also include the patient and hospital characteristics described in section 5, year, county, and Medicaid fixed effects, the interactions of these fixed effects indicated in specification (2), and mother fixed effects. All estimations also include a separate time trend for each HealthChoices zone and for the control group; regressions in the first four columns also include a variable indicating whether the delivery was a C-section.

[2] *** Significant at the 1 percent level (two-tailed test); ** Significant at the 5 percent level (two-tailed test); * Significant at the 10 percent level (two-tailed test).

Table 1.8: DDD Estimations with Lags

	Preventable Complications		Log of Total Charge		C-section	
Medicaid* HealthChoices* POST1	-0.0086*	-0.0094**	-0.0871	-0.0876	0.0026	0.0030
	[0.005]	[0.004]	[0.059]	[0.058]	[0.003]	[0.004]
Medicaid* HealthChoices* POST2	-0.0116**	-0.0132***	-0.0796	-0.0772	-0.0054	-0.0052
	[0.005]	[0.005]	[0.061]	[0.061]	[0.004]	[0.004]
Medicaid* HealthChoices* POST3+	-0.0238***	-0.0252***	-0.1776**	-0.1813**	0.0032	0.0030
	[0.007]	[0.007]	[0.070]	[0.072]	[0.004]	[0.004]
Observations: # of Deliveries	630,499	548,824	630,499	548,824	630,499	548,824
R-squared	0.023	0.023	0.276	0.280	0.153	0.153
Number of Mothers	283,618	249,469	283,618	249,469	283,618	249,469
Control Group	VOL	FFS	VOL	FFS	VOL	FFS

[1] Standard errors, reported in brackets, are clustered at the county level. All regressions also include the patient and hospital characteristics described in section 5, year, county, and Medicaid fixed effects, the interactions of these fixed effects indicated in specification (2), and mother fixed effects. All estimations also include a separate time trend for each HealthChoices zone and for the control group; regressions in the first four columns also include a variable indicating whether the delivery was a C-section.

[2] *** Significant at the 1 percent level (two-tailed test); ** Significant at the 5 percent level (two-tailed test); * Significant at the 10 percent level (two-tailed test).

Chapter 2

Medicare Part D Program

Medicare Part D, Physician Prescribing and Drug Utilization: A Difference-in-Discontinuity Approach

2.1 Introduction

In the first 40 years after its creation in 1965, Medicare provided no insurance coverage for beneficiaries' prescription drug costs. In 2003, about 27% of seniors aged 65 and above lacked a source of insurance coverage for prescription drugs. Meanwhile, prescription drug expenditures for the elderly increased rapidly at an annual rate of 12%. Associations between the lack of prescription drug coverage and low drug compliance as well as worse health status was establish by researchers in public health and economics.[1] Beginning in January 2006, the Medicare Prescription Drug, Improvement, and Modernization Act of 2003 (MMA 2003 hereafter) introduced a new drug benefit through Medicare Part D. It offered drug coverage for the first time, thereby lowering the fraction of prescription drug costs that many Medicare beneficiaries had to pay out-of-pocket at the point of service. Adoption of Part D was the single largest change in health insurance coverage for the elderly since 1965.

The effects of Medicare Part D and its costs have significant policy implications. Part D has

[1] Federman et al., (2001) found that elderly Medicare beneficiaries with coronary heart disease who lack drug coverage are less likely to use statins, which are relatively expensive but proven to be effective in improving survival.

potentially benefited Medicare beneficiaries. Before Part D, Medicare beneficiaries obtained drug coverage through employer-sponsered plan (28%), Medicare HMO plans (15%), Medigap plans (7%), Medicaid plans (10%) and other State Pharmacy Assistance programs (2%) according to a technical report from Kaiser Family Foundation published in 2003. The copayments of some coverages are high and access to those coverages are declining. Since 2006, Part D has become the primary source of drug coverage for Medicare beneficiaries, covering more than half of all beneficiaries (57%) in 2006. It decreased the percentage of Medicare beneficiaries with no source of prescription drug coverage from 27% in 2003 to 10% in 2007 (Kaiser Family Foundation, 2003; Kaiser Family Foundation, 2006; Kaiser Family Foundation, 2007). Improved prescription drug coverage is likely to result in changes in expenditures for other medical care services or changes in health status, although there is no consistent evidence from past research on whether Part D led to lower expenditures on other services and better health status, or higher expenditures and worse health status.

On the other side, this program is costly. Part D has shifted prescription drug costs from Medicaid or other private payers to Medicare, and has contributed to a net increase in federal spending. The Medicare share of total national spending on prescription drugs increased from 2% in 2005 to 22% in 2006, and the net federal cost of the Part D program is estimated to be $982 billion for nine years between 2007 and 2016 (Kaiser Family Foundation, 2007). The nationwide implementation of a policy with such high costs has raised a lot of concern and criticism.

Past literature has studied the effect of Part D on the utilization and out-of-pocket cost of prescription drugs (Lichtenberg and Sun, 2007; Briesacher et al.,2011; Ketcham and Simon, 2008; Kaestner and Khan, 2012). Although they differ in the estimates of the magnitude of the effect of Part D, they all found that utilization of prescription drugs increased, and out-of-pocket cost decreased for elderly patients after 2006 compared to changes among younger patients. Other studies, focused on the supply side of the health care market, found that brand-name drugs with a high pre-Part D market share experienced slower price increases, and increase in utilization compared to other drugs (Duggan and Morton, 2010). However, none of the previous studies have examined the relationship between Part D and physicians' prescribing pattern.

Physicians are not directly reimbursed for prescribing drugs, but they still play an important

role in determining which drugs patients get. Acting as agents on behalf of the patient, physicians are expected to incorporate their patients' best interest when writing prescriptions. They are expected to (jointly) maximize their profit function and the utility of their patients subject to patients' budget constraints. The adoption of Part D is intended to decrease the out-of-pocket cost faced by patients, and is a component in their budget constraint. Thus, it is reasonable to assume that physicians would alter their prescribing patterns since they are indirectly affected by the policy change through the patients' budget constraints. Ultimately, these changes in prescribing behavior can lead to changes in utilization of and expenditure on prescription drugs, as well as patients' health outcomes.

In this paper, we conduct a comprehensive assessment of the impact of Medicare Part D on physicians' prescribing patterns, and on patient's utilization of and expenditure for prescription drugs as well as for other medical care services such as inpatient, outpatient and emergency room care. We also examine the effect on patients' health outcomes measured as in-hospital mortality and the Prevention Quality Indicators (PQI) module developed by the Agency for Healthcare Research and Quality (AHRQ). We adopt an improved empirical strategy called difference-in-discontinuity (DD-RD) design. This specification is developed from the difference-in-difference (DD) and regression discontinuity (RD) specifications that were popular in past Part D-related studies. The DD-RD allows us to address internal validity issues that exist with the DD strategy, as well as confounding policy discontinuity issues with the RD design. We will estimate the policy effect by comparing the discrete jump in outcome values associated with the introduction of Medicare Part D for our sample before 2006 at the age 65 cutoff and for the sample after 2006.

We study three research questions in this paper. The first is whether the adoption of Part D policy affects physicians' prescribing patterns. We link National Ambulatory Medical Care Survey data (NAMCS) data for 2002-04 and 2006-09 with U.S. Food and Drug Administration (FDA) orange book data and construct a visit-level sample for patients between 60 and 69 years old. We then estimate the Part D effect by exploiting the difference between physicians' prescribing patterns before and after the adoption of Part D in January of 2006 for patients above 65 years old, and that of patients below 65. After controlling for confounding factors, we estimate a 33% increase in the number of prescription drugs prescribed and a 55% increase in the number of

generic drugs. We do not find evidence that Part D affects other practicing patterns such as the total number of drugs prescribed, the number of tests ordered by physicians and the duration of visits (in minutes). We further construct a prescription-level data sample for patients between 60 and 69 years old to analyze the effect of Part D on the age of active ingredients in prescribed drugs, measured by the difference in time between the drug approval date and the date the physician prescribed the drug,[2] and find no significant change in the age of prescribed drugs either. We also find evidence of an anticipatory effect that is not well addressed in most past Part D related research. There was a two-year gap between the announcement of Part D policy in December, 2003 and the actual policy implementation in January, 2006. Both patients and physicians might anticipate the policy during this gap and postpone prescribing or filling prescriptions until the adoption of Part D. We test the size of this effect by comparing estimated effects with and without observations from the post-announcement, pre initiation period.

The second question we study is the impact of Part D on utilization of and expenditure for prescription drugs. We estimate this effect also using our DD-RD specification with data from the Medical Expenditure Panel Survey (MEPS) for the periods 2002-04 and 2006-09. Results indicate that the utilization of prescription drugs per person per year rose by 39%, which is a close and slightly larger effect compared to what we observe in the NAMCS data. This demonstrates that changes in physicians' prescribing patterns play an essential role in the impact of Part D policy, and patients mostly comply with their physician's prescriptions. We find that total expenditure and expenditures by Medicare sources for prescription drugs rose, out-of-pocket (OOP) costs for each prescription fell for elderly patients who turn 65 after 2006, but these effects were not precisely estimated. We also stratify the sample according to income categories and dual-eligibility status, and find that non-dually eligible individuals and low-income or near poor individuals are more affected than others by the adoption of Part D policy.

The last question we examine is whether Part D implementation affected expenditures on other medical care services and eventually, patients' health outcomes. We adopt the same DD-RD specification and use the expenditure component of MEPS data to examine outcomes and

[2]We merge dates of drug approval from FDA orange book database to the NAMCS data by drug name. If the drug name as in the NAMCS data merges to more than one approval date, we use the earliest date no matter which dosage form, applicant or manufacturer this drug belongs to. NAMCS does not contain information on these latter attributes of drugs prescribed.

expenditures on other medical care services. We do not find evidence that Part D resulted in significant changes in total medical expenditures or expenditures for inpatient, outpatient, or emergency room visits. We then estimated the effect on PQIs, in-hospital mortality, length of stay and total charges using the National Inpatient Survey (NIS) data set from the Healthcare Cost and Utilization Project (HCUP) for the years 2002-04 and 2006-09. Again, we find no significant effect of Part D implementation on health status for elderly patients after 2006.

We performed a series of sensitivity analyses in order to check the validity of our DD-RD specification and to test the robustness of our results with the NAMCS data.[3] We plot the density of running variables on either side of age separately for the sample before 2006 and after 2006, in order to test whether the eligibility criteria is manipulated around the threshold or not (McCrary, 2008). We validate a necessary assumption that underlying characteristics are smooth across the threshold in the absence of treatment local to the age cutoff. We also estimate effects using different bandwidths, different orders of polynomial functions of age, and with and without baseline covariates. Fourth, we implement a placebo test using an arbitrary cutoff, 64 years old and over. All robustness check confirm our main findings.

The rest of the paper is organized as follows. Section II presents a brief overview of Medicare Part D and of the existing research on its impact. Section III describes the data, outcomes and independent variables, as well as sample size used in analyses. Section IV discusses the empirical strategy we use to estimate the effect of Part D on outcomes. Sections V, VI and VII summarize the results both for the basic specifications and sensitivity analyses. Section VIII concludes, with a discussion of the policy relevance of our findings and of directions for future research.

2.2 Background

2.2.1 Medicare Part D and Enrollment

Prior to the passage of MMA 2003, Medicare only had two fee-for-service components, Part A and Part B, and one managed care component, Part C. Parts A and B are plans for inpatient

[3]We only performed the complete set of robustness check with NAMCS data, and part of sensitivity analysis with MEPS and HCUP data because the latter two datasets do not contain information on specific patients' birth date and visit date. And we are not able to perform some of those tests without complete information on patients' birth date and visit date.

and outpatient visits and related medical services. Part C, or Medicare Advantage (MA) combines Part A and Part B through private managed care plans. None of these three components included comprehensive prescription drug coverage. Meanwhile, prescription drug expenditures were increasing faster than other areas of health care spending. Average annual per capita drug spending was $\$2,318$ in 2003 for Medicare beneficiaries, and was increasing at an annual rate of 12%. Because of the lack of drug coverage, many beneficiaries were paying for prescription drugs out-of-pocket. Medicare beneficiaries paid $644 out-of-pocket for prescription drugs in 2000 on average, rising to $996 in 2003 (Kaiser Family Foundation, 2003). Medicare Part D was created as part of the MMA 2003 to ensure access to prescription drugs and to limit the financial burden associated with prescription drug costs. It went into effect on January 1, 2006.

Under Part D, eligible persons can participate voluntarily by enrolling in one of two types of private insurance plans in his or her area: a Prescription Drug Plan (PDP) that covers only prescription drugs, or a Medicare Advantage-Prescription Drug Plan (MA-PD) that covers both medical services and prescription drugs. The decision to enroll or not depends on whether that person's current prescription drug coverage offers prescription drug coverage that is equal or better to the coverage under the standard Medicare benefits, known as a creditable plan. Beneficiaries enrolled in traditional Medicare (non-managed care) usually obtain drug coverage through a PDP; those enrolled in a managed care plan through a MA organization generally have to obtain drug coverage from MA-PD plans. The elderly could also keep their current plan, as long as the coverage for prescription drugs is creditable compared to a standard plan under Part D. Dual-eligible beneficiaries, those Medicare beneficiaries who are also Medicaid recipients and had drug coverage through Medicaid programs, are automatically enrolled in Part D plans.

The adoption of Part D resulted in sharp changes in prescription drug coverage once an individual reached age 65. About 53% of Medicare beneficiaries enrolled in Part D plans in 2006. Specifically, 16.5 million people enrolled in stand-alone PDPs, 6 million people enrolled into MA-PD plans, and 6.8 million people had other forms of creditable coverage from employer or union (Kaiser Family Foundation, 2006). After the initial enrollment period, the enrollment numbers continued to grow, from 53% in 2006 to around 60% in 2010 (Kaiser Family Foundation, 2010). The percentage of Medicare beneficiaries without any drug coverage decreased from 19%

in 2002 to 10% in June 2006. Although participation in Part D is voluntary, to avoid the problem of adverse selection, Medicare beneficiaries were charged a financial penalty if they joined the program after May 15, 2006 unless they were able to demonstrate they had access to creditable coverage elsewhere. It further ensures for our analysis that transition into drug coverage plans under Part D at the age 65 cutoff is abrupt.

MMA 2003 required that the standard benefits offered by a prescription drug plan are as follows: after a $250 annual deductible, Medicare beneficiaries pay 25 percent of prescription drug costs for the initial coverage of $2,250. After that, patients pay 100 percent of costs up to $5,100, a gap commonly known as the "donut hole", and 5 percent of costs above $5,100 which is also known as catastrophic coverage (Berndt and Frank, 2007). Premium determination process is complicated. Qualified plans will submit a bid to the Centers for Medicare and Medicaid Services (CMS). The bid reflects the plans' net costs for the drugs, administrative costs and desired profit. CMS reviews each bid and negotiates with plans before achieving a final bid amount. CMS then calculates base premiums using the national weighted average bid times a certain percentage, and each plan can charge beneficiaries the base premium plus or minus the difference between the national weighted average bid and its own bid amount. The national average monthly premium increased steadily from $37.37 in 2006 to $46.73 in 2009. On average, Medicare subsidizes about 75 percent of the cost of standard coverage for all types of beneficiaries through different types of subsidies, including direct subsidies, individual reinsurance, risk sharing payments[4] and low-income subsidies. Specifically, low income subsidies means that those who are also eligible for Medicaid or under 135% of federal poverty line pay no premium and have no deductible, and those whose income are between 135% and 150% of the federal poverty line, pay a reduced premium and annual deductible. Both groups have a lower copayment for covered drugs as a result.

Although all plans are required to be actuarially equivalent with the standard benefit design described previously, each Part D plan has some flexibility to design their own plan benefits. Thus, many plans set their own formulary, which is a tiered structure with different levels of cost sharing across therapeutically similar drugs. Plans often place generics on a tier with the lowest

[4]Direct subsidy is a monthly prospective payment paid by CMS, which mainly reimburses the plan for the cost of initial coverage. Individual reinsurance is provided for a certain percentage of drug spending above enrollees' catastrophic coverage. Medicare finances some unexpectedly high costs or acquires unexpectedly excessive profits in order to limit plans' potential losses or gains, which are called risk-sharing payments.

copayment, preferred brand-name drugs on a tier with a higher copayment, and non-preferred brand-name drugs on a tier with highest copayment. [5] Thus, through design of plan formularies, Part D plans encourage the utilization of generic drugs, in order to help control costs in the plan, and to provide a competitive premium to attract potential consumers.

2.2.2 Hypothesis

We expect Part D to affect physicians' prescribing pattern. Physicians are not directly reimbursed for their prescribing behavior. However, there are reasons to expect physicians' prescribing behavior to change after the introduction of Part D. First, a physician's reputation may be partly determined by how well they take their patients' needs into account. This may include both the ability to attend to their patients' health needs, and their ability to best tailor treatment plans to a patient's financial circumstances. This may in turn provides a disincentive to underprescribe drugs where patients do not get necessary medications they can afford under Part D, or to overprescribe drugs to the extent where the costs exceed the benefits.[6] As the patients' agent, physicians should maximize patient utility subject to a budget constraint. As we discussed earlier, Part D lowers the average out-of-pocket cost per prescription for Medicare beneficiaries through decreased copayments for each prescription filled. The reduced copayments for prescription drugs should lead to an increase in prescriptions. Additionally, plans can create their own formularies to encourage the prescription of generic drugs. Last but not the least, the adoption of Part D coincided with many drug patent expirations. This resulted in increases in the number of available generic drugs (Hoadley, 2012). For the reasons above, we expect to see changes in the prescribing patterns measured by number of prescription drugs (especially generic drugs) physicians prescribe to their elderly patients after the introduction of Part D. [7] We also expect Part D to affect the patients' utilization and expenditure for prescription drugs conditional on patients complying with their prescriptions.

We also expect Part D to impact other health care expenditures, and eventually the health status of beneficiaries. Greater utilization of prescription drugs might lead to effects on expenditures

[5] Part D plan formularies are subject to some regulation, for example, they are required to cover all the drugs under protected drug class and at least two drugs in each therapeutically class.

[6] Hellerstein (1998) provided evidence that physicians act as agents for consumers in their prescribing decisions.

[7] See appendix for a detailed derivation of the theoretical model and theorems.

for other health care services, and could improve health outcomes. When physicians maximize patients' utility subject to a budget constraint, the decreased OOP cost for prescription drugs results in greater consumption of prescription drugs. As for consumption of other medical goods, the direction of the effect depends on the cross price elasticity between the consumption of drugs and those other medical care services . If prescription drugs and other medical services are complements (cross price elasticity negative), we would expect increases in consumption of other medical services (Curtis et al., 2004). If prescription drugs and other medical services are substitutes (cross price elasticity positive), we would expect decreases in consumption of and expenditure on other medical services, therefore possibly partially offsetting the costs of Part B. Only a small literature explores this issue and the findings are mixed.

Taken together, we expect that Medicare Part D will increase prescribing of prescription and generic drugs, and increase patients' utilization of and expenditure on prescription drugs among elderly patients. We also expect Part D to impact expenditures on other medical care services, and ultimately patients' health status.

2.3 Literature Review

2.3.1 Prescribing Behavior

Past literature has demonstrated that prescription decisions are associated with patients' insurance status and illness severity, as well as physician and practice characteristics. Studies suggest that physicians play an important role in determining how many and what types of prescription drugs patients receive. Tamblyn et al. (2003) study the prescribing activity of Quebec physicians under the universal health program in Canada to see if physician or practice characteristics are associated with the utilization of new drugs. They find that physician sex, specialty, practice location, visit volume and the proportion of elderly in the physician's practice all influence the utilization of new drugs. Hellerstein (1998) posits that physicians are important agents in the decision-making process of whether patients get generic or trade-name versions of drugs, and finds that very little of the variation in prescription drugs received can be explained by patients' characteristics.

As discussed earlier, physicians are not directly reimbursed by insurance companies for prescribing drugs; however, they are expected to act in the interest of patients when prescribing. Blomqvist (1991) examines the patient/physician relationship and suggests that physicians are "double agents", representing both patients and insurers' interest. They act more in the interest of patients under fee-for-services plans and more in the interest of insurers under health maintenance organization (HMO) plans. Lundin (2000) expands the analysis based on the Blomqvist model, and studies situation where there are no incentives to act in the interest of patients or insurers. He finds that physicians will consider patients' interests more in this situation, and they are less likely to prescribe trade-name versions of drugs to patients who have to pay a large sum out-of-pocket. This lends some credibility to our results. Physicians may also change their behavior in response to Part D if they receive more medications from pharmaceutical representatives (PRs) after the introduction of Part D. [8]

2.3.2 Medicare Part D Studies

There is a previous body of literature that analyzes the effect of Medicare Part D on patients' out-of-pocket costs and the utilization of prescription drugs, usually employing difference-in-difference (DD) methods. Lichtenberg and Sun (2007), Yin et al.(2008) and Ketcham and Simon (2008) use DD to analyze the effect of Part D with prescription claims data from national pharmacy chains. Lichtenberg and Sun (2007) focus on the period from September 2004 to December 2006 and find that Medicare Part D reduces OOP daily cost of therapy by 18.4 percent, and increases number of days of therapy by about 12.8 percent in 2006 compared to those for non-elderly users. They also estimate that every seven prescriptions paid for by the government results in only two additional prescriptions used, which means the crowd out rate is around 72%. Yin et al. (2008) choose the near elderly group (those aged 60-63 year old) as the reference group. They find a 5.9% increase in the monthly average utilization and 13.1% decrease in monthly OOP cost after the penalty free period (i.e., after June 2006). Ketcham and Simon (2008) compare outcomes for those who are always eligible for Medicare (over 66 as of 2007) to a control group of those always

[8] Lurie et al. (1990) conducted a survey among internal medicine facilities in seven large mid-western teaching hospitals and house staff from two of teaching programs and find that frequent contact with PRs is associated with changes in prescribing behavior.

ineligible for Medicare (58-64 as of 2007). They estimate an effect with similar magnitudes, that days' supply and number of individual prescriptions filled increase by 8.1% and 4.8% respectively, and OOP costs fall by 17.2% for the eligible group in the first year after Part D. Engelhardt and Gruber (2011) and Liu et al. (2011) study the topic with Medical Expenditure Panel Survey data as well as the DD strategy and find that Part D has a relatively small impact on OOP spending (Engelhardt and Gruber, 2011), and is associated with a $179.86 reduction in OOP cost and an increase of 2.05 prescriptions per patient year (Liu et al., 2011).

Kaestner and Khan (2012) focus on Medicare beneficiaries aged 65 years and older. They used an instrumental variable method by exploiting plausibly exogenous changes in prescription drug coverage engendered by Medicare Part D. Their study finds that getting drug coverage is associated with an approximately 70% increase in the use of prescription drugs for the general population of elderly from 65 to 85 years old, and a 60% increase in the use of prescription drugs for those with chronic conditions.

Another empirical strategy used to examine the effect of Part D on the utilization and out-of-pocket costs of prescription drugs is time-series analysis. Briesacher et al. (2011) simulated post-Part D outcomes using time-series regressions with a first-order autoregressive correlation structure and pre-Part D data. Comparing simulated with observed results, they conclude that average prescription fills per person increased significantly by 1.8 fills in 2006 and 3.4 fills in 2007, compared to a 0.9 fill increase per year before Part D. They also found that average OOP drug costs decrease significantly in both 2006 and 2007, by $143 and $148 per year respectively.

Other papers examine the effect of Part D in the pharmaceutical market. Duggan and Morton (2010) provide evidence of the effect of Medicare Part D on the prices and utilization of branded drugs. They analyze data on top-selling brands and find that drugs with high pre-Part D Medicare market share experience a significantly lower average price increase than other brands, and an increase in utilization. Combined with the fact that Part D lowers the fraction of drug prices paid by beneficiaries; the average cost of prescription drugs for a Medicare recipient with average drug spending decreases substantially. There are additional price declines in the second and third year that appear to have reversed by the fourth year (2009) (Duggan and Morton, 2011).

Few studies examine the impact of Part D on the utilization or expenditure of health care

services other than prescription drugs, and conclusions are not consistent amongst them. Engelhardt and Gruber (2011) and Liu et al. (2011) find almost no cost offset effects in other medical spending, such as expenditures for inpatient, outpatient and emergency room visits. Similarly, Kaestner and Khan (2012) find that Part D does not reduce inpatient stays or outpatient visits, nor does it improves patient health status, and conclude that Part D is not likely to produce a net gain. On the other side, Zhang et al. (2009) compare three groups of Medicare beneficiaries with no or limited coverage before the implementation of Part D and that enroll in a Part D plan afterwards, with a control group which had stable, uncapped drug coverage. Compared to the control group, the increase in the total monthly drug spending is higher, while the monthly medical expenditure is lower for the new enrollees, suggesting some offset effects.

2.3.3 Contributions of This Paper Compared to Past Literature

We improve upon previous literature on four dimensions. First, most previous studies uses DD (Lichtenberg and Sun, 2007; Yin et al., 2008; Ketcham and Simon, 2008; Engelhardt and Gruber, 201; Liu et al., 2011). These studies estimate the effect of Part D policy by comparing changes in outcomes for elderly beneficiaries before and after 2006 relative to changes for patients who are under age 65. This potentially raises questions about the internal validity of their identification, because by using the DD strategy, they assume that outcomes of the elderly group are comparable with those of the non-elderly group in the absence of Medicare Part D. This is a strong assumption, and may not be valid in the context of utilization of prescription drugs. For example, physicians may have different prescribing patterns for older patients as they are also generally sicker than younger patients. Thus, we adopt an improved empirical strategy by combining DD with a regression discontinuity design. A regression discontinuity (RD) design allows prescribing pattern to be different for elderly patients compared to non-elderly patients both before and after the policy adoption year. Researchers are able to estimate the causal relationship between the policy and health care outcomes by measuring the discrete jump in outcomes at the age cut-off for treatment eligibility among patients within a narrow age range. By combining the DD and RD specifications, we are essentially comparing the change in the discontinuity at the age 65 cut-off before and after the adoption of Part D, which allows us to identify the effect of Part D without

confounding discontinuities produced by such conditions as the fact that individuals also become eligible for Medicare Part A and Part B when they turn age 65.

Second, few studies consider whether patients might have anticipated the upcoming Part D policy in the years prior to 2006 and reduced their utilization of prescription drugs temporarily. The MMA was signed into law in December 2003, but Part D was not implemented until Jan 1, 2006. Given the two year gap between announcement and implementation, patients, as well as their physicians, could have had anticipatory behaviors. However, most past literature utilize data within a narrow window starting from late 2004 or early 2005, and measure the effect of Part D assuming the sample of patients before 2006 would have experienced a similar trend as the sample after 2006 in the absence of the Part D policy. The only study we are aware of considering anticipatory effects is by Alpert (2011), who studied the impact of anticipation of Medicare Part D on drug utilization. They find that individuals defer drug use to the time period with subsidized coverage. After accounting for these anticipatory effects, the Part D effect decreased by about half. This suggests an upward bias in the estimated Part D effect in previous literature. Our study provides further evidence of anticipatory behavior and tests the size of this effect by comparing the estimated effects with and without samples in the anticipation year 2005.

Third, few studies examine the impact of Part D on physician behavior. The introduction of Part D is one of the most significant changes in public health insurance policy since the introduction of the Medicare and Medicaid programs in the 1960s, and the implementation of Part D in 2006 significantly increased federal spending. The impact of Part D on physicians' behavior is pertinent since physicians are critical in deciding the number and kind of prescription drugs patients receive. We also examined the effects on expenditures on other medical care services and patient health outcomes in order to test whether a cost offset effect exists or not.

Finally, we use different data. Some past literature studies the effect of Part D using pharmacy chain claims data, but even though these data sets have rich detail on drug characteristics and payment information, they only represent the sample of patients who had one or more prescription filled during the sample period. Estimating the effect of Part D on patients who have at least one prescription filled may bias the effect of Part D policy upward. And there is a strong chance that the datasets are not nationally representative. In contrast to these studies, we use data from

the National Ambulatory Medical Care Survey. It is a nationally representative sample of visits to non-federally employed office-based physicians in the U.S. It allows us to identify the discrete jump in the number or types of drugs prescribed at the age 65 cutoff before and after year 2006. We take advantage of knowing the specific date of birth of patients and the date of visit, and model the relationship between age and drug prescribing using a flexible polynomial in age that can take different forms for patients under and over age 65, and before and after 2006. Also, we use NAMCS data through 2009, allowing us to examine longer-term effects of Part D on physician behavior compared to previous literature.

2.4 Data

We compile our samples from four different sources. Our main analyses use the National Ambulatory Medical Care Surveys (NAMCS) for 2002-04 and 2006-09 and the Food and Drug Administration (FDA) Orange Book database to estimate the impact of Medicare Part D on physicians' prescribing patterns.[9] The NAMCS is conducted by the National Center for Health Statistics and collects data on a nationally representative sample of visits to non-federally employed office-based physicians in the U.S. excluding radiologists, anesthesiologists, and pathologists. A multistage probability sample design is used to select physicians for interview; this design is described elsewhere (Bryant and Shimizu 1988). Each physician is then randomly assigned to a one-week reporting period. During this period, data for a systematic random sample of about 30 visits are recorded for each physician. Physicians and patients may be selected multiple times but there is no identification number to link them longitudinally. (We exclude the year 2005 from the main analysis because of the possible existence of anticipatory effects, which we will explain in detail later.) Each physician is asked to record information on up to eight drugs (six in 2002) that were ordered, supplied, administered or continued during the visit. Prescription drugs, over-the-counter drugs, immunizations, allergy shots, anesthetics, chemotherapy, and dietary supplements are included.

[9]FDA orange book database contains information on the date of FDA approval, and application type by drug name for drugs approved since January 1, 1982. Application type can be NDA or ANDA which indicate whether it is a brand-name drug or generic drug, respectively (http://www.fda.gov/Drugs/InformationOnDrugs/ucm129689.htm).

We merge the earliest date of FDA approval for the active ingredient contained in the drug and application type onto the NAMCS data using the drug name.[10] We do this because there are no consistent variables to identify the drugs in both the NAMCS data and FDA data . Excluding over-the-counter drugs, the merge rate was around 95%. Most of the non-merges are drugs approved prior to 1982.

We first construct the visit level sample where we examine prescribing patterns measured by number of prescription drugs, number of generic drugs , number of branded drugs, number of over-the-counter (OTC) drugs, total number of drugs prescribed, and any drugs prescribed.[11] There are 27,983 visit records for patients aged 60-69 years old in the NAMCS for the sample period 2002-04 and 2006-09. We limit the sample to 26,474 visits, excluding 1,509 (5.39%) visits among patients who have missing information on any variable used in the analyses. [12]

We also use prescription-level data to examine the effect of Part D on the age of the active ingredients of prescription drugs physicians prescribe to their patients. For drugs with different manufactures or strengths or packages, we use the earliest date of approval. (The NAMCS data contain no information on dosage.) We examine the effect of the introduction of Medicare Part D on the age of active ingredient among prescription drugs approved since 1982. Our prescription-level sample has 54,132 observations for those aged 60-69.

The NAMCS data also includes information on patients' demographic characteristics such as gender, race, disease categories defined by ICD-9 codes, and on physician characteristics like metropolitan statistical area (MSA) status, whether it is a solo practice or not, adoption of electronic medical records, specialty categories, and state of practice. We control for these variables as baseline covariates in our specification.

We then analyze Medical Expenditure Panel Survey (MEPS) 2002-04 and 2006-09 to estimate the effect of Part D on patients' actual utilization of and expenditure on prescription drugs, as well as expenditure on other health care services. MEPS is a nationally representative set of

[10] See appendix for the description of merging FDA data with NAMCS data.

[11] Generic drugs are identified by the variable, "application type" from FDA orange book data set: application type "A" indicates generic drug, application type "N" indicates brand-name drugs.

[12] One of the control variable, percent of revenue from Medicare patients, is imputed for 6.29 percent of the sample. Imputation was done with a tobit model controlling for physician specialty dummies, percent of patients aged 65 and over, and percent of the county population aged 65 and over (for the county where the physician practices).

respondents drawn from the National Health Interview Survey (NHIS). It is a two-year panel that contains information on number of prescriptions filled, total expenditure for prescription drugs, and expenditure for drugs from different sources such as self pay, Medicare, Medicaid or private sources. We also examine outcomes such as expenditures on all medical care services, inpatient, outpatient, office or emergency room services, as well as self reported health status. As for independent variables, we control patients' gender, race/ethnicity, poverty category, census region, MSA status, education level, marital status, and prior health conditions (whether the individual has certain types of chronic conditions) including asthma, high blood pressure, angina, heart attack, joint pain, stroke, emphysema, arthritis or other heart disease. After excluding observations with any missing information on the variables we use, we are left with 276,774 observations for the group of respondents 60-69 years old.

The last data set we study is the Nationwide Inpatient Sample (NIS), which is part of the Healthcare Cost and Utilization Project (HCUP). It is the largest publicly available all-payer inpatient care database in the United States, and containing a twenty percent stratified sample of all discharges from U.S. community hospitals. We use the Prevention Quality Indicators (PQI) module developed by the Agency for Healthcare Research and Quality (AHRQ). PQIs identify hospital admissions that may have been avoided through access to high-quality outpatient care or patient adherence to treatment recommendations. The primary reasons for discharge considered avoidable by this module include diabetes short-term complications, perforated appendix, diabetes long-term complications, COPD or asthma in older adults, hypertension, heart failure, dehydration, bacterial pneumonia, urinary tract infection, angina without procedure, uncontrolled diabetes, lower-extremity amputation diabetes. We examine outcomes such as the incidence of admissions due to each one of these conditions, admission due to any one of these conditions, in-hospital mortality, length of stay and total charges in order to study the effect of Part D on quality of ambulatory care service, as well as measures of quality and cost of inpatient stays. We control for patient characteristics such as gender, whether admission is during a weekend, elective or through emergency department, whether the patient is transferred from another hospital, and Clinical Classification Software (CCS) categories (Elixhauser et al., 2013); we also include hospital characteristics such as bed size, ownership, urban or rural hospital, teaching status and quartile

of median family income at the zip code level. There are 4,770,572 discharge records for patients between age 60 and 69 years old. After excluding patients with missing information on any of the variables we use as independent variables, we have 4,525,332 observations in our final sample.

2.5 Empirical Strategy

We now discuss the empirical strategy we use to analyze the causal effect of Part D on prescribing patterns, utilization and expenditure of prescription drugs, and on health outcomes. In this section, we will present popular estimation strategies from past literature first, discuss the weakness of these approaches, and then propose the regression specification developed from past strategies we employ.

As we discussed in Section 3, the most popular empirical strategy utilized in the past is the difference-in-difference (DD) approach. A DD approach normally defines those eligible for Part D or those with Part D coverage as the "treatment group", and those ineligible for Part D or those without Part D coverage as the "control group". The idea is to compare the outcome changes for the treatment group before and after the treatment (Meyer, 1994), i.e., the introduction of Part D in 2006, with those of the control group before and after the treatment. In the context of this paper, we would estimate the effect of policy by exploiting the relative changes in prescription drug utilization by elderly patients who visit physicians' office before and after Jan 1, 2006, compared to those of near-elderly patients who visit physicians' office before and after Jan 1, 2006. The typical estimation equation is:

$$\begin{aligned} Outcome_{ij} = \beta_0 + \beta_1 \times 1(age >= 65)_i + \beta_2 \times 1(year >= 2006)_j \\ + \beta_3 \times 1(age >= 65)_i \times 1(year >= 2006)_j + \beta_4 X_i + \beta_5 Y_j + \beta_6 State + \epsilon_{ij}, \quad (2.1) \end{aligned}$$

where $Outcome_{ij}$ is the outcome (number of prescription drugs, number of generic drugs, number of brand-name drugs and etc.) for patient i by physician j, $1(age >= 65)_i$ is an indicator for whether the patient i is over 65 years old, and the variable $1(year >= 2006)_i$ is an indicator variable for whether the visit takes place on or after January 1, 2006. The specification includes control variables for patient characteristics, X_i (which are patient attributes including gender,

race, year of the visit took place and dummies for the major diagnostic category associated with the visit), physician practice characteristics, Y_j (which are characteristics of the physician or practice including specialty of the physician,[13] whether the physician is in solo practice or uses electronic medical records, and whether the practice is within a metropolitan statistical area (MSA), percentage of revenue from Medicare patients) and state fixed effects $State$, which control for any time-invariant characteristics of physician practice patterns in different states. The DD estimator is β_3 and Equation (1) is estimated by ordinary least squares. [14]

A weakness of the DD approach is that it assumes that trends in prescribing patterns are the same for both the treatment and control groups throughout the analysis period so that the DD estimator can identify only the effect of Part D. However, trends in prescribing patterns are likely to be different by patient age, making it likely that prescribing patterns may differ for treatment and control groups.[15] The DD approach used in past literature took this difference into account somewhat by limiting the sample to a narrow age range, but it does not really address the issue or verify the validity of the basic assumption. Additionally, because of the anticipation effect we discussed above, the validity of this assumption is even more questionable.

The second empirical approach, more popular among recent policy analyses, is to use a regression discontinuity (RD) design. Card et al., (2009) uses this approach to identify the impact of Medicare. The main idea is to compare the immediate change in outcomes for patients that are just before and after the age 65 threshold with only data from after policy adoption. A general model using the RD design is:

$$Outcome_{ij} = \beta_0 + \beta_1 \times 1(age >= 65)_i + \beta_2 f(Age_i) + \beta_3 f(Days_i) + \beta_4 X_i + \beta_5 Y_j + \beta_6 State + \epsilon_{ij}, \quad (2.2)$$

where $f(Age_i)$ is a flexible polynomial function of patients' ages (measured by days from/to his

[13]Internal Medicine, Pediatrics, General Surgery, Obstetrics & Gynecology, Orthopedic Surgery, Cardiovascular Diseases, Dermatology, Urology, Psychiatry, Neurology, Ophthalmology, Otolaryngology, or Other specialties relative to General/Family Practice.

[14]We also estimate the model using Generalized Least Square (GLM) regressions and present results in Appendix Tables. The results are essentially similar with what we have from OLS regressions.

[15]We calculate the number of prescription and generic drugs prescribed by physicians by age group and perform Wald test to examine whether there is a statistically significant difference between means of outcomes between pair-wise age groups, and p-values from results reject the null hypothesis that means are equal.

or her 65th birthday), and/or its interaction term with an age 65 dummy. $f(Days_i)$ is a flexible polynomial function patients's visit date (measured by days from January 1st, 2002 which is the start of our sample period) and its interaction term with year 2006 dummy. β_1 is an estimate of the causal effect of the Part D policy on outcomes. According to Lee and Lemieux (2010), the key assumption for the RD design is that assignment to either side of the discontinuity threshold is as good as random. In order to satisfy this underlying assumption, Card et al. (2009) narrows the analysis sample to elderly patients with ages just before and after 65, or select "non-deferrable conditions" (unplanned admission through the emergency room with very similar weekend and weekday admission rates) to address sample selection bias.

It would be ideal to adopt the RD specification and estimate the discontinuity in observed outcomes at the age 65 cutoff. Unfortunately, as mentioned earlier, there is a confounding policy discontinuity at the age 65 cutoff. Individuals will be eligible for Part D when they turn 65 years old after year 2006. At the same time, other insurance characteristics change abruptly at age 65 because of the onset of eligibility for Medicare Parts A and B. That is, elderly individuals become eligible for coverage of inpatient and outpatient services under the traditional Medicare program at age 65. Card et al. (2009) show that there is a 65-percentage point increase in Medicare coverage at age 65, and about a nine percentage point increase in the fraction of people with any coverage. Thus, the eligibility cut-off criteria for original Medicare and Part D are the same, both at age 65 years old. If we still adopt am RD specification to analyze the effect of Part D, it will violate the RD assumption that potential outcomes are continuous in the absence of treatment, and it would be difficult to disentangle the impact of Part D policy from the impact of changes in Medicare or health insurance availability generally. Thus, we introduce a new empirical strategy to estimate the effect of Part D in the section below in order to control for the potential impact on outcomes from the eligibility for coverage of inpatient and outpatient services under Medicare when patients turn 65.

2.5.1 DD-RD Identification and Estimation Strategy

In this paper, we propose an empirical approach which combines the DD and RD approaches (we call this specification DD-RD hereafter), in order to identify the average treatment effect of

Medicare Part D when implemented on January 1, 2006 for individuals aged 65 and over.

The DD-RD design enables us to identify the treatment effect while addressing the internal validity issue. The intuition is that the confounding policy, which is Medicare program eligibility at 65 years old, is time-invariant in the sample period. We can estimate this discontinuity prior to adoption of Part D, and subtract that effect from the estimated discontinuity after the adoption of Part D. As long as the discontinuity resulting from the confounding policy is constant, we can address the confounding issue using the DD-RD specification.

The identification strategy we present here requires milder assumptions compared to either the RD or the DD specifications. We do not require that treatment and control groups experience the same trend in outcomes in the absence of policy adoption, nor do we require that all other variables to be continuous across the age 65 threshold. According to Grembi et al. (2011), the only two assumptions required by this specification are: *1, the confounding discontinuities must be time-invariant.* This is equivalent to the RD condition about continuity of potential outcomes; and *2, the treatment effect and the confounding effect are additive.* This is equivalent to the additivity conditions in the DD specification.

To formally estimate the discontinuities in outcomes beginning in 2006 for those aged 65 to 69 compared to those aged 60 to 64 using this approach, we estimate the following equation:

$$Outcome_{ij} = \beta_0 + \beta_1 \times 1(age >= 65)_i + \beta_2 \times f(Age_i) + \beta_3 \times 1(age >= 65)_i \times 1(year >= 2006)_i + \beta_4 \times 1(year >= 2006)_i + \beta_5 \times f(Days_i) + \beta_6 X_i + \beta_7 Y_j + \beta_8 State + \epsilon_{ij}. \quad (2.3)$$

Additional control variables include: X_i (which are patient attributes including gender, race, year of the visit took place and dummies for the major diagnostic category associated with the visit); Y_j, (which are characteristics of the physician or practice including specialty of the physician,[16] whether the physician is in solo practice or uses electronic medical records, whether the practice is within a metropolitan statistical area (MSA) and percentage of revenue from Medicare patients); and *State*, state fixed effects which control for any time-invariant characteristics of physician

[16]Internal Medicine, Pediatrics, General Surgery, Obstetrics & Gynecology, Orthopedic Surgery, Cardiovascular Diseases, Dermatology, Urology, Psychiatry, Neurology, Ophthalmology, Otolaryngology, or Other specialties relative to General/Family Practice.

practice patterns in different states. All analyses use sample weights, and standard errors account for the complex design of the NAMCS survey, the MEPS data and the NIS-HCUP data using Stata software version 12 (Stata-Corp, College Station, Texas).

In order to best model trends in physician prescribing patterns by patients' age, we take advantage of the exact dates of birth in the NAMCS data.[17] Accordingly, Age_i is the number of days measured by a patient's age at visit from/to their 65th birthday. We include $f(Age)_i$, which is a flexible polynomial function of Age_i fully interacted with age 65 and year 2006 dummies. By including $f(Age)_i$, we allow prescribing pattern and other outcomes to take different forms on either side of the age 65 cutoff before and after year 2006. Our base specification uses a cubic form for age in the analysis for NAMCS data, and a linear function in the analysis of MEPS data and and NIS-HCUP data.[18]

$Days_i$ is the number of days since December 31, 2001, the start of our sample period. By including $f(Days)_i$, which is a flexible polynomial function of $Days$ and their interaction terms with year 2006 dummy, we are able to model the trend for outcomes flexibly before and after January 1, 2006. We utilize exact dates of visits in the NAMCS data and measure the number of days since the start of our sample period. This enables us to more precisely model trends in physician prescribing patterns over time, while allowing the discrete change at the time of adoption of the policy.

Under the identifying assumption that other determinants of prescribing patterns are continuous at the age 65 cutoff (defined by whether or not patients have passed their 65th birthday when they visit the physician) both before and after year 2006, the coefficient of the interaction of the age 65 dummy and the year 2006 dummy, β_3 in Equation (3) will be an unbiased DD-RD estimate of the effect of Part D on outcomes.

In addition to visit-level regressions, we also perform prescription-level regressions to see if the age of the active ingredient contained in drugs prescribed by physicians changed beginning in

[17] We don't have the specific date of birth in MEPS and NIS-HCUP data, so the age functions in the estimations for those two datasets only have age in years information.

[18] We decide the order of polynomial for the function of age using two methods. We compare the goodness-of-fit among specifications with different order of polynomial function according to Lee and Lemieux (2009). We also adopt Wald test to examine the joint significance of additional polynomial terms. The results for both tests will be presented in the table along with regression results with the order of polynomial terms chosen by both methods, in order to check the robustness of our results.

2006. The DD-RD specification of the regression is similar with Equation (3):

$$Age\_Drug_{ijk} = \beta_0 + \beta_1 \times 1(age >= 65)_i + \beta_2 * f(Age_i) + \beta_3 \times 1(age >= 65)_i \times 1(year >= 2006)_i + \beta_4 \times 1(year >= 2006)_i + \beta_5 \times f(Days_i) + \beta_6 X_i + \beta_7 Y_j + \beta_8 D_k + \beta_9 State + \epsilon_{ij}, \quad (2.4)$$

where the dependent variable Age_Drug_{ijk} is the age of the active ingredient contained in the drug measured in months between time of visit to the physicians' office and date of FDA approval. We include all controls from Equation (3), also add fixed effects for primary drug class categories, D_k. One issue with the data is that the FDA orange book database assigns the value of "Prior to Jan 1, 1982" to drugs approved before 1982. For this reason, we utilize censored normal regression for the prescription-level analysis with the outcome of age of drugs prescribed, allowing the censoring value to vary from observation to observation.

We test the validity of the RD specification and the robustness of results in many ways. First, we check the density of running variables around the age 65 cutoff before and after year 2006 in order to test whether the eligibility criteria is manipulated around threshold or not (McCrary, 2008). Second, we estimate specifications using different age bandwidths and different orders of polynomial functions of ages, with and without baseline covariates. Third, we implement the DD-RD specification with baseline covariates as outcomes to validate the basic assumption of the DD-RD specification that all underlying characteristics are smooth around the age 65 threshold in the absence of the treatment. Fourth, placebo DD-RD estimation is implemented at an arbitrary cutoff defined as 64 year old and over.

2.6 Main Results: Effect of Part D on Physicians' Prescribing Patterns

We discuss the main results in this section. We first present graphical evidence of the impact of Part D on physician prescribing behavior with NACMS data. Then we estimate how much Part D affected physicians' prescribing patterns for elderly patients after the adoption of Part D policy, and how anticipatory effects might cause overestimates of the true effect. We also show

how other practice patterns change in the physician office visit setting.

2.6.1 Graphical Evidence

Figure 2.1 plots the mean for the number of prescription drugs prescribed by physicians by age per quarter, as well as fitted age profiles from regressions that include cubic terms in age (in days from/to 65 year old birthday), and full interactions with age 65 and year 2006 dummies. There is a small but noticeable decrease in the number of drugs that physicians prescribe to patients when patients turn 65 years old for the sample of 2002-04. For the sample after the adoption of Part D policy, the average number of prescription drugs prescribed per patient increased from 2.4 scripts for non-elderly patients, to around 2.7 scripts when they turn 65 years old.

Figure 2.2 shows the mean for the number of generic drugs physicians prescribe per quarter by age and fitted age profiles from regressions with cubic function of age for each side of age 65 and year 2006 cutoff. There is no discernible jump in the outcome from below age 65 to above age 65 before policy adoption, but the average number of generic drugs prescribed to patients increases from 0.9 scripts to over 1.2 scripts, roughly a 0.3 script increase after policy adoption.

2.6.2 Estimates of the Effect of Part D on the Prescribing Pattern and Anticipatory Effect

In the top panel of Table 2.1, we summarize regression estimates of the change in the number of prescription drugs and in the number of generic drugs prescribed.[19] The first column is the DD-RD specification without any baseline covariates, allowing for cubic functions of age on either side of the age 65 and year 2006 cutoffs. The coefficient of the interaction term between the age 65 dummy and year 2006 dummy is positive and significant, which indicates that there is a 0.61 script increase for the elderly patients relative to nonelderly patients after adoption of the Part D policy. We then separate the 2002-04 sample and 2006-09 sample, and run a conventional RD regression with each sample. The results are listed in the second and third column of the top panel. The coefficient of interest for the regression of the sample after 2006 is positive and

[19] We perform GLM regression with log link and Gaussian distribution, which is determined by Park test. Results are summarized in Appendix Table $C.5$. The significance of results from OLS regression still hold and the calculated marginal effects are similar with what we have from OLS regressions.

significant, implying that there is a 0.3 script increase for elderly patients. The increase is similar in magnitude with that of the discontinuity from Figure 2.1. For the 2002-04 sample, the RD coefficient is negative with a magnitude of 0.29 scripts. This implies that before the adoption of the Part D policy, the number of drugs prescribed at physician visits discontinuously decreased at age 65. The coefficient may reflect the fact that elderly patients were likely to lose their prescription drug coverage from employer sponsored insurance or other public/private coverage at age 65. Subtracting the estimated effect for the sample before 2006 from that for the sample after 2006, we have a 0.6 script change for the elderly relative to nonelderly patients after the adoption of Part D.

In the fourth column, the estimated DD-RD coefficient after controlling for baseline covariates is slightly lower compared to the estimate without controls, but still significant with a 0.56 script increase. This provides support for the assumption that there are no other changes occurring at age 65 in 2006 that are confounding our analysis. This confirms our observation from Figure 2.1, that physicians increase their prescriptions to elderly patients on average by 33 percent (calculated with 0.5625 divided by 1.5995, the mean for number of prescription drugs for non-elderly patients before 2006 from Appendix Table $C.1$) after the adoption of Part D, compared to their non-elderly patients. The coefficient on the interaction term is still significant if we change the order of the polynomial function of age to a quadruple in column (5). Consistent results across different specifications imply that the estimated effect is robust to various functional forms of age, and the significance of coefficients is not an artifact of how we specify the age control function.

Columns (6) - (10) summarize results for the number of generic drugs prescribed. In the top panel, the estimated result for the regression with a cubic function of age is positive and significant, indicating an increase of 0.26 script on average. This is similar to the magnitude of increase in Figure 2.2. We also get similar results with regressions on the 2006-09 sample, using the DD-RD specification with controls and with higher order polynomial functions of age. This indicates that physicians are likely to increase the number of generic drugs prescribed to elderly relative to nonelderly patients after the adoption of Part D policy by 55 percent (calculated with 0.2556 divided by 0.4676, the mean of number of generic drugs prescribed for non-elderly patients before 2006).

The bottom panel of Table 2.1 presents corresponding regression results for the sample that includes patients visiting physician offices in 2005. In the bottom panel, we can see that the magnitudes of effects from the estimation of the DD-RD specification, with or without baseline covariates, are all greater than those in the top panel. After stratifying the sample according to the year 2006 cutoff and running separate regressions on the sample before and after year 2006, we find that the estimated effect for the sample before 2006 is negative and significant for number of prescription drugs. Comparing results in the second column from the bottom panel to that in the top panel, we believe the difference comes from the decrease in the number of prescription drugs for elderly patients in 2005. Elderly patients and their physicians anticipate the upcoming Part D policy, and postpone or reduce the number of prescriptions until the time when patients get their Part D coverage, a result also documented in Alpert (2011). We test the difference between coefficients from the top and the bottom panels using Wald test, in order to examine the existence of an anticipatory effect in the year just before the adoption of Part D policy. The results of tests are summarized in the table and p-values rejects the null hypothesis that two coefficients are equal, which indicate the existence of anticipatory effect. After including the 2005 sample in the regression, our estimate that Part D results in physicians increasing the prescription of drugs for elderly patients by 44 percent, which is a more than 35 percent greater than the effect from our previous DD-RD estimation. This effect is similar to the size of the anticipatory effect found in Alpert (2011). There is also weak evidence that this effect also exists for the prescription of generic drugs, but the estimate is not precise.

2.6.3 Estimates of the Effect of Part D on Other Practicing Patterns in the Physician's Office

We estimate the effect of Part D on practice patterns other than prescribing prescription drugs in the office setting. Again, we utilize the DD-RD specification with cubic and quadruple functions of age. Results are summarized in Table 2.2, those with a cubic polynomial of age profiles are in the top panel, and those with quadruple polynomial of age profiles are in the bottom panel.

Estimation of the results in the first, second and last columns reveals no evidence of change in any prescription drugs and in the number of brand-name drugs prescribed for elderly patients

after the Part D policy. However, physicians write 0.16-0.24 fewer scripts for OTC drugs for their elderly relative to nonelderly patients after 2006. This is a 51-77 percent effect, considering physicians make 0.31 prescriptions for OTC drugs to non-elderly patients on average before 2006. However, if we compare the magnitude of the coefficients to that of the increase in the number of prescription drugs from the previous regressions, we see a smaller decrease in absolute terms. This implies that the total number of drugs physicians prescribe for their elderly patients increases. The fourth column of Table 2.2 provides weak evidence that the total number of drug prescribed increases, although the effect is not precisely estimated.

Appendix Figures $C.1$-$C.7$ plot the age profiles for these outcomes separately for the 2002-04 sample and the 2006-09 sample, and provides evidence that increases our confidence in the sign and magnitudes of regression coefficients we record in Table 2.2.

We also examined the effect of Part D on the number of tests conducted in the physicians' office, patients' time spent with their doctor and the age of active ingredients for prescription drugs.[20] These outcomes are complementary measures of treatment intensity in a doctor's visit. The age of active ingredients for prescription drugs is analogous with physicians' adoption of technology, i.e. "younger" drug physicians prescribe, newer technology they adopt in treating their patients. We didn't find any significant results for these three outcomes with either cubic or quadruple polynomials in age as controls, which indicate that physicians do not change treatment pattern for their elderly patients in these other ways when they learn their elderly patients are more likely to have drug coverage.

2.7 Validity of Specifications and Robustness Check

In this section, we test the assumptions of the DD-RD specification, and perform sensitivity analyses in order to check the robustness of our main results.

First, we graphically examine whether there is evidence of sample selection bias. Sample selection bias is a major threat to identification with the RD design, because the estimated effect may reflect the change in the composition of sample, not the effect from the policy adoption itself.

[20] For this outcome, we estimate the specification with the prescription level data with one prescription as one observation.

In our context, if the sample selections before and after age 65 cut-off are different for the sample before 2006 and after 2006, it will bias our estimates of DD-RD specification. Figure 2.3 shows both scatter plots and fitted cubic age polynomial regression results in order to validate the null hypothesis of continuity of the age profile at the age 65 cutoff for the 2002-04 sample and 2006-09 sample. If patients were less likely to go to physicians' office just before they become eligible for the Part D coverage, and more likely to go after they become eligible, our estimates would suffer from selection bias. Figure 2.3 supports our assumption and shows no sign of such bias. There is not much difference in visit density at the age 65 cutoff between the 2002-04 sample and the 2006-09 sample. As a matter of fact, patients have little incentive to change their frequency of visits to physicians' office because elderly patients are always eligible for Medicare coverage of physician visits, while non-elderly patients are never eligible for Medicare coverage of physician visits.

Second, we check whether baseline covariates are smoothly distributed across the age 65 cutoffs for both samples before and after 2006 as a further test for another possible type of selection bias. We estimate a DD-RD specified model with the baseline covariates as outcome variables. Results are summarized in Table 2.7. All but one variable show no differential jump at the age 65 cutoffs between the two time periods. The coefficient for the race indicator, i.e., the nonwhite dummy, is only significant at 10%, and exclusion of this variable does not affect our basic results in the main analyses. These results further validate our assumption for the DD-RD specification and reduces the possibility of omitted variable bias.

Next, we test the robustness of our main results by estimating the DD-RD model using different bandwidths with and without baseline covariates. Results are summarized in Table 2.3. We control for the different polynomial functions of age in the samples with different bandwidths. In the first column, we control the linear age function for the sample with a bandwidth of one year on each side of the age cutoff. In the second and third column, we use quadratic and cubic age functions for the sample with a bandwidth of two years on each side. The fourth column shows the result with a cubic age function for a bandwidth of three years. The fifth and sixth column shows results with quadruple age functions and bandwidths of five and six years on each side of the age 65 cutoff, respectively. The regression coefficients for the number of prescription

drugs and the number of generic drugs, with or without baseline covariates, are all significantly positive, with magnitude similar to those shown in Table 2.1. We conclude that adoption of Part D causes physicians to prescribe more prescription drugs, especially more generic drugs for their elderly patients when those patients turn 65.

Finally, we conduct an extensive set of placebo tests for the main analysis sample (age 60-69), and for samples with varying bandwidths, to make sure that our results represent a causal relationship instead of spurious correlations between Part D and outcomes. We set age 64 as the cutoff and estimated the DD-RD specification with and without baseline covariates on all samples. Results are summarized in Table 2.4. The results are statistically significant for only three out of thirty two specifications, and the levels of significance are only at the 10 percent. This placebo test further demonstrates the robustness of our main results.

2.8 Patient Utilization, Expenditure and Health Outcomes for Other Medical Care Services

In this section, we extend the analysis from the early stages of the effect of Part D policy on to later stages. The complete chain of how Part D affects the utilization of prescription drugs starts when physicians change their prescribing patterns, and continues to where patients fill their prescriptions at the pharmacy. Examination of the effect of Part D on patients' actual utilization of and expenditure on prescription drugs will give us an indication of whether patients comply with the prescription or not, and of how large the effect of Part D on drug costs is. A later stage in the chain is how elderly patients' expenditure on other health care services, such as inpatient, outpatient, and emergency room (ER) visits and health outcomes change after patients change their consumption of prescription drugs. We are able to get measures of utilization of and expenditures on prescription drugs as well as expenditures on other medical care services from MEPS data, as well as patients' inpatient health outcomes from NIS-HCUP data. We employ the same DD-RD specification to examine the effect of Part D policy on these outcomes.[21]

We first examine the effect of Part D on patients' actual utilization of and expenditure for

[21] We perform Two-Part regression for MEPS data with logged second part outcomes. Results are summarized in Appendix Table $C.6$ and Appendix Table $C.7$. The significance of results from OLS regression still hold and the calculated marginal effects are similar with what we have from OLS regressions.

prescription drugs; these results are shown in Table 2.5. Although we don't have specific date of birth and date of visit in the MEPS data, we are still able to control for a polynomial function of age in years. We get consistent results for all outcomes controlling for either a linear or quadratic function of age. The number of prescription drugs increases by 8.3 to 11.8 scripts per year on average, which means patients increase their utilization of prescription drugs by 39-55 percent when they turn age 65 after the adoption of Part D compared to before the adoption. The total expenditures on prescription drugs per individual per year and the expenditures on prescription drugs from Medicare sources increase at the age 65 cutoff after 2006, although the coefficients for the former outcome are not precisely estimated. OOP cost per script decreases slightly, and the sign and magnitude of estimated effects are consistent with those in previous literature. However, our effects are not significant. The insignificance of our results could partly result from insufficient information in the public use version of the MEPS data to precisely estimate age profiles and time trends of outcomes. We also find Part D is associated with decreases in expenditures on prescription drugs from Medicaid and private sources.

We estimate the effect of Part D on expenditures on other medical care services with MEPS data using the same specifications. Regression results are summarized in Table 2.6. We find no evidence that Part D significantly affected total medical expenditures, expenditures for ER visits, inpatient visits, outpatient visits, office visits, or self reported health status.

Table 2.8 presents results with stratified samples according to dual eligibility status, sex and poverty categories. Previous research suggested that certain groups of Medicare beneficiaries are more likely to be affected by Part D policy, such as dually eligible beneficiaries. Dual-eligible beneficiaries were covered by Medicaid prescription drug coverage before 2006, and they are automatically transferred to Part D plans after 2006. They are less affected by the adoption of Part D policy than others because they are more likely to have coverage for drugs before 2006 compared to their non-dual eligible counterparts. After stratifying the sample according to whether the individual is a dual or non-dual eligible individual and estimating effects separately, we find significant results for the non-dual eligible group, but not the dual eligible group. The other possibility is low-income beneficiaries. As we discussed earlier, low-income beneficiaries are eligible for subsidies, which means they pay zero or reduced premiums, and they pay reduced

co-pay for prescription drugs which they do get. We run DD-RD regression on the stratified sample, and find only significant results for low-income and near poor individuals.[22]

Table 2.9 summarizes regression results from NIS-HCUP dataset. After controlling for a cubic polynomial function of age, we do not find significant effects of Part D on the PQI measures, in-hospital mortality, or charges or length of stay. This indicates that Part D does not have a significant impact on inpatient PQIs or resource use.

2.9 Conclusion and Discussion

The Medicare Part D program was created since prescription drugs were proven to be more and more effective in treating diseases, while drug expenditures were increasing quickly and out-of-pocket cost for drugs was high. Part D decreases the percentage of elderly individuals without drug coverage. It also lowers the cost of prescription drugs for others who before only had access to plans with higher OOP cost. In this paper, we examined whether physicians will prescribe more prescription drugs and more generic drugs for their elderly patients after 2006. We also examined the change in other practice patterns such as number of tests ordered, the number of OTC drugs prescribed, the number of brand-name drugs prescribed, the age of active ingredients in prescription drugs, and the time spent with the physician during each visit to physicians' office.

To estimate the effect of Part D, we employed a new empirical strategy combining the DD and RD designs. Specifically, this new technique allows us to compare discrete jumps in outcomes at the age 65 cutoff for the sample of patients who visited a physician's office during in 2006-09 with that for the sample of patients visiting physicians' office in 2002-04. The difference between these two discontinuities is our estimate of Part D's effect.

We find that Part D is associated with a 33% increase in the number of prescription drugs, and a 55% increase in the number of generic drugs prescribed by physicians to their elderly patients, a larger effect than those found by other Part D related studies. We think this is primarily because we correct for internal validity issues caused by confounding treatments that likely bias previous results. We did not find evidence of changes in other practice behaviors. However, we do find

[22]In MEPS data, low-income group is individuals with income between 125% to less than 200% of federal poverty line, near poor group is individuals with income between 100% to less than 125% of federal poverty line.

evidence for the existence of an anticipatory effect in 2005, the year before the implementation of Part D policy. We perform a series of sensitivity analyses in order to support the validity of our specification assumptions and the robustness of our results.

We believe the provision of formularies is one of many mechanisms through which Part D policy leads to more prescriptions for generic drugs. Part D plans encourage the utilization of generic drugs in each drug class by putting them on a tier with lower copayments. A recent report by the Kaiser Family Foundation shows that the actual net Part D spending has been about 30 percent lower than the initial projections made by the Congressional Budget Office (CBO) in 2003 (Hoadley, 2012), and the report points out that higher utilization of generic drugs was one of many explanations for this phenomenon.

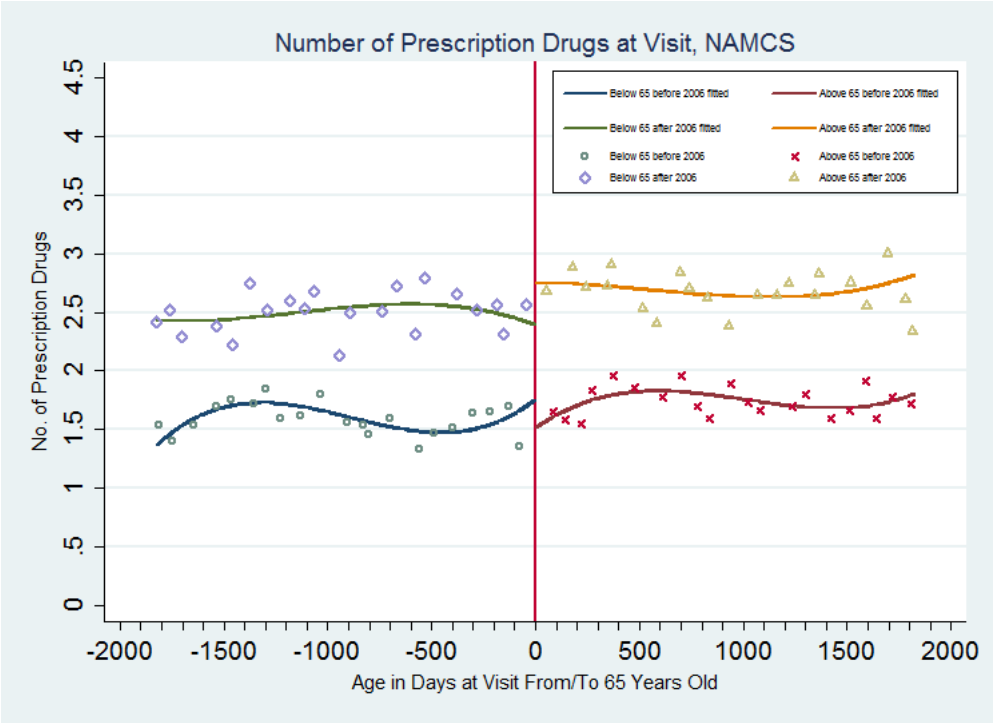
Next, we adopt the same specification to study the impact of Part D on the utilization of and expenditure on prescription drugs that were actually purchased by individuals. We find suggestive evidence that Part D resulted in a 38% increase in the number of prescription drugs purchased. Results also suggest that expenditures on prescription drugs increased but the OOP payment per prescription decreases for elderly patients, but neither effect is precisely estimated.

We also study the effect of Part D on expenditures for other medical care services and health outcomes for hospital patients. Expenditures for all medical services and for outpatient visits increase when we add higher order polynomial functions of age as controls into the DD-RD specification. However, we find no consistent evidence overall that shows significant changes in expenditures for inpatient, outpatient or ER visits using various specifications; Nor did we find any quality-improving effects of Part D on inpatient health outcomes as measured by mortality, charge, and PQIs, results consistent with those of Kaestner and Khan (2012) and Engelhardt and Gruber (2011).

Taken together, our results suggest that Part D resulted changed physicians' prescribing patterns by prescribing more prescription drugs and generic drugs, and by prescribing fewer OTC drugs. However, Part D did not significantly affect expenditures for other health care services or health outcomes. Thus, it may be then that the net impact of this program is fairly small compared to its costs. However, we note a caveat to our findings. We did not examine the impact of Part D policy on the supply side. Pharmaceutical companies might respond to the adoption of

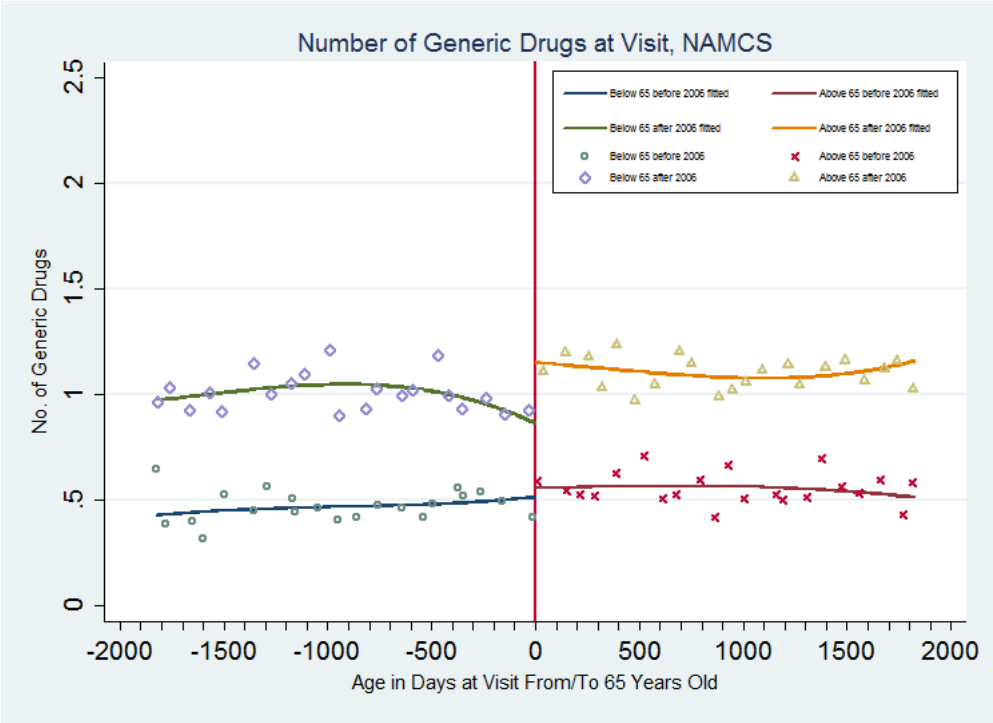
Part D by changing their marketing strategies; this mechanism may be responsible for changes in physician prescribing behavior for the elderly relative to the near elderly beginning in 2006 rather than physicians acting as agents for patients. Additionally, the pharmaceutical markets may also experience changes in competitiveness among different drug classes, thus leading to changes in the price of some prescription drugs (Duggan and Morton, 2010). Disentangling the possible reasons for changes in physician prescribing behavior for elderly relative to nonelderly patients beginning in 2006 is a possible direction for the future research.

Figure 2.1: Number of Prescription Drugs Prescribed by Physicians at Each Visit, NAMCS



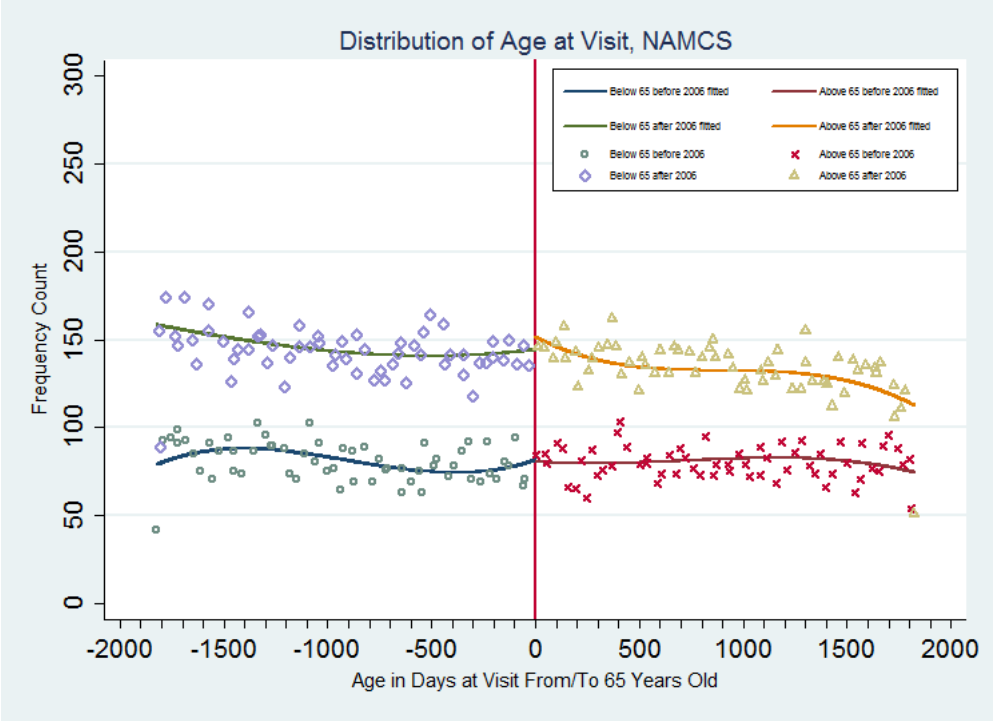
Samples are based on data from the National Ambulatory Medical Care Survey (2002-2004 and 2005-2009). The estimated discontinuities (and standard errors) at age 65 and the fitted lines are from a regression with a cubic polynomial in age fully interacted with a dummy for age greater than or equal to 65 and a dummy for year in or after 2006. Points represent means for people in each age cell (measured in quarters).

Figure 2.2: Number of Generic Drugs Prescribed by Physicians at Each Visit, NAMCS



Samples are based on data from the National Ambulatory Medical Care Survey (2002-2004 and 2005-2009). The estimated discontinuities (and standard errors) at age 65 and the fitted lines are from a regression with a cubic polynomial in age fully interacted with a dummy for age greater than or equal to 65 and a dummy for year in or after 2006. Points represent means for people in each age cell (measured in quarters).

Figure 2.3: Average Number of Visits to Physicians' Office for Age in Month Cells, NAMCS



Samples are based on data from the National Ambulatory Medical Care Survey (2002-2004 and 2005-2009). The estimated discontinuities (and standard errors) at age 65 and the fitted lines are from a regression with a cubic polynomial in age fully interacted with a dummy for age greater than or equal to 65 and a dummy for year in or after 2006. Points represent means for people in each age cell (measured in quarters).

Table 2.1: Regression Results for Prescribing Pattern, NAMCS

	Number of Rx Drugs					Number of Generic Drugs				
	DD-RD	Before 2006	After 2006	DD-RD		DD-RD	Before 2006	After 2006	DD-RD	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: sample without 2005										
Age 65 * Year 2006	0.6063*	-0.2858	0.2918*	0.5625**	0.6206**	0.2596*	0.0249	0.2652***	0.2556**	0.3182**
	[0.310]	[0.226]	[0.155]	[0.267]	[0.310]	[0.137]	[0.099]	[0.086]	[0.127]	[0.155]
Covariates	No	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes	Yes
Order of Polynomial	3	3	3	3	4	3	3	3	3	4
AIC				113099.0	113096.7				83466.8	83465.0
Prob >Chi-Squared					0.4552					0.3920
R-squared	0.041	0.187	0.189	0.204	0.204	0.054	0.123	0.136	0.161	0.161
Observations	26,474	9,737	16,737	26,474	26,474	26,474	9,737	16,737	26,474	26,474
Panel B: sample with 2005										
Age 65 * Year 2006	0.7702***	-0.3971**	0.2918*	0.7151***	0.6280**	0.4647***	-0.1336	0.2652***	0.4249***	0.4231***
	[0.285]	[0.188]	[0.155]	[0.253]	[0.274]	[0.139]	[0.097]	[0.086]	[0.130]	[0.162]
Covariates	No	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes	Yes
Order of Polynomial	3	3	3	3	4	3	3	3	3	4
Prob >Chi-Squared				0.0356	0.9672				0.1202	0.3205
R-squared	0.035	0.190	0.189	0.203	0.204	0.050	0.124	0.136	0.158	0.158
Observations	29,775	13,038	16,737	29,775	29,775	29,775	13,038	16,737	29,775	29,775

[1] Control variables for regressions include dummy of year 2006, age 65 or over, polynomial control of age in days and their full interaction with age 65 dummy and year2006 dummy, patient sex, race, Charlson index dummies, disease category by primary diagnosis codes, visit quarter, physician specialty type, whether it is a solo practice or not, electronic medical records utilization, MSA status and dummy for revenue from Medicare patients above median.

[2] ***Significant at the 1% level (two-tail test); ** Significant at the 5% level (two-tail test); * Significant at the 10% level (two-tail test).

Table 2.2: Regression Results for Other Practicing Pattern, NAMCS

	Any Rx Drugs	Number of Brand	Number of OTC	Number of Drugs	Number of Procedures	Time Spent	Age of Drugs
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Age 65 * Year 2006	0.0647	0.3071	-0.1560*	0.4194	-0.0521	2.8923	4.8608
	[0.066]	[0.226]	[0.093]	[0.304]	[0.123]	[1.788]	[9.654]
Order of Polynomial	3	3	3	3	3	3	3
AIC	30915.28	99867.61	57640.24	120068.3	60864.09	211109.5	N/A
R-squared	0.157	0.135	0.095	0.216	0.322	0.070	N/A
Observations	26,474	26,474	26,474	26,474	26,474	26,474	54,132
Age 65 * Year 2006	-0.0273	0.3024	-0.2363**	0.3821	0.0059	4.3320	-9.6389
	[0.077]	[0.272]	[0.115]	[0.342]	[0.150]	[2.748]	[11.519]
Order of Polynomial	4	4	4	4	4	4	4
AIC	30912.15	99866.19	57640.03	120066.2	60862.56	211111.8	N/A
Prob >Chi-Squared	0.3247	0.8020	0.6028	0.4920	0.4211	0.6751	N/A
R-squared	0.158	0.136	0.095	0.216	0.322	0.071	N/A
Observations	26,474	26,474	26,474	26,474	26,474	26,474	54,132

[1] Control variables for regressions include dummy of year 2006, age 65 or over, polynomial control of age in days and their full interaction with age 65 dummy and year2006 dummy, patient sex, race, Charlson index dummies, disease category by primary diagnosis codes, visit quarter, physician specialty type, whether it is a solo practice or not, electronic medical records utilization, MSA status and dummy for revenue from Medicare patients above median.

[2] ***Significant at the 1% level (two-tail test); ** Significant at the 5% level (two-tail test); * Significant at the 10% level (two-tail test).

Table 2.3: Robustness Check, Different Bandwidths and Specifications, NAMCS

	Age 63-66	Age 62-67		Age 61-68	Age 59-70	Age 58-71
	(1)	(2)	(3)	(4)	(5)	(6)
Number of Rx Drugs						
Panel A: without controls	0.4998*	0.5079*	0.6942*	0.6831**	0.8397**	0.5938*
	[0.269]	[0.293]	[0.390]	[0.325]	[0.358]	[0.317]
R-squared	0.045	0.041	0.042	0.042	0.041	0.042
Panel B: with controls	0.4597*	0.5124**	0.5834*	0.6636**	0.7835**	0.5872**
	[0.235]	[0.257]	[0.336]	[0.284]	[0.309]	[0.277]
R-squared	0.216	0.210	0.211	0.207	0.201	0.198
Number of Generic Drugs						
Panel A: without controls	0.2513**	0.2520*	0.3397*	0.3501**	0.3272**	0.2613*
	[0.125]	[0.133]	[0.182]	[0.150]	[0.159]	[0.136]
R-squared	0.053	0.052	0.053	0.055	0.055	0.058
Panel B: with controls	0.2405**	0.2538**	0.2948*	0.3453**	0.3130**	0.2658**
	[0.112]	[0.125]	[0.174]	[0.140]	[0.148]	[0.129]
R-squared	0.165	0.163	0.164	0.164	0.158	0.158
Order of Polynomial	1	2	3	3	4	4
Observation	10,538	15,772	15,772	21,072	31,898	37,400

[1] Control variables for regressions include dummy of year 2006, age 65 or over, polynomial control of age in days and their full interaction with age 65 dummy and year2006 dummy, patient sex, race, Charlson index dummies, disease category by primary diagnosis codes, visit quarter, physician specialty type, whether it is a solo practice or not, electronic medical records utilization, MSA status and dummy for revenue from Medicare patients above median.

[2] ***Significant at the 1% level (two-tail test); ** Significant at the 5% level (two-tail test); * Significant at the 10% level (two-tail test).

Table 2.4: Robustness Check, Main Outcome With Placebo Cut-Off, NAMCS

	Age 63-66	Age 62-67		Age 61-68	Age 60-69		Age 59-70	Age 58-71
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Number of Rx Drugs								
Panel A: without controls	-0.1406	-0.4182	-0.3096	-0.5411	-0.3022	-0.7190*	-0.3412	-0.5498
	[0.301]	[0.344]	[0.406]	[0.363]	[0.332]	[0.393]	[0.373]	[0.349]
R-squared	0.043	0.041	0.042	0.041	0.040	0.041	0.041	0.042
Panel B: with controls	-0.2334	-0.4394	-0.4069	-0.5363	-0.4227	-0.6650*	-0.4499	-0.6086*
	[0.272]	[0.321]	[0.382]	[0.346]	[0.314]	[0.376]	[0.358]	[0.329]
R-squared	0.216	0.210	0.210	0.207	0.204	0.204	0.201	0.198
Number of Generic Drugs								
Panel A: without controls	-0.0638	-0.1631	-0.0543	-0.1497	-0.1586	-0.1697	-0.1563	-0.2086
	[0.146]	[0.167]	[0.213]	[0.189]	[0.157]	[0.203]	[0.181]	[0.168]
R-squared	0.052	0.052	0.053	0.055	0.054	0.054	0.055	0.058
Panel B: with controls	-0.0842	-0.1489	-0.0820	-0.1325	-0.1873	-0.1300	-0.1657	-0.2199
	[0.139]	[0.162]	[0.206]	[0.183]	[0.154]	[0.202]	[0.178]	[0.164]
R-squared	0.164	0.163	0.164	0.164	0.161	0.161	0.158	0.158
Order of Polynomial	1	2	3	3	3	4	4	4
Observation	10,538	15,772	15,772	21,072	26,474	26,474	31,898	37,400

[1] Control variables for regressions include dummy of year 2006, age 65 or over, polynomial control of age in days and their full interaction with age 65 dummy and year2006 dummy, patient sex, race, Charlson index dummies, disease category by primary diagnosis codes, visit quarter, physician specialty type, whether it is a solo practice or not, electronic medical records utilization, MSA status and dummy for revenue from Medicare patients above median.

[2] ***Significant at the 1% level (two-tail test); ** Significant at the 5% level (two-tail test); * Significant at the 10% level (two-tail test).

Table 2.5: Regression Results for Utilization and Expenditure of Prescription Drugs, MEPS

	Number of Prescription Drugs	Expenditure for Prescription Drugs	OOP cost per Prescription	Prescription Expenditure from Medicare	Prescription Expenditure from Medicaid	Prescription Expenditure from Private
	(1)	(2)	(3)	(4)	(5)	(6)
Age 65 * Year 2006	8.2893*	727.5529	-0.8345	986.4728***	-66.0228	-438.2857
	[4.700]	[500.927]	[6.005]	[289.892]	[79.854]	[284.976]
Covariates	Yes	Yes	Yes	Yes	Yes	Yes
Order of Polynomial	1	1	1	1	1	1
AIC	160112.3	319625.1	170499.2	298424.3	285938.3	299065.2
R-squared	0.281	0.153	0.025	0.115	0.087	0.077
Observations	16,931	16,931	16,931	16,931	16,931	16,931
Age 65 * Year 2006	11.8794**	893.2102	-3.4908	874.4338***	-70.9477	-274.3548
	[5.200]	[556.599]	[6.741]	[311.782]	[109.848]	[320.956]
Covariates	Yes	Yes	Yes	Yes	Yes	Yes
Order of Polynomial	2	2	2	2	2	2
AIC	160117	319630.2	170502.6	298427.2	285938.3	299062.1
Prob >Chi-Squared	0.6410	0.8909	0.5597	0.5166	0.2378	0.0165
R-squared	0.281	0.153	0.026	0.115	0.087	0.078
Observations	16,931	16,931	16,931	16,931	16,931	16,931

[1] Control variables for regressions include dummy of year 2006, age 65 or over, polynomial control of age in days and their full interaction with age 65 dummy and year2006 dummy, patient sex, race, poverty categories, census region dummies, education level categorical dummies, marital status dummies, prior medical conditions and MSA status.

[2] ***Significant at the 1% level (two-tail test); ** Significant at the 5% level (two-tail test); * Significant at the 10% level (two-tail test).

Table 2.6: Regression Results for Other Medical Expenditure, MEPS

	Total Expenditure	**E**R Expenditure	**I**npatient Expenditure	**O**utpatient Expenditure	**O**ffice Visit Expenditure	**G**ood Health or Better
	(1)	(2)	(3)	(4)	(5)	(6)
Age 65 * Year 2006	683.9062	16.3795	-252.5719	511.5787	-573.8796	-0.0266
	[2768.729]	[197.938]	[2215.952]	[468.637]	[711.422]	[0.049]
Order of Polynomial	1	1	1	1	1	1
AIC	376866.5	292183.8	366893.4	326912.3	336297.6	8108.994
R-squared	0.077	0.010	0.032	0.015	0.032	0.162
Observations	16,931	16,931	16,931	16,931	16,931	16,931
Age 65 * Year 2006	4618.1934*	204.0077	2023.7823	1443.4873*	-514.2386	-0.0780
	[2731.214]	[232.573]	[1756.791]	[799.135]	[756.838]	[0.054]
Order of Polynomial	2	2	2	2	2	2
AIC	376868.6	292188	366894.6	326916.6	336305.3	8107.305
Prob >Chi-Squared	0.0678	0.2366	0.1025	0.4473	0.7073	0.2190
R-squared	0.078	0.010	0.033	0.017	0.032	0.162
Observations	16,931	16,931	16,931	16,931	16,931	16,931

[1] Control variables for regressions include dummy of year 2006, age 65 or over, polynomial control of age in days and their full interaction with age 65 dummy and year2006 dummy, patient sex, race, poverty categories, census region dummies, education level categorical dummies, marital status dummies, prior medical conditions and MSA status.

[2] ***Significant at the 1% level (two-tail test); ** Significant at the 5% level (two-tail test); * Significant at the 10% level (two-tail test).

Table 2.7: Robustness Check, DD-RD Estimates for Baseline Covariates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
NAMCS data:							
	Female	Nonwhite	Charlson Index=1	Charlson Index=2	MSA Status	Solo Practice	EMR
Age 65 * Year 2006	0.0489	-0.0733*	0.0419	0.0051	0.0368	-0.0514	0.0780
	[0.073]	[0.042]	[0.034]	[0.035]	[0.045]	[0.065]	[0.066]
Covariates	No	No	No	No	No	No	No
Order of Polynomial	3	3	3	3	3	3	3
R-squared	0.002	0.002	0.003	0.001	0.004	0.005	0.075
Observations	26,474	26,474	26,474	26,474	26,474	26,474	26,474
MEPS data:							
	Female	Race: Black	Ethnicity: Hispanic	Very Poor	Near Poor	Low Income	Middle Income
Age 65 * Year 2006	-0.0741	0.0217	0.0371	0.0251	0.0014	-0.0483	0.0056
	[0.090]	[0.044]	[0.037]	[0.032]	[0.056]	[0.082]	[0.096]
Covariates	No	No	No	No	No	No	No
Order of Polynomial	1	1	1	1	1	1	1
R-squared	0.001	0.001	0.001	0.005	0.005	0.001	0.007
Observations	16,931	16,931	16,931	16,931	16,931	16,931	16,931
	High Income	High School	Bachelor Degree	Post-Bachelor	Asthma	High BP	Angina
Age 65 * Year 2006	-0.0229	-0.0802	0.0387	0.0026	0.0752	-0.0497	-0.0365
	[0.080]	[0.093]	[0.071]	[0.071]	[0.058]	[0.087]	[0.041]
Covariates	No	No	No	No	No	No	No
Order of Polynomial	1	1	1	1	1	1	1
R-squared	0.001	0.001	0.005	0.002	0.001	0.014	0.003
Observations	16,931	16,931	16,931	16,931	16,931	16,931	16,931
	Heart Attack	Joint Pain	Stroke	Emphysema	Arthritis	Other Heart Problem	
Age 65 * Year 2006	-0.0747	-0.0916	-0.0161	-0.0145	-0.0752	-0.0869	
	[0.047]	[0.088]	[0.040]	[0.032]	[0.089]	[0.064]	
Covariates	No	No	No	No	No	No	
Order of Polynomial	1	1	1	1	1	1	
R-squared	0.005	0.002	0.003	0.005	0.011	0.015	
Observations	16,931	16,931	16,931	16,931	16,931	16,931	

[1] For regressions with NAMCS data, control variables include dummy of year 2006, age 65 or over, polynomial control of age in days and their full interaction with age 65 dummy and year2006 dummy, patient sex, race, disease category by primary diagnosis codes, physician specialty type, electronic medical records utilization, MSA status.

[2] For regressions with MEPS data, control variables include dummy of year 2006, age 65 or over, polynomial control of age in days and their full interaction with age 65 dummy and year2006 dummy, patient sex, race, poverty categories, census region dummies, education level categorical dummies, marital status dummies, prior medical conditions and MSA status.

[3] ***Significant at the 1% level (two-tail test); ** Significant at the 5% level (two-tail test); * Significant at the 10% level (two-tail test).

Table 2.8: DD-RD Estimates for Stratified Sample, MEPS

	Dual Status		Sex		Income Categories				
	Dual	Non-Dual	Female	Male	Very Poor	Near Poor	Low Income	Middle Income	High Income
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Number of Prescriptions									
Age 65 * Year 2006	-15.2461	9.1695**	6.5288	10.2342*	1.0450	23.8852	21.0178*	1.1769	8.6730
	[29.416]	[4.566]	[6.380]	[5.842]	[17.543]	[19.917]	[12.240]	[8.848]	[5.612]
Order of Polynomial	1	1	1	1	1	1	1	1	1
R-squared	0.310	0.272	0.280	0.291	0.300	0.372	0.313	0.277	0.251
Observations	1,390	15,541	9,239	7,692	2,216	893	2,374	4,789	6,659
Prescription Expenditure									
	Dual	Non-Dual	Female	Male	Very Poor	Near Poor	Low Income	Middle Income	High Income
Age 65 * Year 2006	-3.74e+03	940.5926*	995.2298	499.0009	-1.34e+03	3800.3844**	184.8155	445.5545	1119.3192*
	[2298.864]	[513.733]	[749.760]	[573.871]	[1408.648]	[1872.400]	[1052.006]	[1125.844]	[621.143]
Order of Polynomial	1	1	1	1	1	1	1	1	1
R-squared	0.202	0.148	0.151	0.159	0.194	0.238	0.203	0.135	0.150
Observations	1,390	15,541	9,239	7,692	2,216	893	2,374	4,789	6,659

[1] Control variables for regressions include dummy of year 2006, age 65 or over, polynomial control of age in days and their full interaction with age 65 dummy and year2006 dummy, patient sex (not included in regressions in column 3 and 4), race, poverty categories(not included in regressions in column 5-9), census region dummies, education level categorical dummies, marital status dummies, prior medical conditions and MSA status.

[2] ***Significant at the 1% level (two-tail test); ** Significant at the 5% level (two-tail test); * Significant at the 10% level (two-tail test).

[3] There is a caveat associated with the stratified sample regression according to the dual status of the respondent. The incentive to be a dual, i.e. to apply for Medicaid after age 65, would have decrease after 2006 since individuals would not need to apply for Medicaid to get drug coverage. So the sample of duals would change from before to after 2006.

[4] MEPS computed poverty category by dividing CPS family income by the applicable poverty line (based on family size and composition), and then classifies respondents into one of five poverty categories: negative or poor (less than 100%), near poor (100% to less than 125%), low income (125% to less than 200%), middle income (200% to less than 400%), and high income (greater than or equal to 400%).

Table 2.9: DD-RD Estimates for NIS-HCUP Data Set

VARIABLES	Mortality	Log of length of Stay	Log of Total Charge	Number of Procedures	Diabetes (Short Term)	Perforated Appendix
Age 65 * Year 2006	0.0023	-0.0163	-0.0242	-0.0226	0.0006	0.0004
	[0.002]	[0.013]	[0.017]	[0.034]	[0.001]	[0.000]
Order of Polynomial	3	3	3	3	3	3
R-squared	0.056	0.216	0.357	0.286	0.117	0.89
Observations	4,525,332	4,525,332	4,525,332	4,525,332	4,525,332	4,525,332
VARIABLES	Diabetes (Long Term)	COPD or Asthma	Hypertension	Heart Failure	Dehydration	Bacterial Pneumonia
Age 65 * Year 2006	-0.0006	-0.0006	0.0000	-0.0001	-0.0020*	0.0004
	[0.001]	[0.001]	[0.001]	[0.000]	[0.001]	[0.001]
Order of Polynomial	3	3	3	3	3	3
R-squared	0.766	0.938	0.665	0.985	0.617	0.835
Observations	4,525,332	4,525,332	4,525,332	4,525,332	4,525,332	4,525,332
VARIABLES	Urinary Infection	Angina Without Procedure	Uncontrolled Diabetes	Diabetes with Amputation	Any of Those Conditions	
Age 65 * Year 2006	-0.0004	-0.0002	0	0	-0.0024	
	[0.001]	[0.001]	[0.001]	[0.001]	[0.002]	
Order of Polynomial	3	3	3	3	3	
R-squared	0.871	0.068	0.111	0.092	0.852	
Observations	4,525,332	4,525,332	4,525,332	4,525,332	4,525,332	

[1] Control variables for regressions include dummy of year 2006, age 65 or over, polynomial control of age in days and their full interaction with age 65 dummy and year2006 dummy, patient sex, dummy for weekend admission, dummy for emergency admission, dummy for transferred from another facilities, dummy for elective admission, dummies for hospital bed size categories, dummies for hospital ownership, dummy for urban hospital, dummy for teaching hospital, dummies for quartiles of zip code level median household income and Clinical Classifications Software (CCS).

[2] ***Significant at the 1% level (two-tail test); ** Significant at the 5% level (two-tail test); * Significant at the 10% level (two-tail test).

Chapter 3

Pay-For-Performance (P4P) Program

Analysis of P4P Program for Utilization of Preventive Care Services among Medicaid Population in United States

3.1 Introduction

Medicaid finances health care for more than one out of every five Americans; primarily eligible are low-income individuals and families. In the 1990's, Medicaid expenditures grew rapidly, at an average annual rate of 22.4% between 1988 and 1992 and 9.5% between 1992 and 1995. Beginning in the late 1990's and through the 2000's, in response to rising health care costs, many state Medicaid agencies subcontracted with managed care organizations to administrate their Medicaid program. Among states with Medicaid managed care programs, some further adopted pay-for-performance (P4P) programs in order to link their health care spending to quality and efficiency of care. Over the past ten years, twenty states adopted Medicaid managed care P4P programs and new programs and approaches are continuously being added. However, limited literature exists that studies the effect of such P4P programs. This paper examines the effect of pay-for-performance (P4P) programs on the utilization of cancer screening procedures for adults and vaccine rate for children among Medicaid populations in United States. We also address effectiveness of different incentive design of those state P4P program in terms of improving utilization of those preventive care services

In many states, the main reason to adopt P4P programs is because of the concerns about the consequences of implementations of Medicaid managed care plans. Medicaid managed care plans provide beneficiaries with enhanced coverage of preventive care services in order to improve their access to care; however, they use a capitation payment scheme, which seeks to reduce costs, rather than fee-for-service (FFS). Their effect on the quality of health care is uncertain since they can reduce unnecessary or necessary services. The number of enrollees in managed care plans continued to grow a lot in the 90's. And by 2000, over 50% of Medicaid beneficiaries in the States were insured through managed care organizations (MCOs). As a result, there were concerns among advocates for low-income and disadvantaged groups.

On the other hand, preventive care plays an important role in the health care delivery system, especially for the low-income population. For example, Hillman et al. (1999) pointed out that "childhood immunization is a cost-effective means of preventing diseases also reflects the adequacy of pediatric health care in general". Alexander and Korenbrot (1995) regards prenatal care as a means to identify mothers at risk of "delivering a preterm or growth-retarded infant" and to provide available medical interventions intended to reduce the risks of low birth weight and other adverse pregnancy outcomes. Despite the benefits of preventive care, the low-income population, Medicaid's primary target group, tends to have a lower utilization rate for preventive care services and poorer health status.

For those reasons, Centers for Medicare and Medicaid Services (CMS) started to promote quality and value-based purchasing through its Medicaid/SCHIP Quality Initiative. By CMS's definition, P4P is a "quality improvement and reimbursement methodology which is aimed at moving towards payments that create much stronger financial support for patient focused, high value care ... attempts to promote reimbursement for quality, access efficiency, and successful outcomes." (CMS, 2006). As a joint program between federal and state governments, states also adopted P4P programs for their Medicaid managed care plans by either rewarding plans with high performance or 'punishing' plans with low performance. And most states use utilization of preventive care services, such as blood lead screen rates, child immunization rates, and timeliness of prenatal care as performance measures. Kuhmerker and Hartman (2007) find that over 70 percent of existing Medicaid P4P programs operate under managed care environments. According

to their follow-up interviews with state Medicaid agencies, this number is still rising.

Past literature finds that the implementation of state P4P programs is especially effective in improving the utilization of preventive care. McMenamin et al. (2003) and Roski et al. (2003) both found a positive effect of financial incentives on a smoking cessation intervention. Rosenthal et al. (2005) performed a difference-in-difference analysis using generalized estimating equations and found improvements in cervical cancer screening, mammography, and hemoglobin A1C testing. However, little research exists devoted to the nationwide investigation of whether or not P4P programs affect the utilization of preventive care among the low-income population. Kuhmerker and Hartman (2007) summarize existing and new P4P activities in state Medicaid programs, but their study does not examine the effect of P4P programs on any health outcomes.

The purposes of our study is to assess whether or not the P4P program affects the utilization of preventive care for beneficiaries enrolled in a Medicaid managed care organization. We also want to examine if the magnitude of the effect differs depending on different types of "incentives" and "measures". Our study utilizes data from the National Health Interview Survey (NHIS), which is a national longitudinal survey data set that allows us to observe changes in utilization of adult preventive care services before and after the adoption of P4P programs in every state. Specifically, we will study the effect on the utilization of the following preventive care services: mammograms among women 40-64; colonoscopy for men or women aged 50-64; cholesterol testing; blood pressure checks; and prostate (PSA) testing. We also analyze National Immunization Survey (NIS) data, with which we further examine the impact on preventive care, specifically childhood immunization rate. Our hypotheses states that adoption of Medicaid managed care P4P programs will increase utilization of preventive care services for both adults and children, and different incentive design of P4P program impact outcomes differently.

Specifically, we will utilize difference-in-difference (DD) estimation strategy that compares changes in utilization among Medicaid beneficiaries in states adopting P4P programs, relative to those in states that fail to adopt, before and after the implementation. We expect to see a greater increase in utilization in states with P4P programs relative to others without P4P programs. We don't have information on the enrollment into managed care plans in the dataset. So in order to establish the association between the P4P program and outcomes among Medicaid

managed care population, we utilize difference-in-difference-in-difference (DDD) specification and further compares the outcome changes for states with above median and below median Medicaid managed care penetration rate. To examine the relative effectiveness of different incentive designs, we estimate the DDD specification by separating time trends for states with different incentive designs.

The rest of this paper is outlined as follows. In the second section, we introduce background and details of the P4P program and provide an in-depth review of relevant literature. In the third section, we describe the data source as well as independent and dependent variables used in the analysis. Next, we address our estimation equation and econometric method. In the following section, we present our preliminary results and perform additional estimations in order to check the robustness of basic results,. In the last section, we conclude our results and discuss future research questions.

3.2 Literature Review

Petersen et al. (2006) performed a systematic review of several studies assessing the effect of explicit financial incentives for improved performance on measures of health care quality. He/She found few empirical studies available and most focused on the utilization of preventive care among those available studies. Hillman et al. (1998) and Hillman et al. (1999) utilize randomized controlled trials to assign a single lump sum bonus to Medicaid HMOs and found no difference between treatment and control groups on physician compliance with cancer screening guidelines and with pediatric preventive care, respectively. However, Kouides et al. (1998) also conducted a randomized controlled trial on 54 solo or group practices that participated in the 1990 Medicare Demonstration Project in Rochester and surrounding Monroe County in New York. They rewarded providers in the treatment group with \$0.80-\$1.60 per shot during the 1991 influenza immunization season and found a significant improvement in the immunization rate for the treatment group as compared to the control group.

Other studies use survey data to analyze the effect of financial incentives. McMenamin et al. (2003) utilize nationwide survey data documenting the extent to which physician organizations provide support for smoking cessation interventions. They found factors of support for smoking

cessation are positively associated with financial incentives to promote smoking cessation intervention, requirements to report HEDIS data, the percentage of primary care physicians, and HMO ownership of the organization. Rosenthal et al. (2005) exploited a natural experiment related to pay-for-performance using administrative reports of physician group quality from an intervention group (California physician groups) and a comparison group (Pacific Northwest physician groups) from October 2001 through April 2004. They performed a difference-in-difference analysis examining cervical cancer screening, mammography, and hemoglobin A1c testing, and they find that compared to the control group, the treatment group demonstrated greater quality improvement for cervical cancer screening after the pay-for-performance intervention.

According to past literature, the design of P4P programs also makes a difference on outcomes. Felt-Lisk et al. (2007) found that "money talks" only works if certain supportive elements are presented in the programs. They examine five Medicaid-focused plans that offer financial incentives for improving baby care and identify key features of plans with better performance, i.e. better birth outcomes. Those key features are strong communication with providers, placing enough dollars at stake to compensate providers for their effort. Casalino et al. (2007) also pointed out that P4P programs, if not carefully designed, may have the unintended consequence of increasing racial and ethnic disparities. For instance, P4P and public reporting may adversely affect the income of physicians practicing in minority communities, therefore limiting the number of physicians practicing in those areas and the physician's ability to invest in processes to improve quality. Alternatively, P4P and public reporting may induce individual physicians or medical groups to avoid treating patients with poorer health status, who may lower their quality score if the quality measure does not adjust for patients' overall health status. Furthermore, the public report might benefit educated patients more than uneducated patients. Thus, Felt-Lisk suggests that P4P programs should use risk adjustments and stratified analysis and reward both the absolute quality score as well as improvements over time.

Our study makes several contributions to existing literature. Few studies investigate the effect of P4P programs at the national level. Kuhmerker and Hartman (2007) summarize existing and new P4P activities in state Medicaid programs. However, they do not examine the effect of P4P programs on any health outcomes. Other studies, which assess health outcomes, focus on a small

scale by using data from randomized controlled trials (Hillman et al., 1998; Hillman et al., 1999) or using state-level data (Shen, 2003). We will use national data to study the effect of P4P programs. Since most of the literature has focused on pay for performance to health care providers, such as hospitals and physicians, we analyze the effect of P4P on managed care plans.

Secondly, we take advantage of variation due to the staggered adoption of P4P in each state. By limiting the study sample to the Medicaid population, and by exploiting differential changes in the utilization of preventive care and health outcomes of states with P4P policies relative to those without P4P policies, we minimize the extent to which unobserved time-invariant state characteristics might contaminate estimates.

Additionally, we investigate the differential effect of various incentive types and measures. As discussed earlier, states can adopt different incentive types, such as bonuses, penalties, and differential reimbursement rates. They can also measure "performance" by attainment, improvement, or a hybrid of these two. In order to provide more policy relevance, our regression analysis allows us to obtain estimates of the relative strengths of each policy component.

3.3 Background

The Medicaid P4P program is a state-specific, plan-based, and performance-tracking system with assessment and feedback. Participating managed care plans receive incentives, or in some instances are "punished", based on specific performance criteria measured within a certain time horizon. Generally, P4P programs have several components, each consisting of several options that states select in order to best address their particular improvement goals.

States first choose outcomes as measurements for plans' performance. Different states select different performance measures that are best suited to address their particular improvement goals. The most popular measures used by P4P programs are the Health Plan Employer Data and Information Set (HEDIS) and HEDIS-like measures. HEDIS is a set of standardized performance measures for managed care organizations, and the measures are mainly related to preventive and primary care for chronic diseases. Examples of HEDIS measures include childhood immunization rates, timeliness of prenatal care, comprehensive diabetes care measures, use of appropriate medications for people with asthma, various types of cancer screening, and assistance with smoking

cessation. HEDIS-like measures are similar to HEDIS measures and are related to other preventive care performance. Some states target different aspects of Medicaid programs, so they use other measures, such as structural measures, cost/efficiency measures, measures based on patient experiences, and measures based on medical records. [1]

Then in order to determine whether or not the plan qualifies for an "incentive", states choose among different methods to evaluate performance of each plan. Methods include attainment, improvement, or a hybrid of these two methods, and in rare situations, peer comparison. [2] There are both advantages and disadvantages to attainment and improvement methods. While easier to implement, the attainment method discourages lower-level performers that have little chance to attain the target level. Such performers may opt-out from the program if participation is not mandatory. Also, plans that have already achieved the adequate level of performance have little incentive for further improvement. As for the improvement method, a poor performer might improve his performance from low-level to medium-level, but a good performer has limited potential to improve since he is already performing at a high-level and incurs a higher marginal cost to further improve. Thus, a hybrid method of attainment and improvement provides a better balance of the advantages and disadvantages of both methods. However, different states adopt different method best suited their own situation. For example, to ensure a basic level of attainment is reached, Nevada established a bottom level of performance, beneath which no incentive payment is provided. Massachusetts is considering using incentives to reward attainment of specified levels of performance as well as improvement.

Lastly, states choose different incentive designs to reward plans with high performance, or punish plans with low performance. Incentive designs include one or more of these types: differential reimbursement rates, bonuses, grants to penalties, withholds and public reporting. Differential reimbursement is a change in the ongoing reimbursement rate or fee to reflect achievement of or

[1] Structural measures, the second most common type of performance measure, relate to a specific status or activity, such as accreditation status, health information technology adoption, being open on weekends, or the time it takes to get an appointment. They are not direct measures of quality and outcomes, but they assess patients' access to care and if the plan provides quality care. Cost/efficiency measures evaluate overall savings in the present period as compared to a prior period, for a given subpopulation of Medicaid beneficiaries.

[2] Attainment is an established level of performance, which is used to evaluate the actual performance of each plan and decide if the plan qualifies for an incentive. Improvement is another assessment method that rewards improvement over a previous baseline or performance level. States that use a hybrid method of attainment and improvement could set up both a specific level of performance and gradations of improvement, and a plan has to meet both criteria to get the rewards.

improvement in the required performance levels. Auto-assignment rewards high-quality providers by assigning beneficiaries who fail to choose a managed care plan or provider to them. Penalties are one-time requirements for plans or providers to repay the state (or to have the state recoup previously paid funds) to reflect the failure to meet required performance levels. Withholds are performance-related funding that Medicaid programs set aside and do not return to providers until providers demonstrate that a particular standard has been met. We categorize these inventive designs into three types: positive incentive type which includes differential reimbursement rate, negative incentive type which includes penalties and withholds as well as non-financial incentive type which includes both auto-assignment and public reporting.

Table 3.1 summarizes all P4P programs for Medicaid managed care plans in the United States.[3] We list the adoption year, incentive type, whether the state targets the improvement of preventive care, and whether there has been an evaluation for the program. From the table, we can see that nineteen out of fifty states adopted P4P programs for Medicaid managed care plans up until 2009. Nine of those P4P programs focus on improving utilization of preventive care; eight has been done some form of evaluations. For the incentive types, each state adopted one to three types. Sixteen states adopted differential reimbursement rate, six adopted auto-assignment, while five have withholds some funding as an incentive type. There are also a lot of variations in the time of adoption that we can exploit in order to estimate the impact of P4P programs on outcomes.

3.4 Data

We compile our samples from three different sources. We use National Health Interview Survey (NHIS) data to estimate the impact of Medicaid managed care P4P programs on the utilization of cancer screening procedures and other preventive care services for adults. The NHIS, conducted annually by the National Center for Health Statistics (NCHS), is a cross-sectional, household interview survey that includes a nationally representative sample of the civilian, non-institutionalized population in the United States.

The dependent variables we examine are extracted from HEDIS measures. We use are mam-

[3] 2009 National Summary of State Medicaid Managed Care Programs: Program Description as of June 30, 2009.Available online `https://www.cms.gov/MedicaidDataSourcesGenInfo/downloads/2009NationalSummaryReport.pdf` (CMS, 2009)

mogram exam among women 50-64, colonoscopy for men or women aged 50-64, cholesterol exam for men or women aged 50-64, cholesterol check exam for respondents aged 40-64, blood pressure check for respondents aged 40-64, Pap smear test for women aged 40-64 and adult hepatitis B vaccine for respondents aged 18-64. All those outcomes will be dummies, 1/0 indicates the person has done it or not in the survey year. NHIS only has related survey questions in certain years and we summarized survey years for each outcome in Table 3.2. Therefore, our analysis will use different analysis samples for different outcomes. [4] Fortunately, most states adopted P4P policies around our sample period. According to the summary of the state adoption dates in Table 3.1, we can exploit time variation in order to estimate the effect of P4P programs. The NHIS data also includes information on patients' demographic characteristics as gender, age, race, Ethnicity, education level, health status and any limitation on activity level. We control for these variables as baseline covariates in our specification.

We then analyze National Immunization Survey (NIS) (1999-2010) to study the impact of Medicaid managed care P4P program on the childhood immunization rate. The NIS is conducted jointly by NCIRD and the National Center for Health Statistics (NCHS), Centers for Disease Control and Prevention. It is a list-assisted random-digit-dialing telephone survey followed by a mailed survey to childrens immunization providers that began data collection in April 1994 to monitor immunization coverage for children aged 19 to 35 months.

We study the impact of P4P program on all performance measures from HEDIS in the paper, including up-to-date 4 doses of diphtheria-tetanus toxoids-pertussis vaccine (DTP), up-to-date 3 doses of poliovirus vaccine (Polio), up-to-date 1 does of measles-mumps-rubella vaccine (MMR), up-to-date 3 does of Haemophilus influenzae type B vaccine (Hib), up-to-date 3 does of hepatitis B vaccine (Hep B), up-to-date 1 doe of Varicella at 12+ months (Varicella), up-to-date 4:3:1 vaccine series (DTP, Polio, MMR), up-to-date 4:3:1:3:3 vaccine series (DTP, Polio, MMR, Hib and Hep B) and the most complete vaccine series, up-to-date 4:3:1:3:3:1 vaccine series (DTP, Polio, MMR, Hib, Hep B and Varicella).

There is no specific indicator for insurance coverage in NIS data. So we utilize a variable called family income as percentage of federal poverty line, and refer to the Medicaid eligibility

[4] Please check Table 3.3 for sample sizes of each analysis.

criteria for each state in each year, in order to determine Medicaid eligibility status. And we only select observations with verified information either from a shot card or from a provider into our final sample. The final sample size varies from outcome to outcome.[5] We control for child characteristics such as age, birth parity, race, Ethnicity and sex, as well as mother characteristics such as number of children in the household, education level, mobility and age in our specification.

Our final data source is Medicaid managed care penetration rate for each state and each year from CMS. We merge this rate into NHIS and NIS datasets in order to identify whether the respondent lives in a state with above or below median Medicaid managed care penetration level.

3.5 Research Design and Methods

We now discuss the empirical strategy we use to address our research questions. In this section, we will present the estimation strategy for estimating the impact of Medicaid managed care on utilization of preventive care services, as well as the relative effectiveness of different incentive designs of various states P4P program.

3.5.1 Main Specifications

We use difference-in-difference (DD) approach to address our first research question. A DD approach normally defines Medicaid beneficiaries living in a state that adopted a P4P program as the treatment group, and those Medicaid beneficiaries living in a state that does not adopt a P4P program as the control group. The idea is to compare the outcome changes for the treatment group before and after the treatment, i.e., the introduction of P4P programs, with outcome changes for those of the control group. In the context of this paper, we would estimate the effect of policy by exploiting the relative changes in utilization of various preventive care services by Medicaid beneficiaries who live in a state with P4P program before and after the adoption of the program, compared to those Medicaid beneficiaries who live in a state without P4P program.

Using outcomes of patient i in state j in year t as dependent variable, we estimate the following

[5] Please check Table 3.4 for sample sizes of each analysis.

equation:

$$Outcome_{ijt} = \beta_0 + \beta_1 P4P_j + \beta_2 Year_t + \beta_3 P4P_j * Post_t + \beta_4 X_i + \beta_5 Y_j + \beta_6 State_j + \epsilon_{ijt}, \quad (3.1)$$

where $Outcome_{ijt}$ is one of sixteen measures for patient i in state j in year t. We use $P4P_j$ to indicate a respondent who lives in a state with P4P program for its Medicaid managed care program, and $Post_t$ to indicate the time period after the adoption of P4P program for that state. We control for X which is a vector of personal characteristics, $Year_t$ which is a vector of year dummies and $State_j$ which is a vector of state dummies.[6] State dummies control for any time-invariant trends in outcomes in different states, and year dummies control for any idiosyncratic shocks to outcomes in all the states. $P4P_j * Post_t$ is the variable of interest. The coefficient β_3 measures the effect of the P4P program on the outcome variables by comparing the outcome changes of treatment group with those of control group. In order to account for the design of survey data, all coefficients and standard errors are weighted statistics using survey instruments.

The second empirical approach we use to analyze the impact of P4P program is difference-in-differece-in-difference (DDD) specification.

Since Medicaid managed care P4P programs are meant for beneficiaries covered by managed care plans, it would be a nature practice to estimate the outcome changes for those covered by Medicaid managed care plans with outcomes changes for those covered by Medicaid non-managed care plans. However, there are certain disadvantage associated with this method. First, Medicaid beneficiaries who enrolled into managed care plans are different from those who enrolled into non-managed care plans, for example, sicker beneficiaries may be more likely to choose FFS over managed care if they have a choice. Also, beneficiaries who enrolled into managed care plans and those who enrolled into FFS plans are likely to live in different geographic locations; and those different residence locations might have different economic and other conditions that may be correlated with the health status of their Medicaid beneficiaries. These may all lead to biased estimates for the effect of P4P program.

[6]Personal characteristics include gender, age, race, Ethnicity, education level, health status and any limitation on activity level for the analysis of NHIS data; and child characteristics such as age, birth parity, race, Ethnicity and sex, as well as mother characteristics such as number of children in the household, education level, mobility and age for the analysis of NIS data.

In order to analyze the impact of P4P program for Medicaid managed care plan, we add the third tier of difference by introducing the indicator of being in a state with above median Medicaid managed care penetration rate. This allows us to estimate the impact of P4P policy for those who are more likely to be covered by Medicaid managed care plans without introducing the self selection bias we discussed earlier. [7] Specifically, we estimate the following DDD specification using the sample of Medicaid beneficiaries:

$$Outcome_{ijt} = \beta_0 + \beta_1 P4P_j + \beta_2 Year_t + \beta_3 Above_{jt} + \beta_4 P4P_j \times Post_t \times Above_{jt} + \beta_5 P4P_j \times Year_t + \beta_6 P4P_j \times Above_{jt} + \beta_7 Year_t \times Above_{jt} + \beta_8 X_i + \beta_9 State_j + \epsilon_{ijt}, \quad (3.2)$$

where $Above_{jt}$ is an indicator for states with above median Medicaid managed care penetration rate.

The key variable is $P4P_j \times Post_t \times Above_{jt}$ because it identifies outcomes of Medicaid beneficiaries in states with a P4P program and above median Medicaid managed care penetration rate after P4P has been implemented. Its coefficient, β_4, is thus an estimate of the change in outcomes for Medicaid beneficiaries in P4P state with high penetration rate after the adoption of P4P program, compared to their previous outcome before adoption, relative to changes in outcomes over time for P4P states with below median penetration rate, relative to the same outcome difference over time for non-P4P states.

3.5.2 Alternative Estimation

In order to examine the differential effect of various incentive types, we allow the time trend to vary with different policy components. Coefficients for these interactions will indicate which types of incentives has a greater effect on the outcomes of interest.

$$Outcome_{ijt} = \beta_0 + \beta_1 P4P_j + \beta_2 Year_t + \beta_3 P4P_j \times Post_t \times A_j + \beta_4 P4P_j \times Post_t \times B_j + \beta_5 P4P_j \times Post_t \times C_j + \beta_6 X_i + \beta_7 State_j + \epsilon_{ijt}, \quad (3.3)$$

[7] We examined the variation of state-level Medicaid managed care penetration rate in Appendix Table$C.8$ and found some variation of this indicator in most states along years

where A_j, B_j and C_j denote dummies for using positive financial incentives, negative financial incentives and non-financial incentives respectively. The coefficients β_3, β_4 and β_5 are estimates for impact of P4P programs on outcomes with different incentive designs.

3.6 Results

3.6.1 Results for Main Specification

Table 3.3 presents descriptive statistics for the outcomes used in the estimations, for the entire sample and for the states with adoption of P4P program, before and after policy adoption, as well as for the states without adoption of P4P program.

The DD and DDD estimation results for analysis of NHIS data are shown on Table 3.4: Panel A shows the DD results, Panel B shows DDD results. All estimations include the personal characteristics described in data section as well as year, and state fixed effects. Sample weights were used and standard errors accounted for the complex design of the surveys.

The results in Panel A suggest that among those on Medicaid, state adoption of a P4P program is positively associated with the likelihood that adults have received several preventive care services, including mammograms, blood pressure checks and Pap smears. Specifically, P4P adoption is associated with a 5.2 percentage point (about 6 percent) increase in the probability that women aged 50-64 has received a mammogram in the past year and about a 5.6 percentage point (13.2 percent) increase in the probability that individuals aged 50-64 have had their cholesterol checked in the past five years. Panel B shows that these beneficial effects are larger in states with higher Medicaid managed care penetration rates. There is a 16 percentage point (36.6 percent) increase in the probability that individuals aged 50-64 have had their cholesterol checked, 11.2 percentage point (24.5 percent) increase in the probability that individuals aged 50-64 have had their blood pressure taken and 9 percentage point (25.9 percent) increase in the probability that women aged 40-64 has done a Pap smear exam.

The DD and DDD estimation results for analysis of NIS data are shown on Table 3.5: Panel A shows the DD results, Panel B shows DDD results. Again we control for children and mother characteristics mentioned in the data section, and we account for the survey design by using all

survey instruments in the regression. Results in Panel A confirms our hypotheses that adoption of state P4P program is associated with higher utilization of childhood immunization, an important form of preventive care services among children. Specifically, P4P adoption is associated with about a 1.1, 1.6, and 2.3 percentage point (1, 2, and 3 percent, respectively) increase in the probability that children on Medicaid are up-to-date on the 3 doses of Haemophilus influenza type B vaccine, the 4:3:1, and the 4:3:1:3:3 vaccine series, respectively. Panel B suggests that the beneficial effect is still significant if we compare the differential impact for high Medicaid managed care penetration state with low Medicaid managed care penetration state. There is a 2.95 percentage point (about 3.5 percent) increase in the probability of up-to-date 1 dose of Varicella and 3.58 percentage point (about 5.17 percent) increase in probability of the most complete vaccine series, 4:3:1:3:3:1 associated with the beneficiaries that lives in a state with P4P program and higher penetration rate after policy adoption.

3.6.2 Effectiveness of Different Incentive Designs

The DDD estimation results for analysis of relative effectiveness of different incentive design for P4P program with NHIS data, and NIS data respectively are shown on Table 3.6 and Table 3.7. Consistent significance for the coefficient of the interaction term among above median penetration rate, P4P post adoption dummy and dummy for using negative financial incentive type suggest that states that use negative financial incentives, such as withholds and penalties, rather than states with positive financial incentives and non-financial incentives are more effective in improving utilization of adult cancer screen procedure as well as other preventive care services, and children immunization rate.

3.7 Conclusion

The overall results in this paper suggest that Medicaid managed care P4P programs are associated with increases in the use of preventive care services. We also find that P4P states with higher Medicaid managed care penetration rate or with negative financial incentive type in their program design are most effective in achieving this goal.

With expansions in Medicaid eligibility possible, Medicaid managed care programs may cover

more individuals. Our conclusion confirms that states attempt to link health care spending to quality and efficiency of care with P4P programs are effective. Results provide evidence that P4P programs are associated with increases in the use of preventive care services among Medicaid enrollees, especially for states with higher Medicaid managed care penetration rates. The incentive design of the program also matters since there are significant associations between different incentive types and the magnitude of increases in preventive care use.

Table 3.1: Summary of States Medicaid Managed Care P4P Programs

State	Date	Incentive1	Incentive2	Incentive3	Preventive care	Evaluation
California	2005	auto-assignment			no	no
Colorado	2007	differentials			no	no
Illinois	2006	payments			no	no
Indiana	2008	differentials	public reporting	withholds	yes	no
Maryland	2002	differentials	public reporting		yes	yes
Massachusetts	2010	differentials	withholds		no	no
Michigan	2001	differentials	auto-assignment		yes	yes
Minnesota	2006	differentials			yes	no
Minnesota	1999	differentials			no	no
Missouri	2001	differentials	auto-assignment		no	yes
Nevada	2006	differentials			yes	yes
New Mexico	1997	differentials	auto-assignment	withholds	yes	no
New York	2000	differentials	auto-assignment	public reporting	no	yes
Ohio	2002	differentials	auto-assignment	penalties	yes	yes
Oregon	2008	differentials			no	no
Pennsylvania	2006	differentials			no	yes
Rhode Island	1999	differentials			yes	yes
Tennessee	2006	differentials	withholds		no	no
Washington	2004	withholds			no	no
Wisconsin	1996	differentials			yes	no

[1] 2009 National Summary of State Medicaid Managed Care Programs: Program Description as of June 30, 2009.Available online

Table 3.2: Dependent Variables and Analysis Sample

Data sources	Dependent variable	Survey year
NHIS	Ever had a mammogram for female respondents between 50 and 64	98,99,00, 03, 05, 08, 10
	Ever had a colonoscopy for respondents between 50 and 64	00, 03, 05, 08
	Ever had any colorectal exam for respondents between 50 and 64	00, 03, 05, 08
	Ever had cholesterol checked for respondent between 40 and 64	98, 03, 08
	Ever had the blood pressure taken for respondent between 40 and 64	98,99, 03,08
	Ever had a Pap smear test for female respondent between 40 and 64	98,99,00, 03, 05, 08, 10
	Ever received hepatitis B vaccine for respondent between 18 and 64	00, 03, 05, 08, 10

[1] Survey years are summarized by authors from NHIS documents from 1998-2010.

Table 3.3: Summary Statistics for Outcomes, Stratifies by Treatment Status and Time Period

		Treatment state			Control state
	Medicaid	All	Before	After	
Mammogram for female between 50 and 64;	0.8656	0.8761	0.8321	0.9117	0.8501
	[0.008]	[0.009]	[0.013]	[0.012]	[0.013]
Observations	1,904	1,148	560	588	756
Colonoscopy for respondents between 50 and 64	0.1607	0.1512	0.1180	0.1721	0.1764
	[0.007]	[0.008]	[0.009]	[0.012]	[0.011]
Observations	3,003	1,874	764	1,109	1,129
Colorectal exam for respondents between 50 and 64	0.1828	0.1709	0.1554	0.1807	0.2024
	[0.007]	[0.009]	[0.012]	[0.012]	[0.011]
Observations	3,003	1,874	764	1,109	1,129
Cholesterol checked for respondent between 40 and 64	0.4484	0.4491	0.4222	0.4711	0.4473
	[0.009]	[0.011]	[0.015]	[0.016]	[0.014]
Observations	4,068	2,591	1,350	1,241	1,477
Blood pressure taken for respondent between 40 and 64	0.4817	0.4734	0.4247	0.5322	0.4957
	[0.008]	[0.010]	[0.013]	[0.016]	[0.012]
Observations	5,331	3,418	2,121	1,297	1,913
Pap smear test for female between 40 and 64	0.3480	0.3421	0.3691	0.3218	0.3582
	[0.004]	[0.005]	[0.008]	[0.007]	[0.007]
Observations	21,859	14,142	6,676	7,466	7,717
Hepatitis B vaccine for respondent between 18 and 64	0.1542	0.1508	0.1435	0.1535	0.1600
	[0.003]	[0.004]	[0.007]	[0.005]	[0.006]
Observations	17,105	11,073	3,355	7,716	6,032

[1] In order to account for the design of survey data, all means and standard deviations are weighted statistics using survey instruments.

Table 3.3: Summary Statistics for Outcomes, Stratified by Treatment Status and Time Period (Continued)

		Treatment state			Control state
	Medicaid	All	Before	After	
Up-to-date 4 doses of diphtheria-tetanus toxoids-pertussis vaccine (DTP)	0.8392	0.8444	0.8310	0.8510	0.8332
	[0.002]	[0.003]	[0.004]	[0.004]	[0.003]
Up-to-date 3 doses of poliovirus vaccine (Polio)	0.9243	0.9274	0.9166	0.9329	0.9207
	[0.001]	[0.002]	[0.003]	[0.002]	[0.002]
Up-to-date 1 dose of measles-mumps-rubella vaccine (MMR)	0.9271	0.9302	0.9287	0.9309	0.9234
	[0.001]	[0.002]	[0.003]	[0.002]	[0.002]
Up-to-date 3 doses of Haemophilus influenzae type B vaccine (Hib)	0.9196	0.9227	0.9381	0.9150	0.9160
	[0.001]	[0.002]	[0.003]	[0.003]	[0.002]
Up-to-date doses of hepatitis B vaccine (Hep B)	0.9277	0.9301	0.9226	0.9339	0.9249
	[0.001]	[0.002]	[0.003]	[0.002]	[0.002]
Up-to-date 1 dose of Varicella at 12+ months (Varicella)	0.8495	0.8503	0.7768	0.8872	0.8486
	[0.002]	[0.002]	[0.004]	[0.003]	[0.002]
Up-to-date 4:3:1 vaccine series (DTP, Polio, MMR)	0.7899	0.7937	0.7623	0.8090	0.7855
	[0.002]	[0.003]	[0.005]	[0.004]	[0.003]
Up-to-date 4:3:1:3:3 vaccine series (DTP, Polio, MMR, Hib and Hep B)	0.7472	0.7523	0.7239	0.7661	0.7412
	[0.002]	[0.003]	[0.005]	[0.004]	[0.003]
Up-to-date 4:3:1:3:3:1 vaccine series (DTP, Polio, MMR, Hib, Hep B and Varicella)	0.7016	0.7061	0.6395	0.7397	0.6963
	[0.002]	[0.003]	[0.005]	[0.004]	[0.003]
Observations	117,438	51,207	19,396	31,811	66,231

[1] In order to account for the design of survey data, all means and standard deviations are weighted statistics using survey instruments.

Table 3.4: Regression Results for Adult Preventive Care Services

	Mammogram	Colonoscopy	Colorectal	Cholesterol	Blood Pressure	Pap Test	Adult HepB
DD							
P4P * post	0.0517*	0.0125	-0.0111	0.0559*	0.0408	0.0204	-0.0073
	[0.028]	[0.025]	[0.026]	[0.029]	[0.026]	[0.017]	[0.011]
R squared	0.082	0.079	0.076	0.091	0.130	0.042	0.057
Obs.	1904	3003	3003	4068	5331	15076	16161
DDD with median MC pen rate							
Above Median MC * P4P * post	0.0709	0.0749	0.0599	0.1595**	0.1124**	0.0897**	0.0348
	[0.055]	[0.060]	[0.060]	[0.062]	[0.055]	[0.038]	[0.023]
R squared	0.094	0.083	0.080	0.093	0.132	0.044	0.058
Obs.	1904	3003	3003	4068	5331	15076	16161

[1] Control variables include respondents' gender, age, race, Ethnicity, education level, health status and any limitation on activity level and state dummies.

[2] ***Significant at the 1% level (two-tail test); ** Significant at the 5% level (two-tail test); * Significant at the 10% level (two-tail test).

Table 3.5: Regression Results for Utilization of Children Immunization

	DTP	Polio	MMX	HIB	HepB	Varicella	431	43133	431331
DD									
P4P *post	0.0102	0.0009	0.0074	0.0112**	0.0004	-0.0025	0.0159*	0.0215**	0.0072
	[0.008]	[0.005]	[0.005]	[0.005]	[0.005]	[0.007]	[0.008]	[0.009]	[0.009]
R squared	0.042	0.019	0.016	0.031	0.014	0.096	0.042	0.039	0.064
Obs.	116210	116900	117071	116723	116695	116530	113641	113641	117438
DDD with median MC pen rate									
Above Median MC * P4P * post	0.0069	-0.0077	0.0040	0.0143	-0.0026	0.0295***	0.0021	0.0153	0.0358***
	[0.012]	[0.008]	[0.008]	[0.009]	[0.008]	[0.011]	[0.013]	[0.013]	[0.014]
R squared	0.043	0.020	0.016	0.032	0.015	0.097	0.043	0.040	0.065
Obs.	116210	116900	117071	116723	116695	116530	113641	113641	117438

[1] Control variables include state dummies, child characteristics such as age, birth parity, race, Ethnicity and sex, as well as mother characteristics such as number of children in the household, education level, mobility and age for the analysis.

[2] ***Significant at the 1% level (two-tail test); ** Significant at the 5% level (two-tail test); * Significant at the 10% level (two-tail test).

Table 3.6: Regression Results for Adult Preventive Care Services, Different Trend by Incentive Types

	Mammogram	Colonoscopy	Colorectal	Cholesterol	Blood Pressure	Pap Test	Adult HepB
Above Median MC * post * Type 1	0.0066	-0.0046	-0.0204	0.1983***	0.0970	0.0736	0.0391
	[0.060]	[0.068]	[0.070]	[0.072]	[0.065]	[0.047]	[0.027]
Above Median MC * post * Type 2	0.0954*	0.1058**	0.1126**	0.0498	0.1257**	0.0697*	0.0114
	[0.051]	[0.049]	[0.049]	[0.065]	[0.061]	[0.040]	[0.021]
Above Median MC * post * Type 3	0.0831	0.0473	0.0314	-0.1281*	-0.0856	-0.0362	0.0091
	[0.072]	[0.074]	[0.077]	[0.071]	[0.063]	[0.044]	[0.029]
R squared	0.097	0.084	0.082	0.093	0.133	0.044	0.056
Obs.	1904	3003	3003	4068	5331	15076	17105

[1] Control variables include respondents' gender, age, race, Ethnicity, education level, health status and any limitation on activity level and state dummies.

[2] Type 1 indicates positive financial incentive types, such as differential reimbursement rate; Type 2 indicates negative financial incentive types, such as withholds and penalties; Type 3 indicates non-financial incentive types, such as auto assignment and public reporting.

[3] ***Significant at the 1% level (two-tail test); ** Significant at the 5% level (two-tail test); * Significant at the 10% level (two-tail test).

Table 3.7: Regression Results for Utilization of Children Immunization, Different Trends by Incentive Types

	DTP	Polio	MMX	HIB	HepB	Varicella	431	43133	431331
Above Median MC * post * Type 1	-0.0026	-0.0064	0.0047	0.0072	-0.0067	-0.0191	-0.0030	-0.0007	-0.0171
	[0.013]	[0.009]	[0.009]	[0.010]	[0.009]	[0.012]	[0.014]	[0.015]	[0.015]
Above Median MC * post * Type 2	0.0129	0.0052	0.0085	0.0165	0.0056	0.0647***	0.0017	0.0261*	0.0721***
	[0.013]	[0.009]	[0.009]	[0.010]	[0.009]	[0.012]	[0.014]	[0.015]	[0.015]
Above Median MC * post * Type 3	0.0110	0.0096	-0.0005	-0.0061	0.0194*	-0.0018	0.0223	0.0184	0.0075
	[0.015]	[0.011]	[0.011]	[0.011]	[0.011]	[0.015]	[0.016]	[0.017]	[0.018]
R squared	0.043	0.020	0.016	0.032	0.015	0.097	0.043	0.040	0.065
Obs.	116210	116900	117071	116723	116695	116530	113641	113641	117438

[1] Control variables include state dummies, child characteristics such as age, birth parity, race, Ethnicity and sex, as well as mother characteristics such as number of children in the household, education level, mobility and age for the analysis.

[2] Type 1 indicates positive financial incentive types, such as differential reimbursement rate; Type 2 indicates negative financial incentive types, such as withholds and penalties; Type 3 indicates non-financial incentive types, such as auto assignment and public reporting.

[3] ***Significant at the 1% level (two-tail test); ** Significant at the 5% level (two-tail test); * Significant at the 10% level (two-tail test).

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Appendix A

Description of the Data-Merging Procedure Between NAMCS and FDA Orangebook Database

The most updated FDA drug approval database is located at `http://www.fda.gov/Drugs/InformationOnDrugs/ucm129689.htm`.

Three zipped ASCII files are downloaded from the website. The one under the name products.txt is the one containing information about ingredient, dosage form, trader name, approval date and type (whether it is a prescription drug or not). Next, we transferred the data into STATA format using StatTransfer. We cleaned the data to calculate the approval date for each drug, counting drugs with different manufacturers or strengths or packages as the same drug.

The NAMCS data has variables called MED1-MED8 (up to MED6 if before 2003). These variables using a different drug coding system beginning in 2006, but with the SAS program $DRUGCHAR\_MULTUM\_`year'.sas$ provided by NCHS, one can change the variables in NAMCS 2002-2005 and make them consistent with those in or after 2006. We then identify the drug trade name using appendix B (drug entry codes and names in numeric order) from the FDA site. We copied and pasted the list into STATA ($drug\_name\_code2002.dta - drug\_name\_code2009.dta$) and merged this list with NAMCS data.

Now that we have drug trade names in both the FDA and NAMCS datasets, we can go ahead and merge the FDA approval date onto NAMCS data using the first word of the drug name (most of the drugs can be identified with the first word in its name). And the merge rate was around 95% with this method for prescription drugs.

Appendix B

Theoretical Framework for Chapter 2

B.1 Set Up of the Model

In this appendix, we present the theoretical model supporting hypotheses of this paper.

We have several basic assumptions to set up this model. The first is that physicians are altruistic in their treatment decisions. Physicians decide the best amount of prescription drugs for the patient.

This physician altruistic assumption is commonly imposed by a lot of literatures. Arrow (1963) suggested that health care providers, unlike providers in other business, may care about their clients' welfare, and behave in an altruistic way. As it comes to Medicare patients, physicians do not pay for the drugs they prescribe to patients financially (it might take effort and time for physicians to get to know patients' insurance plan policies and coverage, which is a non-financial cost), nor do they get reimbursed for their prescription behavior. Also, we assume physicians are price takes for health care services provided to Medicare patients.

Thus, we can write utility function for an individual patient as:

$$\max_{D_i, A_i, T_i} w^f(i) = mF(D_i, A_i, T_i) - k_d p_d D_i - k_t p_t T_i \tag{B.1}$$

D_i represents the number of drug treatment for patient i, A_i, is the effort level physicians take to treat the patient, and T_i is the quantity of other medical care services performed in the physician office for patient i, such as blood pressure check, X-ray, EKG/ECG, Pap test, urinalysis, PSA test, and CBC (complete blood count). F is a health production function of prescription drug, other medical care services and physician effort inputs. We assume F is increasing in all three inputs and twice continuously differentiable for positive levels of inputs. Health adds positive utility to patients' utility function and each unit of health worths $\$m$. p_d and p_t stands for the listed price of each unit of drug and other medical care services, respectively. Although different in the generosity of coverage, with Medicare or private insurance, patients only need to pay a fraction of listed prices, the fraction for prescription drugs is k_d, for other services is k_t.

The partial derivatives are F_D, F_T and the second derivatives are F_{DD}, F_{TT} and F_{DT}.

We will also assume $F(D_i, T_i)$ is homothetic or homogeneous in D_i and T_i. And the health

production function satisfies the Inada conditions. That is

$$\lim_{D_i \to 0} F_D(D_i, T_i) = \lim_{T_i \to 0} F_T(D_i, T_i) = 0 \tag{B.2}$$

and

$$\lim_{D_i \to \infty} F_T(D_i, T_i) = \lim_{T_i \to \infty} F_T(D_i, T_i) = 0 \tag{B.3}$$

B.1.1 The Impact of Part D on the Prescribing Pattern

The adoption of Part D policy will lower the OOP cost of prescription drug to Medicare patients, as we dicussed in the background section. Specifically, it will reflect in the decrease of k_d. We can thus analyze the effect of the adoption of Part D policy as comparative statics of k_d. If we don't consider the utilization of other services, we can further separate prescribed drugs into two category: generic and branded drugs. Assume physician prescribe D_{gi} unit of generic drugs and D_{bi} unit of branded drugs for the patients. OOP cost fraction of price patients need to pay for generic drugs is k_{d_g}, for branded drugs is k_{d_b}.

Then we can write the objective function as:

$$\max_{D_{gi}, D_{bi}} w^f(i) = mF(D_{gi}, D_{bi}) - k_{d_g} p_g D_{gi} - k_{d_b} p_b D_{bi} \tag{B.4}$$

PROPOSITION 1: assume $F(D_{gi}, D_{bi})$ be homothetic in D_{gi} and D_{bi}, then the move from less generous prescription drug coverage to more generous drug coverage may affect the generic-branded drug ratio. Using $D_{gi}^{'}$ and $D_{bi}^{'}$ to indicate the number of prescribed generic and branded drugs after the adoption of Part D, we have

$$\frac{D_{gi}^{'}}{D_{bi}^{'}} >=< \frac{D_{gi}}{D_{bi}} \text{if and only if} \frac{k_{d_g}^{'}}{k_{d_b}^{'}} <=> \frac{k_{d_g}}{k_{d_b}} \tag{B.5}$$

Proof: The first order conditions of this maximization problem are:

$$mF_{D_{gi}}(D_{gi}, D_{bi}) - k_{d_g} p_g = 0 \tag{B.6}$$

and

$$mF_{D_{bi}}(D_{gi}, D_{bi}) - k_{d_b} p_b = 0 \tag{B.7}$$

for generic and branded prescription drugs.

Taking the ratio of (6) to (7) we have:

$$\frac{F_{D_{gi}}(D_{gi}, D_{bi})}{F_{D_{bi}}(D_{gi}, D_{bi}} = \frac{k_{d_g} p_g}{k_{d_b} p_b} \tag{B.8}$$

Applying the same procedure to derive the equation for the prescription drugs after the adoption of Part D, we have:

$$\frac{F_{D'_{gi}}(D'_{gi}, D'_{bi})}{F_{D'_{bi}}(D'_{gi}, D'_{bi})} = \frac{k_{d'_g} p_g}{k_{d'_b} p_b} \tag{B.9}$$

Further taking the ratio of (9) to (8), we obtain

$$\frac{F_{D'_{gi}}(D'_{gi}, D'_{bi})/F_{D'_{bi}}(D'_{gi}, D'_{bi})}{F_{D_{gi}}(D_{gi}, D_{bi})/F_{D_{bi}}(D_{gi}, D_{bi}} = \frac{k'_{d_g}/k'_{d_b}}{k_{d_g}/k_{d_b}} \tag{B.10}$$

When $F(D_{gi}, D_{bi})$ is homothetic in D_{gi} and D_{bi}, the left side of equation (10) is a decreasing function of $\frac{D'_{gi}/D'_{bi}}{D_{gi}/D_{bi}}$. Then we can establish the relationship in equation (5).

Proposition 1 implies that when the relative generosity of insurance coverage for generic and branded drugs changes, the ratio of generic drugs and branded name drugs in physicians' prescription will change as well. Specifically, when Part D increase the generosity for generic drugs more than they do for branded drugs, i.e.: the OOP cost fraction for generic drugs decrease more than that for branded drugs ($\frac{k'_{dg}}{k'_{d_b}} < \frac{k_{dg}}{k_{d_b}}$), results will show that the utilization for generic drugs increase more than the increase of branded drugs ($\frac{D'_{gi}}{D'_{bi}} > \frac{D_{gi}}{D_{bi}}$).Otherwise, empirical results will show that the utilization for generic drugs increase less than the increase of branded drugs ($\frac{D'_{gi}}{D'_{bi}} < \frac{D_{gi}}{D_{bi}}$).

B.2 The Impact of Part D on the Utilization of Prescription Drugs and Other Medical Care Services

The adoption of Part D policy will lower the OOP cost of prescription drug to Medicare patients. Specifically, it will reflect in the decrease of out-of-pocket cost fraction of prescription drug k_d. We can thus analyze the effect of the adoption of Part D policy as comparative statics of k_d.

Then we can write the objective function as:

$$\max_{D_i, A_i, T_i} w^f(i) = mF(D_i, A_i, T_i) - k_d p_d D_i - k_t p_t T_i \tag{B.11}$$

PROPOSITION 2: Let $D_i(k_d)$ and $T_i(k_d)$ be the optimal choices for patient i at the copay k_d. Then

$$\frac{dD_i(k_d)}{dk_d} = \frac{p_d F_{DD}}{F_{DD}F_{TT} - (F_{DT})^2} < 0 \tag{B.12}$$

PROPOSITION 3: assume $F(D_i, A_i, T_i)$ be homogeneous of degree $\alpha < 1$ in D_i and T_i,we can get $F(D_i, A_i, T_i) = G_1(A_i)\phi(D_i, T_i)^\alpha$, in which $\phi(\cdot, \cdot)$ exhibits constant return to scale. Let the local elasticity of substitution between drug and non-drug treatment be ϵ_{DT}. Thus we have

$$\frac{dT_i(k_d)}{dk_d} >=< 0 \text{if and only if} \frac{1}{1-\alpha} <=> \epsilon_{DT} \tag{B.13}$$

Proof: The first order conditions of this maximization problem are:

$$mF_D(D_i, A_i, T_i) - k_d p_d = 0 \tag{B.14}$$

and

$$mF_T(D_i, A_i, T_i) - k_t p_t = 0 \tag{B.15}$$

for prescription drug and other medical care services.

The first order conditions are necessary and sufficient for the maximization of patients' utility function. Taking the total differential of the two first order condition with respect to D_i, T_i and k_d, we have:

$$\begin{pmatrix} F_{DD} & F_{DT} \\ F_{TD} & F_{TT} \end{pmatrix} \begin{pmatrix} dD \\ dT \end{pmatrix} = \begin{pmatrix} p_d \\ 0 \end{pmatrix} dk_d \tag{B.16}$$

Apply Cramer's rule, we can get $\frac{dD_i(k_d)}{dk_d} = \frac{p_d F_{DD}}{F_{DD}F_{TT} - (F_{DT})^2}$. The second order condition for maximization of patient utility function ensures that $F_{DD}F_{TT} - (F_{DT})^2 > 0$ while the concavity of F function ensures that $F_{DD} < 0$. Thus $\frac{dD_i(k_d)}{dk_d} < 0$.

We also get $\frac{dT_i(k_d)}{dk_d} = \frac{-p_d F_{DT}}{F_{DD}F_{TT} - (F_{DT})^2}$ after applying Cramer's rule. Thus $\frac{dT_i(k_d)}{dk_d}$ will be positive if $F_{DT} < 0$, and it will negative it $F_{DT} > 0$.

Assuming $F(D_i, A_i, T_i)$ be homogenous of degree $\alpha < 1$ in D_i and T_i (that is, $F(D_i, A_i, T_i) = G(A_i)\phi(D_i, T_i)^\alpha$), we have

$$F_{DT} = \alpha G(A_i)\phi(D_i, T_i)^{\alpha-2}((\alpha - 1)\phi_D\phi_T + \phi_{DT}\phi) \tag{B.17}$$

The definition for the local elasticity of substitution indicates $\epsilon_{DT} = \frac{d\ln(D/T)}{d\ln(F_D/F_T)}$. Because ϕ exhibits constant returns to scale, we derive

$$\epsilon_{DT} = \frac{\phi_D\phi_T}{\phi_{DT}\phi} \tag{B.18}$$

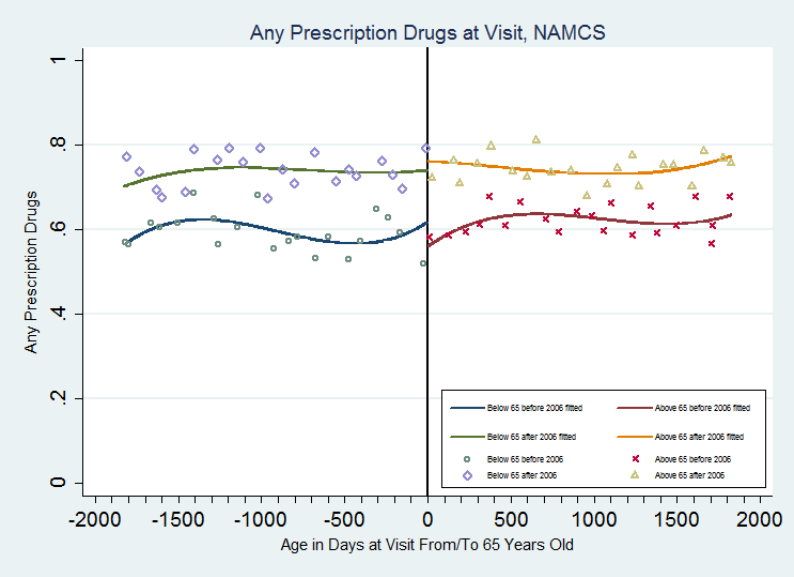
Above two equations imply that $F_{DT} > 0$ if and only if $1/(1-\alpha) > \epsilon_{DT}$ and $F_{DT} < 0$ if and only if $1/(1-\alpha) < \epsilon_{DT}$. Thus we can derive equation (13).

Proposition 1 implies that when copay for drug treatment decreases for a certain patient, physician increase the quantity of prescription drugs for him, that is, $dD_i(k_d)/dk_d < 0$. However, the quantity of other health care could increase, decrease or stay the same, depending on the relative size of "decreasing return" to scale (α) and elasticity of substitution between drug and other services(ϵ_{DT}). For example, if $\epsilon_{DT} < 1$, two types of treatments are complementary in the health production function, quantity of other services will increase as copay for prescription drugs decreases. If $\epsilon_{DT} > 1$ and $\alpha < 1$, there is enough substitutability between two treatments, the quantity of other services will decrease.

Appendix C

Appendix Figures and Tables

Figure C.1: Any Prescription Drugs at Visit, NAMCS



Samples are based on data from the National Ambulatory Medical Care Survey (2002-2004 and 2006-2009)data. The estimated discontinuities (and standard errors) at age 65 and the fitted lines are from a regression with a cubic polynomial in age fully interacted with a dummy for age greater than or equal to 65 and a dummy for year in or after 2006. Points represent means for people in each age cell (measured in quarters).

Figure C.2: Number of Branded Drugs at Visit, NAMCS

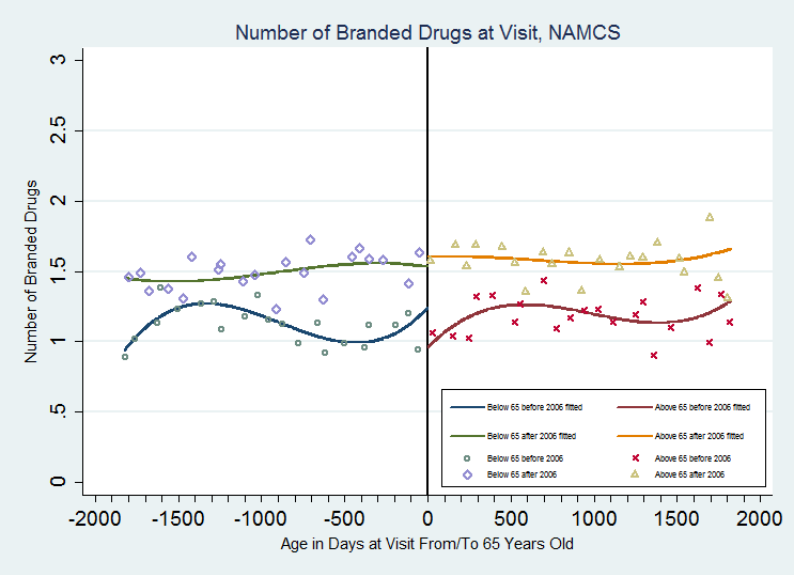
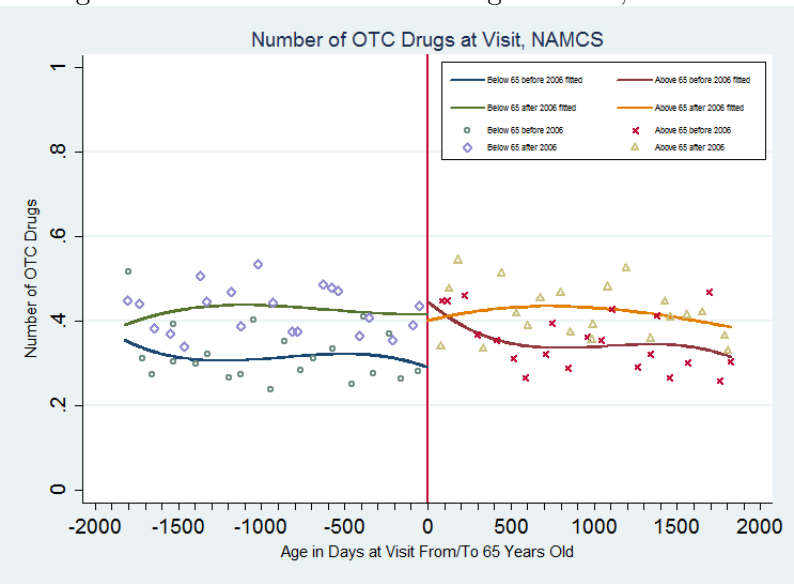


Figure C.3: Number of OTC Drugs at Visit, NAMCS



Samples are based on data from the National Ambulatory Medical Care Survey (2002-2004 and 2006-2009)data. The estimated discontinuities (and standard errors) at age 65 and the fitted lines are from a regression with a cubic polynomial in age fully interacted with a dummy for age greater than or equal to 65 and a dummy for year in or after 2006. Points represent means for people in each age cell (measured in quarters).

Figure C.4: Total Number of Drugs at Visit, NAMCS

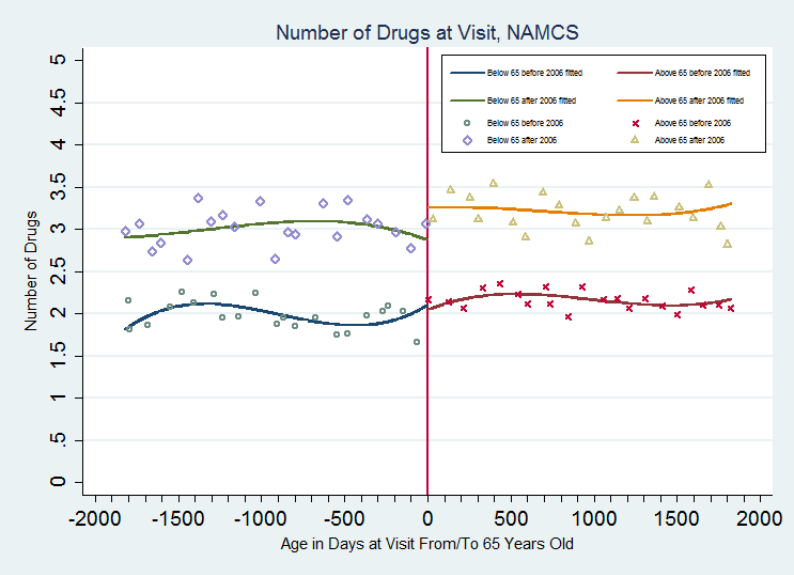
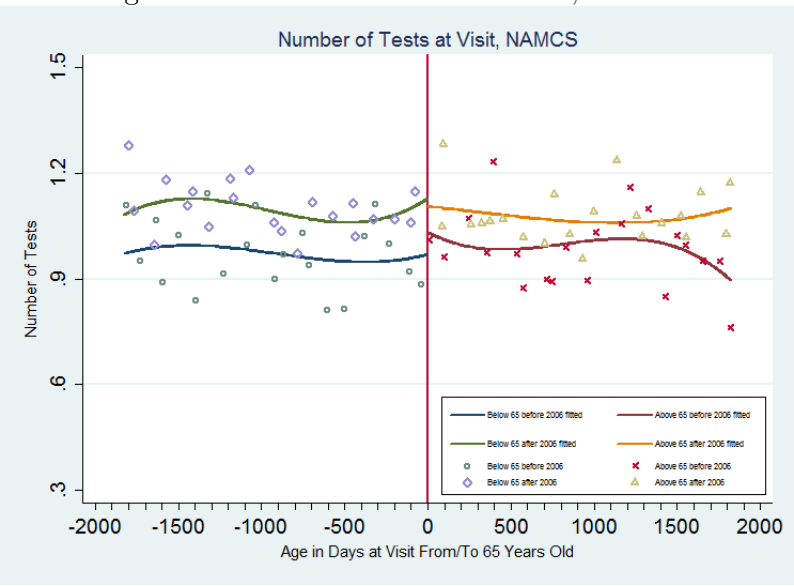


Figure C.5: Number of Tests at Visit, NAMCS



Samples are based on data from the National Ambulatory Medical Care Survey (2002-2004 and 2006-2009)data. The estimated discontinuities (and standard errors) at age 65 and the fitted lines are from a regression with a cubic polynomial in age fully interacted with a dummy for age greater than or equal to 65 and a dummy for year in or after 2006. Points represent means for people in each age cell (measured in quarters).

Figure C.6: Time with MD at Visit, NAMCS

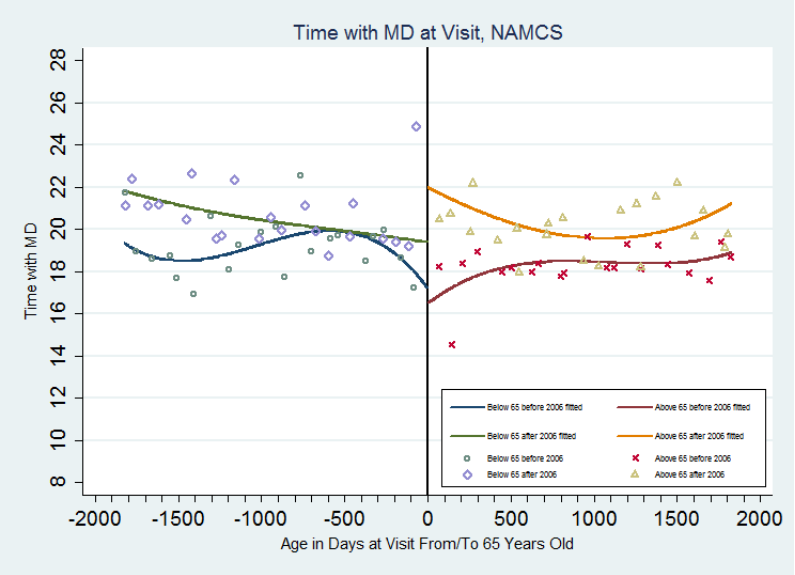
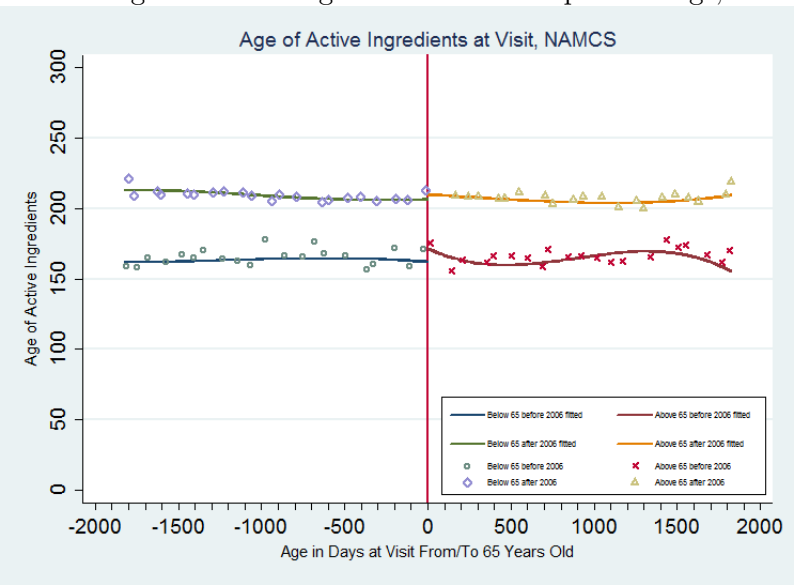


Figure C.7: Age of Active Ingredients of Prescription Drugs, NAMCS



Samples are based on data from the National Ambulatory Medical Care Survey (2002-2004 and 2006-2009)data. The estimated discontinuities (and standard errors) at age 65 and the fitted lines are from a regression with a cubic polynomial in age fully interacted with a dummy for age greater than or equal to 65 and a dummy for year in or after 2006. Points represent means for people in each age cell (measured in quarters).

Table C.1: Summary of Utilization and Expenditure,NAMCS/MEPS/NIS-HCUP

	Before 2006		After 2006	
	Below 65	Above 65	Below 65	Above 65
NAMCS data				
Number of Rx Drugs	1.5995	1.7421	2.4978	2.6894
	[0.049]	[0.060]	[0.067]	[0.073]
Number of Generic Drugs	0.4676	0.5548	1.0071	1.1063
	[0.025]	[0.028]	[0.034]	[0.034]
Any Rx Drugs	0.5959	0.6199	0.7366	0.7443
	[0.012]	[0.014]	[0.010]	[0.010]
Number of Brand Name Drugs	1.1316	1.1874	1.4907	1.5831
	[0.038]	[0.044]	[0.047]	[0.051]
Number of OTC Drugs	0.3166	0.3524	0.425	0.42
	[0.017]	[0.019]	[0.017]	[0.017]
Number of Drugs	1.9878	2.1582	3.0136	3.2123
	[0.060]	[0.071]	[0.078]	[0.084]
Number of Diagnostic Tests	0.9726	0.9905	1.0951	1.0756
	[0.026]	[0.034]	[0.020]	[0.022]
Time Spent with MD	19.0852	18.2012	20.4644	20.3143
	[0.394]	[0.347]	[0.285]	[0.587]
Sample size	4857	4880	8589	8148
Age of Active Ingredient	163.2512	164.3038	209.0620	205.9645
	[1.646]	[1.614]	[1.377]	[1.171]
Sample size	6657	7317	20090	20068
MEPS data				
Number of Prescriptions	21.2433	23.4335	22.0002	26.4892
	[0.674]	[0.662]	[0.509]	[0.772]
Expenditure for Prescriptions	1,556.62	1,687.74	1,688.55	2,074.38
	[55.644]	[48.229]	[53.439]	[80.166]
OOP Cost Per Script	26.3011	33.9963	19.7876	21.3611
	[0.737]	[0.810]	[0.508]	1.000]
Rx Expenditure from Medicare	21.278	137.4626	167.8622	990.0724
	[5.590]	[17.196]	[19.516]	[47.770]
Rx Expenditure from Medicaid	153.2722	175.6891	113.3067	15.1114
	[18.374]	[18.767]	[15.451]	[2.914]
Rx Expenditure from Private	649.168	441.2063	824.9692	372.736
	[29.370]	[27.207]	[45.240]	[32.840]
Total Exp	6,577.93	7,361.14	7,135.29	7,318.92
	[411.756]	[351.460]	[303.715]	[235.426]
ER Exp	168.9881	172.838	217.3752	134.9842
	[18.033]	[38.645]	[23.575]	[14.162]
Inpatient Exp	2,199.66	2,597.48	1,917.99	1,987.40
	[347.287]	[269.855]	[152.403]	[136.954]
Outpatient Exp	643.12	756.6879	820.5137	609.0538
	[40.900]	[83.804]	[113.207]	[40.135]
Office Visit Exp	1,372.59	1,480.73	1,754.29	1,765.59
	[76.368]	[63.704]	[88.539]	[73.562]
Health Good	0.8881	0.8976	0.9001	0.9033
	[0.007]	[0.007]	[0.006]	[0.006]
Sample size	3,897	3,075	5,650	4,309

[1] All means and standard deviations are weighted statistics using survey instruments

Table C.1: Summary of Utilization and Expenditure,NAMCS/MEPS/NIS-HCUP (continued)

	Before 2006		After 2006	
	Below 65	Above 65	Below 65	Above 65
NIS-HCUP data				
Mortality	0.024	0.0277	0.0213	0.0247
	[0.000]	[0.000]	[0.000]	[0.000]
Length of Stay	5.3484	5.4869	5.3048	5.3595
	[0.035]	[0.033]	[0.040]	[0.037]
Total Charge	26,700.26	26,777.77	38,847.55	39,115.95
	[478.396]	[465.065]	[796.325]	[780.776]
Number of Procedures	1.8025	1.7743	2.0194	2.0148
	[0.021]	[0.021]	[0.035]	[0.033]
Diabetes Short-Term Complications	0.0024	0.0017	0.0025	0.0017
	[0.000]	[0.000]	[0.000]	[0.000]
Perforated Appendix	0.0044	0.0032	0.0051	0.0036
	[0.000]	[0.000]	[0.000]	[0.000]
Diabetes Long-Term Complications	0.0141	0.0129	0.0138	0.0128
	[0.000]	[0.000]	[0.000]	[0.000]
COPD or Asthma in Older Adults	0.0445	0.046	0.0377	0.0397
	[0.001]	[0.001]	[0.001]	[0.001]
Hypertension	0.0044	0.0042	0.005	0.0045
	[0.000]	[0.000]	[0.000]	[0.000]
Heart Failure	0.0412	0.0468	0.0343	0.0391
	[0.001]	[0.001]	[0.001]	[0.001]
Dehydration	0.0103	0.0106	0.0144	0.0144
	[0.000]	[0.000]	[0.000]	[0.000]
Bacterial Pneumonia	0.0314	0.0342	0.0276	0.0309
	[0.000]	[0.001]	[0.001]	[0.001]
Urinary Tract Infection	0.009	0.0102	0.0101	0.0117
	[0.000]	[0.000]	[0.000]	[0.000]
Angina without Procedure	0.0056	0.0048	0.0033	0.0028
	[0.000]	[0.000]	[0.000]	[0.000]
Uncontrolled Diabetes	0.0022	0.0019	0.002	0.0017
	[0.000]	[0.000]	[0.000]	[0.000]
Lower-Extremity Amputation Diabetes	0.0029	0.0028	0.0024	0.0022
	[0.000]	[0.000]	[0.000]	[0.000]
Any Avoidable Hospitalization	0.1711	0.1781	0.1571	0.164
	[0.002]	[0.002]	[0.002]	[0.002]
Sample size	1,209,946	1,343,435	962,385	1,009,566

[1] All means and standard deviations are weighted statistics using survey instruments.
[2] Standard deviations are in brackets.

Table C.2: Summary Statistics for Independent Variables, NAMCS

	Mean	Std. Dev.
Patient Characteristics:		
1(Age=65)	0.4984	[0.005]
Age	64.4111	[0.027]
Female	0.5608	[0.005]
Nonwhite	0.1365	[0.008]
Charlson Index=1	0.0683	[0.003]
Charlson Index=2	0.0561	[0.003]
Infectious and Parasitic Diseases	0.0128	[0.001]
Neoplasms	0.0544	[0.003]
Endocrine, Nutritional and Metabolic Diseases, and Immunity Disorders	0.0978	[0.004]
Diseases of The Blood and Blood-Forming Organs	0.0077	[0.001]
Mental Disorders	0.0303	[0.002]
Diseases of The Nervous System and Sense Organs	0.0967	[0.003]
Diseases of The Circulatory System	0.1459	[0.004]
Diseases of The Respiratory System	0.0787	[0.004]
Diseases of The Digestive System	0.0406	[0.002]
Diseases of The Genitourinary System	0.0468	[0.002]
Diseases of The Skin and Subcutaneous Tissue	0.0428	[0.002]
Diseases of The Musculoskeletal System and Connective Tissue	0.1068	[0.004]
Congenital Anomalies	0.0018	[0.000]
Symptoms, Signs, and Ill-Defined Conditions	0.068	[0.002]
Injury and Poisoning	0.1308	[0.005]
Visit Quarter==1	0.2407	[0.009]
Visit Quarter==2	0.2796	[0.010]
Visit Quarter==3	0.2424	[0.009]
Visit Quarter==4	0.2374	[0.008]
Physician Practice Characteristics:		
MSA	0.8567	[0.032]
Solo Practice	0.3406	[0.013]
Electronic Medicaid Record	0.3308	[0.014]
General and Family Practice	0.2192	[0.010]
Internal Medicine	0.2156	[0.009]
Pediatrics	0.0048	[0.001]
General Surgery	0.0272	[0.002]
Obstetrics & Gynecology	0.0277	[0.002]
Orthopedic Surgery	0.0652	[0.005]
Cardiovascular Diseases	0.0542	[0.003]
Dermatology	0.0391	[0.003]
Urology	0.0356	[0.002]
Psychiatry	0.0195	[0.001]
Neurology	0.0142	[0.001]
Ophthalmology	0.0888	[0.004]
Otolaryngology	0.0184	[0.001]
Other specialties	0.1606	[0.008]
Oncologists	0.0079	[0.001]
Mid-level provider	0.002	[0.000]
Dummy (above median of prmcare=1)	0.6884	[0.011]
Sample Size		26,474

[1] *prmcare* stands for precent of revenue from Medicare patients by physicians, missing values are imputed using predicted value of tobit regression of prmcare on the year dummy, physician specialty dummy, interaction between specialty dummy and percent of population over 65 years old, interaction between specialty dummy and percent of patients over 65 years old by physicians, whether physician use electronic medical records. Cutoff value for visit level analysis is 0.25, for drug level analysis is 0.30.

[2] All means and standard deviations are weighted statistics using survey instruments.

[3] Standard deviations are in brackets.

Table C.3: Summary Statistics for Independent Variables, MEPS

	Mean	Std. Dev.
Female	0.5251	[0.004]
White	0.8500	[0.006]
Black	0.0949	[0.004]
Other Race	0.0551	[0.004]
Hispanic	0.0720	[0.004]
Poverty Category: Poor (Less Than 100%)	0.0862	[0.003]
Poverty Category: Near Poor (100% To Less Than 125%)	0.0410	[0.002]
Poverty Category: Low Income (125% To Less Than 200%)	0.1196	[0.004]
Poverty Category: Middle Income (200% To Less Than 400%)	0.2728	[0.006]
Poverty Category: High Income (Greater Than Or Equal To 400%)	0.4803	[0.007]
Census Region: Northeast	0.1947	[0.008]
Census Region: Midwest	0.2195	[0.009]
Census Region: South	0.3788	[0.010]
Census Region: West	0.2070	[0.008]
MSA	0.7911	[0.011]
Education Level: No Degree	0.1619	[0.004]
Education Level: High School	0.4934	[0.007]
Education Level: Bachelor Degree	0.1551	[0.005]
Education Level: Post Degree	0.1200	[0.005]
Education Level: Other Degree	0.0696	[0.004]
Marital Status: Married	0.6725	[0.007]
Marital Status: Widowed	0.1044	[0.004]
Marital Status: Divorced	0.1606	[0.004]
Marital Status: Separated	0.0168	[0.001]
Marital Status: Never Married	0.0456	[0.002]
Prior Condition: Diabetes	0.1782	[0.005]
Prior Condition: Asthma	0.0964	[0.003]
Prior Condition: High Blood Pressure	0.5422	[0.006]
Prior Condition: Angina	0.0525	[0.002]
Prior Condition: Heart Attack	0.0723	[0.003]
Prior Condition: Joint Pain	0.5193	[0.006]
Prior Condition: Stroke	0.0564	[0.003]
Prior Condition: Emphysema	0.0439	[0.002]
Prior Condition: Arthritis	0.4443	[0.006]
Prior Condition: Other Heart Condtions	0.1364	[0.004]
Observations		16,931

[1] All means and standard deviations are weighted statistics using survey instruments.
[2] Standard deviations are in brackets.

Table C.4: Summary Statistics for Independent Variables, NIS-HCUP

	Mean	Std. Dev.
Female	0.5075	[0.001]
Admission During Weekends	0.1718	[0.001]
Emergency Room Admissions	0.5119	[0.006]
Transfer from Other Facilities	0.0609	[0.002]
Elective Admissions	0.2974	[0.004]
Hospital Bed Size: Medium	0.2387	[0.008]
Hospital Bed Size: Large	0.6386	[0.009]
Hospital Ownership: Government, Nonfederal, Public	0.0668	[0.005]
Hospital Ownership: Private, Non-Profit, Voluntary	0.2023	[0.008]
Hospital Ownership: Private, Invest-Own	0.1026	[0.005]
Hospital Ownership: Private, Collapsed Category	0.0387	[0.003]
Urban Hospital	0.8633	[0.005]
Teaching Hospital	0.4402	[0.011]
Zip Code Level Median Household Income Quartile==2	0.2547	[0.005]
Zip Code Level Median Household Income Quartile==3	0.2415	[0.004]
Zip Code Level Median Household Income Quartile==4	0.2578	[0.008]
Observations		4,525,332

[1] All means and standard deviations are weighted statistics using survey instruments.

[2] Standard deviations are in brackets.

Table C.5: GLM Regression Results for Prescribing Pattern, NAMCS

	Number of Prescription Drug	Number of Generic Drug	Any Prescription Drugs	Number of Brand	Number of OTC Drugs	Number of Drugs	Number of Procedures	Time Spent
Age 65 * Year 2006	0.3177**	0.3279	0.1205	0.3543	-0.4446*	0.1846	-0.0728	0.2088
	[0.160)	[0.202)	[0.102)	[0.216)	[0.265)	[0.141)	[0.135)	[0.186)
Marginal Effects	0.6807	0.2488	0.0822	0.4712	-0.1310	0.4688	-0.0507	4.1700
Order of Polynomial	3	3	3	3	3	3	3	3
Age 65 * Year 2006	0.3148*	0.3739	-0.0229	0.2529	-0.7517**	0.1021	0.0099	0.3537
	[0.189]	[0.270]	[0.124]	[0.270]	[0.363]	[0.169]	[0.163]	[0.368]
Marginal Effects	0.6733	0.2861	-0.0152	0.3275	-0.2088	0.2541	0.0070	7.2733
Order of Polynomial	4	4	4	4	4	4	4	4
Observations	26,474	26,474	26,474	26,474	26,474	26,474	26,474	26,474

[1] Control variables for regressions include dummy of year 2006, age 65 or over, polynomial control of age in days and their full interaction with age 65 dummy and year2006 dummy, patient sex, race, Charlson index dummies, disease category by primary diagnosis codes, visit quarter, physician specialty type, whether it is a solo practice or not, electronic medical records utilization, MSA status and dummy for revenue from Medicare patients above median.

[2] Results are estimated using survey GLM regressions with Gaussian family distribution with log link. Standard errors are in brackets. Marginal effects are also listed in the table.

[3] ***Significant at the 1% level (two-tail test); ** Significant at the 5% level (two-tail test); * Significant at the 10% level (two-tail test).

Table C.6: Two-Part Regression Results for Utilization and Expenditure of Prescription Drugs, MEPS

	Number of Rx Drugs	Exp for Rx Drugs	OOP cost per script	Rx Exp from Medicare	Rx Exp from Medicaid	Rx Exp from Private
Part 1:	0.0202	-0.0555	0.0265	0.5596***	-0.0152	-0.3781***
Age 65 * Year 2006	[0.058]	[0.037]	[0.060]	[0.064]	[0.031]	[0.083]
Part 2:	0.3368*	0.5153**	-0.2128	0.0254	-3.0605***	-1.4030***
Age 65 * Year 2006	[0.200]	[0.231]	[0.173]	[0.935]	[1.138]	[0.468]
Marginal Effects	4.85	515.82	-3.45	31677.91	-22403.36	-390.52
Order of Polynomial	1	1	1	1	1	1
Observations	16,931	16,931	16,931	16,931	16,931	16,931

[1] Control variables for regressions include dummy of year 2006, age 65 or over, polynomial control of age in days and their full interaction with age 65 dummy and year2006 dummy, patient sex (not included in regressions in column 3 and 4), race, poverty categories(not included in regressions in column 5-9), census region dummies, education level categorical dummies, marital status dummies, prior medical conditions and MSA status.

[2] Results are estimated using survey two-part regressions with logged second part outcomes. Standard errors are in brackets. Marginal effects are also listed in the table.

[3] ***Significant at the 1% level (two-tail test); ** Significant at the 5% level (two-tail test); * Significant at the 10% level (two-tail test).

Table C.7: Two-Part Regression Results for Other Medical Expenditure, MEPS

	Total Exp	ER Exp	Inpatient Exp	Outpatient Exp	Office Visit Exp
Part 1:					
Age 65 * Year 2006	-0.0555	0.0685	0.0022	0.0948	0.0566
	[0.037]	[0.055]	[0.052]	[0.077]	[0.060]
Part 2:					
Age 65 * Year 2006	0.5153**	-0.3051	0.6346	0.1775	0.1731
	[0.231]	[0.555]	[0.622]	[0.463]	[0.257]
Marginal Effects	4201.07	73.77	389.54	456.09	338.67
Order of Polynomial	1	1	1	1	1
Observations	16,931	16,931	16,931	16,931	16,931

[1] Control variables for regressions include dummy of year 2006, age 65 or over, polynomial control of age in days and their full interaction with age 65 dummy and year2006 dummy, patient sex (not included in regressions in column 3 and 4), race, poverty categories(not included in regressions in column 5-9), census region dummies, education level categorical dummies, marital status dummies, prior medical conditions and MSA status.

[2] Results are estimated using survey two-part regressions with logged second part outcomes. Standard errors are in brackets. Marginal effects are also listed in the table.

[3] ***Significant at the 1% level (two-tail test); ** Significant at the 5% level (two-tail test); * Significant at the 10% level (two-tail test).

Table C.8: Variation of State-Level Medicaid Managed Care Penetration Rate, NIS

State Abbreviation	Mean	Std. Dev.
AK	0.0000	[0.000]
AL	0.6115	[0.057]
AR	0.7036	[0.097]
AZ	0.9082	[0.021]
CA	0.5142	[0.014]
CO	0.9405	[0.020]
CT	0.7247	[0.038]
DC	0.6963	[0.123]
DE	0.7678	[0.037]
FL	0.6386	[0.041]
GA	0.8587	[0.102]
HI	0.8378	[0.087]
IA	0.8566	[0.041]
ID	0.6522	[0.222]
IL	0.2393	[0.209]
IN	0.7004	[0.031]
KS	0.6263	[0.140]
KY	0.8317	[0.114]
LA	0.4889	[0.297]
MA	0.6114	[0.032]
MD	0.7143	[0.041]
ME	0.5309	[0.161]
MI	0.9225	[0.081]
MN	0.6416	[0.024]
MO	0.6133	[0.259]
MS	0.3606	[0.300]
MT	0.6582	[0.139]
NC	0.7121	[0.061]
ND	0.5922	[0.038]
NE	0.7743	[0.050]
NH	0.2633	[0.332]
NJ	0.6732	[0.049]
NM	0.6627	[0.044]
NV	0.6317	[0.224]
NY	0.5493	[0.155]
OH	0.3499	[0.162]
OK	0.7530	[0.109]
OR	0.8800	[0.058]
PA	0.8037	[0.038]
RI	0.6687	[0.027]
SC	0.3647	[0.399]
SD	0.9282	[0.089]
TN	1.0000	[0.000]
TX	0.4994	[0.151]
UT	0.8806	[0.035]
VA	0.6097	[0.045]
VT	0.6666	[0.130]
WA	0.9156	[0.055]
WI	0.5125	[0.063]
WV	0.4561	[0.036]
WY	0.0000	[0.000]

[1] Standard deviations are in brackets.

Biography

Tianyan Hu was born May 6, 1986 in Jiaxing, P.R. China. She attended Shanghai Jiao Tong University in Shanghai, P.R. China, during which time she was selected as an exchange student , and went to study at University of Victoria, Canada for a semester in 2006. She received her Bachelor of Science in Finance in June of 2008.

She was admitted into the Economics Ph.D. Program in the College of Business and Economics at Lehigh University, where she took courses, passed microeconomics and macroeconomics comprehensive exams as well as health economics field exam. Her research focused on various health policy analyses and the advancement of modern econometrics models. She presented her third year paper in the spring semester of 2011, and proposed dissertation topics in the fall semester of 2011.

During her graduate studies, Tianyan was a teaching assistant during school year and taught recitation for Introduction to Economics and Introduction to Finance. She also taught summer online course Introduction to Economics independently in the summer of 2012. She worked as a health scientist at National Center for Health Statistics, CDC in Hyattsville, MD in the summers of 2011 and 2012, where she accessed data and worked on her dissertation.

Tianyan's research led to two publications during her graduate study. Tianyan presented her work at various conference, including American Society for Health Economist Conference, Eastern Economics Association Annual Meeting, Southern Economics Association Annual Meeting and the Association for Public Policy Analysis & Management Fall Research Conference. She was also invited to present her work at Johns Hopkins University and National Center for Health Statistics, CDC.