ABSTRACT

Title of dissertation: ESSAYS ON THE CONSEQUENCES OF PUBLICLY PROVIDED PRESCRIPTION DRUG INSURANCE

Abby Elizabeth Alpert, Doctor of Philosophy, 2011

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Chapter 1: The Anticipatory Effects of Medicare Part D on Drug Utilization

This paper quantifies the anticipatory effects of the passage of Medicare Part D on prescription drug utilization. Part D expanded Medicare to include insurance coverage for prescription drugs for the first time. While the program was implemented in 2006, it had been signed into law two years earlier in December 2003 as part of the widely publicized Medicare Modernization Act. The advance announcement of this permanent future price reduction may have induced forward-looking individuals to change their drug spending *before* Part D took effect. In a life-cycle demand framework, this pre-reform utilization response is theoretically ambiguous due to opposing income and intertemporal substitution effects. In this paper, I estimate the causal utilization response to the announcement of Part D in 2003 using data from the MCBS and MEPS. This contrasts with previous evaluations of Part D, and of drug co-insurance changes more broadly, which have estimated only contemporaneous utilization effects, thus implicitly assuming a myopic policy response. My main empirical strategy exploits the predicted differential responses of chronic and acute drugs to anticipated future prices. Given that acute drugs treat illnesses that are largely unpredictable and short in duration, their demand is more likely to respond to only current prices, whereas chronic drug use may respond negatively or positively to anticipated future price reductions. I find evidence of an overall decline in drug use for Medicare beneficiaries between 2003 and 2005. As predicted, this pre-reform decline is differentially driven by reductions in chronic drug use, while acute drugs are responsive to only price changes at the time of program implementation. The effect is also concentrated among the youngest Medicare beneficiaries, for whom the health costs of delaying treatment are lowest, and for those with below-median incomes. After accounting for this negative anticipatory response, I

find a total treatment effect on utilization in the first year of the program that is substantially smaller than if anticipation effects are ignored.

Chapter 2: Perverse Reverse Price Competition: Average Wholesale Prices and Medicaid Pharmaceutical Spending (with Mark Duggan and Judith Hellerstein

Generic drugs comprise an increasing share of total prescriptions dispensed in the U.S., rising from nearly 50 percent in 1999 to 75 percent in 2009. The generic drug market has typically been viewed as a mostly competitive market with price approaching marginal costs. However, the large presence of third party payers as final purchasers may distort prices and market shares relative to what a standard model of price competition would predict. In this paper, we investigate how generic drug producers compete in the presence of the procurement rules of the Medicaid program. Medicaid reimbursement to pharmacies, like that of other payers, is based on a benchmark price called the average wholesale price (AWP), which is published in several pricing catalogues. This list price is reported by generic producers themselves, and until recently has been subject to essentially no independent verification. As a result, generic producers have had an incentive to compete for pharmacy market share by reporting AWPs that are much greater than actual average prices, as this spread leads to larger pharmacy profits. In 2000, after a federal government audit of actual wholesale prices of generic products, states were advised to reduce Medicaid reimbursement by as much as 95% for about 400 generic and off-patent injectable, infusion, and inhalation drug products. We use variation induced by the timing of this policy along with its differential impact on drug products to identify the impact of this exogenous price change on the market share of affected products. Our findings indicate that pharmacies respond to the perverse incentives of the Medicaid program by stocking products with the highest AWPs.

ESSAYS ON THE CONSEQUENCES OF PUBLICLY PROVIDED PRESCRIPTION DRUG INSURANCE

by

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Chapter 1

Introduction

One of the most dramatic changes in health care over the last 50 years has been the almost exponential rise in prescription drug spending and utilization (Figure 1.1). Nearly 75% of the growth in spending since 1960 has occurred in the last 15 years. For most of the last three decades, drug spending has also grown faster than all other medical expenditures with an average annual growth rate of 7% compared to 4% for medical care (Figure 1.2). Prescription drugs are inherently different from other types of medical care. The unique features of this market– such as rapid innovation, stringent patent protection, and widespread patient and physician directed advertising– may help to explain part of the dramatic rise in spending over the last half century.

During this same time span, the government's role in paying for prescription drugs has expanded substantially beginning with the introduction of Medicaid in 1965. The introduction of Medicare Part D in 2006 increased the government's role even further. In 2009, these two programs paid for nearly one-third of the $250 billion of total prescription drug expenditures in the U.S (Figure 1.3). The government's share of prescription drug spending will continue to rise when the Affordable Care Act expands Medicaid coverage in 2014 and closes the "donut hole" gap in Part D coverage by 2020.

Understanding the impacts of this significant rise in public sector spending on prescription drugs is important for policy decision-making. This dissertation contributes to the body of literature examining the consequences of publicly provided drug insurance on drug utilization and drug prices. In the first chapter, I quantify the anticipatory effects of the passage of Medicare Part D on prescription drug utilization. Part D was implemented two years after it was signed into law as part of the widely publicized Medicare Modernization Act in 2003. I examine how the advance announcement of this program may have induced forward-looking individuals to change their drug spending *before* Part D took effect. Estimating this effect is of interest because it has important consequences for estimating the overall program impact. In the second chapter, I present the results from research with Mark Duggan and Judith Hellerstein investigating how generic drug producers compete in the presence of the procurement rules of the Medicaid program. Distortions to generic drug prices have important consequences for Medicaid spending growth.

The first chapter estimates the causal utilization response to the announcement of Part D in 2003. Given the two-year lag in implementation, anticipation of the permanent future price change brought about by subsidized coverage may have induced individuals to change their drug consumption patterns in advance of the program's implementation. In a life-cycle demand framework, this pre-reform utilization response is theoretically ambiguous due to opposing income and intertemporal substitution effects. On the one hand, individuals might delay initiating therapies for which they are newly eligible or reduce the use of ongoing medications until after the program is implemented, when the price is lower. This intertemporal

substitution effect would lead to a pre-program decline in utilization. On the other hand, since Part D lowered the cost of drugs in all future periods– reducing the lifetime cost of long-term therapies and increasing lifetime income – individuals might begin drug therapies that they would not have otherwise started or initiate them earlier, leading to a pre-program increase in utilization.

Estimating the announcement effect of Part D is of interest for two main reasons. First, the idea that policy announcements can have quantitatively important effects (separate from their implementation effect) on outcomes has been relatively unexplored in the program evaluation literature. Many public policies– such as changes to the minimum wage, taxes, welfare benefits, and so forth– are implemented with a lag from their enactment date. Importantly, the health care reform legislation that was signed into law in March 2010 will not have its major provisions implemented until 2014 – thus, providing ample time for anticipatory responses along many dimensions such as decisions about the purchase of health insurance, the timing of medical care, and labor supply; and supply-side decisions about pricing, employer insurance offerings, and innovation. While economists acknowledge the potential bias from anticipation, there are few studies that explicitly estimate anticipation effects. More specifically, anticipation effects have not been accounted for in previous studies of the utilization effect of Part D– which may lead to biased estimates of the program impact.

The second reason that the announcement effect is of interest is that it provides the first test of forward-looking behavior in the context of drug demand. Finding evidence of an anticipatory response to Part D would demonstrate that

individuals make drug consumption decisions in a life-cycle framework. In models of intertemporal labor supply and dynamic commodity demand more generally, optimizing individuals trade-off present and future consumption (or leisure) based on their knowledge of the lifetime path of prices. However, it is not known whether individuals are responsive to expectations of future prices when determining current drug consumption levels. Exploiting the widely publicized announcement of Part D as a forecastable permanent reduction in the future out-of-pocket price of drugs provides a strong test.

In my main empirical approach, I test for an anticipatory response by exploiting the predicted differential responses of chronic and acute drugs to anticipated future prices. This strategy makes use of the observation that acute drugs treat illnesses that are largely unpredictable and short in duration, thus their demand is more likely to respond to only current prices, whereas chronic drugs– which treat long duration illnesses– may respond negatively or positively to anticipated future price reductions.

The results of this chapter demonstrate a marked decline in drug use following the announcement of Part D, in addition to reversion towards long-run utilization trends after the program was implemented. These results are consistent with a dominating intertemporal substitution effect– that is, individuals deferring drug use to the time period with subsidized coverage. Moreover, the anticipatory effects are largest for the youngest elderly, for whom the costs of delaying treatment are lowest, and for those with the lowest income. In the difference-in-difference results, I find a nearly 7 percent statistically significant decline in chronic drug use relative to acute

drug use after the announcement. This is supportive of the prediction that chronic drugs are more responsive to future prices than acute drugs. After accounting for this anticipatory response, the implementation effect shrinks by about one-half, suggesting a potentially large upward bias in previous studies that evaluate the first or second year impacts of the program.

Finally, I evaluate two alternative supply-side explanations for the observed negative utilization response. Pharmaceutical firms may have begun to increase prices after the announcement in anticipation of Part D, thus generating a contemporaneous negative demand effect. However, I do not find empirical support for this explanation, given that price growth changes after 2003 were negative and statistically insignificant for drugs differentially used by Medicare beneficiaries. I also consider the possibility that insurers discontinued drug coverage or reduced benefit generosity before the implementation of Part D, thus increasing out-of-pocket costs. While I find that there was a small decline in certain types of drug insurance coverage, this change is not large enough to explain the reduction in drug utilization. Moreover, neither of these supply-side responses can explain the differential effect for chronic and acute drugs. The results of this chapter demonstrate that drug utilization responds to predictable changes in future drug prices in an economically meaningful way. I find strong evidence of a negative anticipatory response to Part D and show that the total treatment effect on utilization in the first year of the program is substantially smaller than if anticipation effects are ignored.

In the second chapter, Mark Duggan, Judith Hellerstein, and I study the impact of Medicaid procurement on generic price competition. In particular, we hy-

pothesize that procurement rules may distort generic drug prices away from marginal cost by perversely rewarding higher-priced generics with greater market share. Medicaid reimbursement for each prescription is based on a benchmark price called the average wholesale price (AWP). For each product, this list price is reported to the catalogues by generic manufacturers themselves, and until recently has been subject to essentially no independent verification of its resemblance to the actual average price that pharmacies pay manufacturers to acquire drugs. As a result, generic manufacturers have had an incentive to compete for pharmacy market share by reporting AWPs that are much greater than actual average prices, as higher "spreads" lead to larger pharmacy profits. Put another way, since higher AWPs generate higher reimbursement for pharmacies, manufacturers might report higher and higher AWPs in order to induce pharmacies to stock their drug rather than a competitor's drug. Thus, competition among manufacturers may increase rather than reduce the prices on which Medicaid reimbursement is based, leading to inflated Medicaid spending.

To empirically investigate the impact of Medicaid procurement on price competition, we examine an intervention that caused a sharp decline in price for a well-defined set of generic drugs. In the late 1990s, an investigation by the Department of Justice (DOJ) and the National Association of Medicaid Fraud Control Units (NAMFCU) "revealed a pattern of misrepresentations by some drug manufacturers of the average wholesale prices and wholesale acquisition costs of certain of their products."[1] As a result of this audit, in May 2000, states were advised to reduce the AWP used to reimburse pharmacies by as much as 95% for approxi-

[1]Office of the Attorney General, Medicaid Fraud Control Unit, State of New York, 2000.

mately 400 generic and off-patent injectable, infusion, and inhalation drug products. This intervention provides a useful setting for our study because it differentially targeted drugs within classes of bioequivalent products, allowing for a comparison of drug purchases before and after the intervention for DOJ targeted drugs and their competitors.

Using more than a decade of Medicaid State Drug Utilization Data from the Centers for Medicare and Medicaid Services (CMS), we demonstrate that actual reimbursement per prescription purchased declined substantially for targeted drugs following the 2000 DOJ recommendations, relative to competitor products whose AWPs were not targeted by the DOJ. Also after the intervention, we find evidence of a decline in the number of targeted drug prescriptions dispensed by pharmacies and an increase in the number of competitor drugs dispensed. Overall, the market share for targeted drugs fell by about 45% through 2004 relative to the baseline year. While the findings in this paper are preliminary, they suggest that pharmacies substituted away from drug products whose prices were reduced. This is inconsistent with a standard model of price competition in which lower-priced drugs capture higher market share. Thus, these findings provide preliminary evidence that Medicaid procurement incentives could lead generic manufacturers to compete by overstating AWPs and thus reducing the cost savings from these drugs.

Figure 1.1: Prescription Drug Expenditures in the U.S., 1960-2009

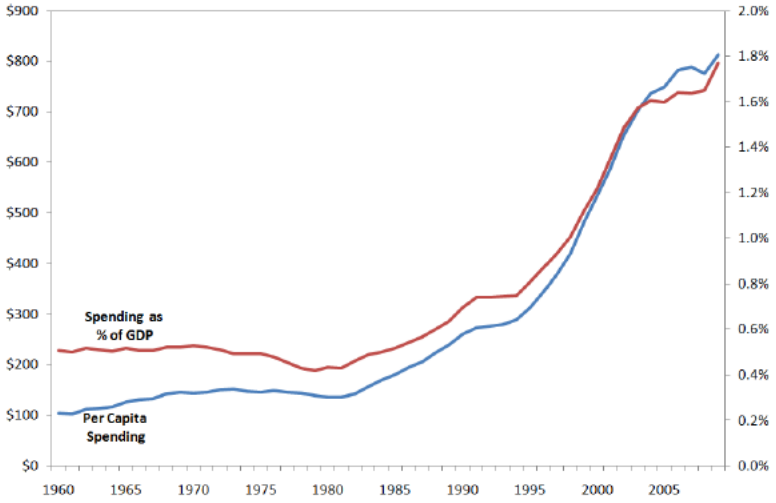
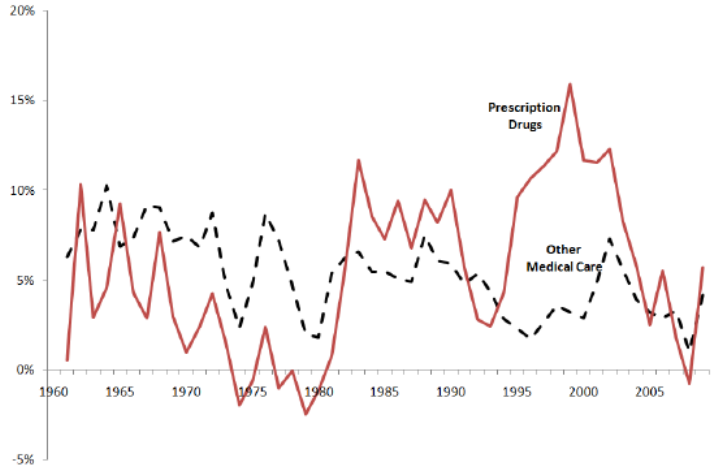
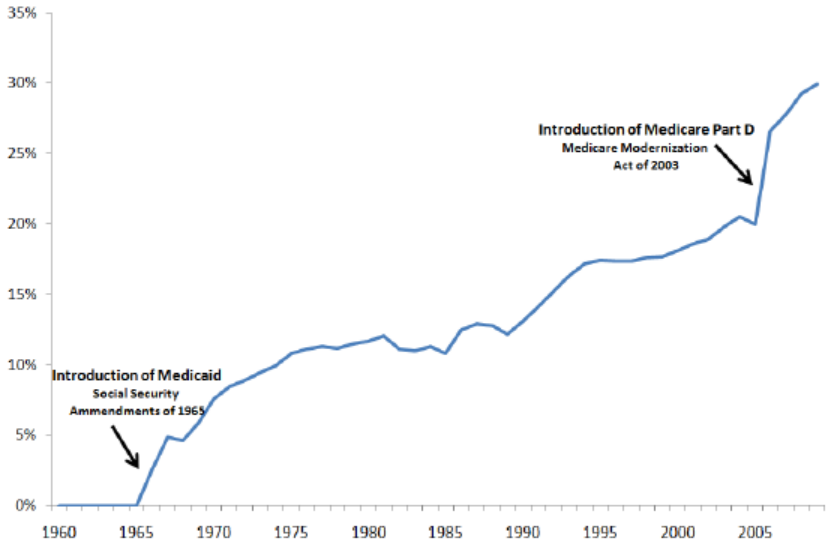


Figure 1.2: Annual Percentage Growth in Expenditures for Drugs and Other Medical Care, 1960-2009



Notes: Source is National Health Expenditure Accounts, adjusted for inflation using the CPI-U.

Figure 1.3: Public Share of Drug Expenditures, 1960-2009



Notes: Source is National Health Expenditure Accounts.

Chapter 2

The Anticipatory Effects of Medicare Part D on Drug Utilization

2.1 Introduction

In December 2003, the Medicare Prescription Drug, Improvement, and Modernization Act (MMA) was signed into law, expanding the Medicare program to include insurance coverage for prescription drugs. At a cost of $32 billion in the first year, this program, known as Part D, substantially reduced the out-of-pocket price of drugs for beneficiaries. Part D was not implemented until January 1, 2006. Given the two-year lag in implementation, anticipation of the permanent future price change brought about by subsidized coverage may have induced forward-looking individuals to change their drug consumption patterns *before* Part D took effect. In a life-cycle demand framework, this pre-reform utilization response is theoretically ambiguous due to opposing income and intertemporal substitution effects. This paper seeks to estimate the causal utilization response to the announcement of Part D in 2003 using detailed drug utilization data from the Medicare Current Beneficiary Survey (MCBS) and the Medical Expenditure Panel Survey (MEPS). This contrasts with previous evaluations of Part D, and of drug co-insurance changes more broadly, which have estimated only contemporaneous utilization effects, thus implicitly assuming a myopic policy response.

Estimating the announcement effect of Part D is of interest for two main rea-

sons. First, the idea that policy announcements can have quantitatively important effects (separate from their implementation effect) on outcomes has been relatively unexplored in the program evaluation literature. Many public policies– such as changes to the minimum wage, taxes, welfare benefits, and so forth– are implemented with a lag from their enactment date. This lag can extend from months to years. For example, the health care reform legislation that was signed into law in March 2010 will not have its major provisions implemented until 2014 – thus, providing ample time for anticipatory responses along many dimensions such as decisions about the take-up of health insurance, the timing of medical care, and labor supply; and supply-side decisions about pricing, [1] employer insurance offerings, and innovation.

There are numerous examples in which anticipatory behavior may affect the evaluation and interpretation of program treatment effects. An extreme example of "implementation lag" are the Social Security Ammendments of 1983 which increased the full retirement age for cohorts retiring two decades later. In this case, the effects of the policy on retirement behavior were likely attenuated by decades of anticipatory consumption-smoothing (Mastrobuoni, 2009). In another example of the quantitative importance of anticipatory behavior, a small literature has arisen examining "timing" responses versus "real" responses to the Tax Reform Act of 1986, which was phased in over two years (e.g. Slemrod, 1995; Scholes, Wilson, and Wolfson, 1992). The advance announcement of the reform enabled firms and indi-

[1] Perhaps partly out of concern of anticipatory price hikes, insurers are required to report their medical loss ratios beginning in 2010 and provide rebates for ratios below specified limits after January 1, 2011.

viduals to postpone income to periods when the income tax rate had been lowered or accelerate investments and capital gains before the capital gains tax increase took effect. These timing responses present a challenge to quantifying the real "income creation" response or revenue consequences of the tax policy. In order to estimate this effect, as noted in Slemrod (1995), capital gains realizations shifted to the pre-policy period must not be counted as revenue losses and income postponed to the post-policy period must not be counted as gains. Similarly, in estimating the real utilization impact of Part D, drug use that is simply deferred to periods when the costs are subsidized should not contribute to measurements of the total effect of the program. Furthermore, any anticipatory increases in drug use due to, for example, the income effects of the program should not be excluded from the treatment effect. In another recent paper, Blundell, Francesconi, and van der Klaauw (2010) also examine the consequences of anticipatory responses to policy announcements, in the case of welfare reform in the UK. Their model allows for outcomes to be a function of beliefs about the likelihood of future policy changes in addition to the actual benefits derived from policy implementation. Policy announcements change the information set available to agents in forming their beliefs about future policy changes. Thus, valid inference of the total program effect requires acknowledging changing information sets and thus selecting identification strategies that take into account anticipatory responses.

The second reason that the announcement effect of Part D is of interest is that it provides the first test of forward-looking behavior in the context of drug demand. Finding evidence of an anticipatory response to Part D would demonstrate that

individuals make drug consumption decisions in a life-cycle framework. In models of intertemporal labor supply and dynamic commodity demand more generally, optimizing individuals trade-off present and future consumption (or leisure) based on their knowledge of the lifetime path of prices.[2] However, it is not known whether individuals are responsive to expectations of future prices when determining current drug consumption levels. Exploiting the widely publicized announcement of Part D as a forecastable permanent reduction in the future out-of-pocket price of drugs provides a strong test.[3]

I hypothesize that drug utilization can respond negatively or positively to the announcement of Part D. On the one hand, individuals might delay initiating therapies for which they are newly eligible or reduce the use of ongoing medications until after the program is implemented, when the price is lower. This intertemporal substitution effect would lead to a pre-program decline in utilization. On the other hand, since Part D lowered the cost of drugs in all future periods– reducing the lifetime cost of long-term therapies and increasing lifetime income – individuals might begin drug therapies that they would not have otherwise started or initiate them earlier, leading to a pre-program increase in utilization. This positive effect would be magnified for drugs that exhibit strong complementarities in marginal health benefits across time periods (Becker and Murphy, 1988). Nevertheless, given the

[2]See for example MaCurdy (1981), Altonji (1986), Hotz et al. (1988), Ham and Reilly (2002) for models of life-cycle labor supply and empirical estimates of intertemporal wage elasticities. Friedman (1957), Hall (1978), and others highlight the role of expectations about future income in explaining current consumption behavior.

[3]The lag between the announcement and implementation of policies has also been used in tests of the "rational addiction" model (Gruber and Koszegi, 2001) and the life-cycle hypothesis for commodity demand (Wilcox, 1989). These papers exploit the advance announcement of state cigarette tax changes and changes to social security benefits, respectively.

health costs associated with delaying treatments and the fact that liquidity constraints may prevent individuals from consuming out of increases in future income, it remains to be seen whether these effects exist empirically. Still, a large empirical literature has demonstrated that individuals forgo drugs and medical care following price increases (Goldman, Joyce, Zheng, 2007). It seems that a less extreme finding would show that changes in the relative prices across periods alter the timing of drug treatment rather than cause individuals to forgo treatment altogether.

In my main empirical approach, I propose a test for an anticipatory response that exploits the predicted differential responses of chronic and acute drugs to anticipated future prices. This strategy makes use of the observation that acute drugs treat illnesses that are largely unpredictable and short in duration, thus their demand is more likely to respond to only current prices, whereas chronic drugs– which treat long duration illnesses– may respond negatively or positively to anticipated future price reductions. This test is similar in spirit to Sorensen (2000) who also exploits variation in drug characteristics– in his case, the frequency of purchases– to estimate the impact of search costs on pharmacy price dispersion. In another test, I use a similar difference-in-difference strategy to compare changes in utilization for the elderly who are currently eligible for Medicare relative to the near-elderly who are not yet eligible, with the caveat that the near-elderly may also be responsive to the expectation of future price changes that are available upon becoming eligible for Medicare. I test for this directly by comparing the anticipatory responses of near-elderly who are very close to becoming eligible for Medicare with near-elderly who are further away from the eligibility threshold. The age-eligible and age-ineligible

comparison has been employed in most of the previous studies of Part D. However, the three previous studies that have used this strategy estimate only the contemporaneous response to the policy, thus potentially overstating or understating the total effect of Part D depending on the sign of the anticipatory effect.

The results of this study indicate a substantial negative announcement effect in the aggregate, in addition to reversion towards long-run utilization trends after the program was implemented. These results are consistent with a dominating intertemporal substitution effect– that is, individuals deferring drug use to the time period with subsidized coverage. Moreover, the anticipatory effects are largest for the youngest elderly, for whom the costs of delaying treatment are lowest, and for those with the lowest income. In the difference-in-difference results, I find a nearly 7 percent statistically significant decline in chronic drug use relative to acute drug use after the announcement. This is supportive of the prediction that chronic drugs are more responsive to future prices than acute drugs. After accounting for this anticipatory response, the implementation effect shrinks by about one-half, suggesting a potentially large upward bias in previous studies that evaluate the first or second year impacts of the program. This finding is robust to several different specifications and sensitivity tests. The results for the age group comparisons also show a decline in utilization for the elderly relative to those aged 50-58. I also find evidence that adults who are nearing Medicare eligibility (aged 59-64) have a negative anticipatory response to the announcement of Part D relative to younger adults.

Finally, I evaluate two alternative supply-side explanations for the observed

negative utilization response. Pharmaceutical firms may have begun to increase prices after the announcement in anticipation of Part D, thus generating a contemporaneous negative demand effect. However, I do not find empirical support for this explanation, given that price growth changes after 2003 were negative and statistically insignificant for drugs differentially used by Medicare beneficiaries. I also consider the possibility that insurers discontinued drug coverage or reduced benefit generosity before the implementation of Part D, thus increasing out-of-pocket costs. While I find that there was a small decline in certain types of drug insurance coverage, this change is not large enough to explain the reduction in drug utilization. Moreover, neither of these supply-side responses can explain the differential effect for chronic and acute drugs. The results of this paper demonstrate that drug utilization responds to predictable changes in future drug prices in an economically meaningful way. I find strong evidence of a negative anticipatory response to Part D and show that the total treatment effect on utilization in the first year of the program is substantially smaller than if anticipation effects are ignored.

The remainder of the paper proceeds as follows. Section 2.2 provides detailed background about Medicare Part D and the related literature. Section 2.3 describes the conceptual framework for estimating and evaluating anticipatory effects. Details about the data and descriptive statistics are included in Section 2.4 and Section 2.5 outlines the empirical methods. Sections 2.6 and 2.7 present the results. Section 2.8 concludes.

2.2 Background

2.2.1 Program Coverage and Participation

Medicare is an over $500 billion federal program that provides health insurance to the elderly, aged 65 and over, and qualifying non-elderly disabled individuals.[4] The traditional program consists of two fee-for-service components, Part A and Part B, which together cover most medical services including hospital stays, doctors' visits, nursing facilities, and home health care. Part B also covers drugs that are physician-administered such as cancer and immunosuppressive drugs. A notable excluded benefit from Parts A and B is outpatient prescription drugs.[5] Despite having become an increasingly large component of health care spending for the elderly, outpatient prescription drugs were not covered by traditional Medicare until the introduction of Part D in 2006. After the implementation of Part D, Medicare's share of total national spending on prescription drugs increased from 2% in 2005 to 22% in 2006 (KFF, 2007). The cost of the program was projected to be $780 billion over the first 10 years.

Enrollment in Part D is voluntary. However, in effort to mitigate adverse se-

[4]The disabled comprise 16% of enrollment (SSA Annual Statistical Supplement, 2008). Individuals with end-stage renal disease are also covered under Medicare. They make up less than 1 percent of enrollees.

[5]Some outpatient prescription drug coverage has been provided through Part C, also known as Medicare Advantage (MA). Part C is an alternative to Parts A and B in which beneficiaries receive Medicare benefits through a private managed care plan. Part C plans are required to provide a minimum level of services, but may also augment their coverage with extra benefits such as prescription drugs. Only about 24 percent of Medicare eligibles choose to enroll in Part C over the traditional plan (CMS, 2009). During the study period, average MA penetration was about 12% from 2003 to 2005 and 16% in 2006 (Brown et al., 2010). In 2003, 69 percent of Part C enrollees in basic plans received drug coverage with 60 percent of plans covering only generic drugs (Achman and Gold, 2003).

lection, the program encourages take-up by raising the base premium incrementally for each month that enrollment is delayed beyond initial eligibility.[6] This penalty is waived if beneficiaries can demonstrate access to actuarially equivalent coverage elsewhere. By January 2007, 54% of Medicare beneficiaries had enrolled in Part D (KFF, 2007), over one-third of whom did not have any source of drug insurance two years earlier (Levy and Weir, 2009). Individuals who were dually eligible for Medicaid and Medicare were automatically enrolled in Part D and most Medicare Advantage (Part C) plans began to offer Part D benefits, thus immediately covering those who stayed enrolled in their plans (L&W, 2009).[7] Enrollment in Part D was also high among those who were previously covered by a private Medigap prescription drug plan.[8] Medicare beneficiaries who had received drug benefits from employer-sponsored insurance were least likely to take-up Part D, with only 19 percent enrolling in 2006 (Levy and Weir, 2009). This low participation rate can likely be attributed to the government's efforts to prevent crowd-out of private insurance by subsidizing employers who continued to provide coverage equivalent to Part D.[9] Levy and Weir (2009) estimate that the fraction of the elderly who were drug-uninsured declined from 24% to 7% in the first year of Part D.

[6]In a policy simulation, Lucarelli (2006) predicts that the 1% premium increase per month of delayed take-up has no impact on front-loading enrollment.

[7]Beneficiaries must give up MA medical coverage if they enroll in a stand-alone PDP.

[8]Medigap plans were required to discontinue prescription drug coverage after Part D was implemented.

[9]Firms received a subsidy, known as the Retiree Drug Subsidy (RDS), equal to 28 percent of the cost of providing insurance for expenses between $500 to $5,000 per beneficiary (KFF, 2006).

2.2.2 How Did Part D Lower Drug Costs?

Part D is administered by stand-alone private drug plans (PDPs) and Medicare Advantage plans (MA-PDs) that compete for Medicare enrollees within defined regions of the U.S. In 2006, beneficiaries could choose from among at least 40 PDPs and one or more MA-PDs in most states (KFF, 2006). The program lowered the out-of-pocket cost of drugs for enrollees primarily through two mechanisms.[10] The first was through the coinsurance design. All plans must offer a benefit that is at least actuarially equivalent to a standard benefit defined by Medicare. The standard benefit, illustrated in Figure 2.1, provides a drug subsidy that is non-linear in annual expenditures. Plans typically require a monthly premium, which was on average $32 (or $384 annually) in 2006 (KFF, 2006). The first $250 of drug expenditures are borne fully out-of-pocket, while the next $2000 are subsidized by 75 percent. After reaching a spending threshold of $2,250, the beneficiary enters what is known as the "donut-hole" in which he again bears 100 percent of the costs. After $5,100 in total drug spending, catastrophic coverage begins and a 95 percent subsidy takes effect for all remaining expenditures for the year. Many plans differ from this standard design, for example, by offering flat copays for different drugs in the first region rather than 25% coinsurance (KFF, 2006). Low-income beneficiaries receive additional subsidies, such as reduced or zero premiums and deductibles, smaller coinsurance, and subsidized coverage in the "donut hole" region. Medicaid beneficiaries make-up half of those eligible for the low-income subsidy (Duggan et al., 2008).

[10] Part D may have also indirectly lowered out-of-pocket costs for individuals who *did not* enroll in Part D if employers increased plan generosity in response to the Retiree Drug Subsidy.

During the transition from the announcement to implementation of Part D, prescription drug discount cards were also offered as a provision of the MMA. Beginning in June 2004, Medicare beneficiaries could enroll for a small fee in a plan that offered discounts off the retail price of drugs at the point-of-sale, with estimated savings of approximately 17% (Cubanski et al., 2004). Beneficiaries with income below 135% of the federal poverty level who were not covered by Medicaid or other drug insurance plans were eligible for an additional $600 transitional assistance (TA) subsidy for drug purchases. Take-up of the drug discount program was low. Only 5.8 million Medicare beneficiaries had enrolled in the program six months after it was introduced, with the vast majority automatically enrolled by their MA plans, state pharmacy assistance plans, or by CMS due to their low income status (Thomas, 2005). Moreover, many of the automatic enrollees did not activate their discount cards.[11] The drug discount program was discontinued when Part D was fully implemented in 2006.

In addition to lowering enrollees' out-of-pocket payments mechanically through the coinsurance design, PDPs and MA-PDs could also lower spending by using their bargaining power to negotiate lower prices from manufacturers.[12] Formularies are an instrument that can facilitate this. They shift enrollees' utilization towards particular drug products by designating different levels of cost-sharing across different therapeutically-similar drugs (e.g. one cholesterol-lowering drug may be designated

[11] For example, of the nearly two million low-income beneficiaries who were automatically enrolled by CMS in September 2004, only 100,000 had activated their card by a phone call by December 2004 (Thomas, 2005).

[12] Plans can also restrict participating pharmacy networks to leverage additional discounts from pharmacies.

by the plan as a "preferred brand" and thus require a smaller copay than other therapeutically similar "non-preferred" brands).[13] Drugs that are least costly or have favorable therapeutic benefits are placed on "preferred" tiers of the formulary. Drug manufacturers, in turn, compete for placement on the "preferred", low copay tiers – where demand for their product will likely be higher– by lowering prices or offering discounts and rebates directly to drug plans. Duggan and Scott-Morton (2010) show evidence that this type of strategic behavior has led to a reduction in prices of brand name drugs by approximately 20% for enrollees who moved from not having drug insurance to Part D.

Together, the coinsurance design and strategic behavior of plan providers have contributed to a 20.5% decline in the share of drug spending that is paid for out-of-pocket by the non-institutionalized 65 and over population between 2005 and 2006. This can be seen in Figures 2.2 and 2.3, which plot the age-profile of out-of-pocket spending using MEPS data. In comparison, the out-of-pocket share decreased by 2.2% for the near-elderly (ages 55-64) during this time period. In levels, the introduction of Part D appears to have scaled back annual out-of-pocket spending to approximately 2000 levels.[14] Other studies have found a decline in elderly spending of 13 to 22% (Yin et al 2008; Ketcham and Simon 2008).

[13]Formularies also lower utilization by assigning some drugs prior-approval requirements or step-therapy.

[14]In the MEPS, mean out-of-pocket spending for the 65 and over population declined by $224 between 2005 and 2006. Median, 25th percentile, and 95th percentile out-of-pocket spending declined by $49, $22, and $887, respectively.

2.2.3 Previous Evaluations of the Part D Utilization Effect

Given the large decline in the out-of-pocket price of drugs, we would expect to see an increase in the demand for prescription drugs through substitution and income effects. High rates of drug non-compliance and sub-optimal take-up of medically beneficial therapies among the elderly prior to Part D (Adams et al, 2001; Mojtabai and Olfson, 2003), combined with moral hazard effects, suggest that this utilization effect potentially could be large. A large body of literature has estimated the insurance price elasticity of drug demand in other contexts. In a recent meta-analysis of over 100 studies, Goldman, Joyce, Zheng (2007) report elasticities ranging from -0.2 to -0.6.[15]

Several studies have evaluated the impact of the implementation of Part D on drug utilization in the first and second years of the program. The three most widely cited studies employ a difference-in-difference strategy comparing changes in drug use right before and after the implementation of Part D for the elderly relative to the near-elderly who are not yet eligible for Medicare (Lichtenberg and Sun, 2007; Yin et al, 2008; Ketcham and Simon, 2008). Using a large sample of claims from the Walgreens pharmacy chain or Wolters Kluwer Health (representing 1.4 billion prescriptions), these studies have estimated an aggregate increase in utilization of 4-

[15]The case of Part D differs from the previous literature in several ways. First, only a handful of studies have estimated the price elasticity of demand for the elderly (Artz et al, 2002; Thomas, 2002; Balkrishnan et al 2001; Johnson et al 1997a; Johnson et al 1997b; Coulson, et al 1995). Second, unlike most other studies, the change in price was very large. Moreover, Part D represents a rare event in which prices were actually lowered for beneficiaries (GJZ, 2007). Finally, the MMA varied drug prices for Medicare beneficiaries, while holding prices for other medical goods constant. Thus, it is possible to isolate the own-price elasticity of demand for prescription drugs from other demand responses such as doctor visits which may indirectly influence drug use. This is a known limitation of the RAND Health Insurance experiment (Leibowitz et al, 1985) which applied the same coinsurance rate to drugs and non-drug medical expenditures.

10% [16] with implied elasticities ranging from -0.2 to -0.7. A fourth recent paper uses the MCBS to compare the previously drug-uninsured elderly with those with drug insurance finding statistically insignificant differences in the aggregate utilization effect across insurance types (Kaestner and Kahn, 2010). Finally, using IMS sales data, Duggan and Scott-Morton (2010) examine whether drug use increased between 2003 and 2006 differentially for drugs that had a higher Medicare market share. They find a large utilization effect that is insignificant and statistically imprecise.

One critical limitation of the previous difference-in-difference studies using the elderly and near-elderly comparison is that they do not possess a long enough time series of data to account for possible anticipation effects. In these studies, the "pre-period" begins in September or December of 2004[17]– nearly one year *after* the announcement of Part D. If the announcement caused Medicare beneficiaries to shift the timing of drug purchases until after implementation, leading to a transitory pre-implementation decline in utilization, the DID estimator will overstate the program effect. The near-elderly "comparison group" is not an adequate control for anticipatory responses by the elderly "treated group" because those who are not yet eligible for Part D would not be expected to respond to the announcement with the same intensity as those who are already eligible. By not accounting for anticipatory effects, the DID estimate will falsely attribute the upward reversion in drug use following the pre-reform decline to the Part D program effect. This identi-

[16]These estimates are from the age-standardization that Ketcham and Simon (2008) perform to compare the results of Yin et al. (2008) and Lichtenberg and Sun (2007).

[17]The pre-period data for the 3 DID studies comparing the elderly and near-elderly are as follows: September 2004-December 2005 (Lichtenberg and Sun, 2007, Yin et al. 2008); December 2004-December 2005 (Ketcham and Simon, 2009).

fication problem is structurally similar to the "pre-program dip" (also known as the "Ashenfelter dip") that has been widely discussed in the literature evaluating job training programs (see for example Ashenfelter, 1978; Heckman and Smith, 1999). Conversely, if the announcement caused beneficiaries to increase drug use in the pre-implementation period, the DID estimator will understate the program effect, since part of the real impact of Part D occurs before the program is implemented. Thus, using only a small window of data around the implementation date generates biased and variable treatment effect estimates if there is an anticipatory response.

2.3 Accounting for Anticipatory Responses

2.3.1 Conceptual Framework

The implicit assumption in the previous studies that compare drug utilization right before and after the implementation of Part D is that individuals respond myopically to the policy change. I take a more dynamic view. Given that the lag in program implementation allowed individuals to forecast price changes two years in advance, individuals' demand for prescription drugs may respond not only to current prices, but also to expectations of future prices. Thus, estimates of the treatment effect of Part D on drug utilization should combine the net effect of the anticipatory response and the response at implementation.

A. Life-Cycle Demand for Prescription Drugs

The notion that future prices can affect present behavior is well-established. This

idea is central to models of dynamic commodity demand and intertemporal labor supply, beginning with Lucas and Rapping (1970) and Friedman's "permanent income hypothesis" (1957). The large theoretical and empirical literature that has followed for labor supply is surveyed in Card (1991). These models emphasize the role of the lifetime path of prices in the allocation of consumption and leisure over the life-cycle. Similarly, the demand for healthcare and prescription drugs is part of a life-cycle decision-making process (Grossman, 1972).[18] For forward-looking individuals, current demand should be a function of everything that is known about the lifetime path of prices– past, present and future. Then, through intertemporal substitution, individuals allocate greater drug use to periods when drugs are cheaper and less drug use to periods when drugs are more expensive, all else equal.

The sudden announcement of Part D in 2003 changed individuals' expectations about the future path of prices for drugs. Since this reform represented a permanent change, it lowered the entire stream of out-of-pocket prices in all future periods beginning in the year of implementation. In response, the life-cycle model predicts that individuals should have immediately used this new information to re-optimize their consumption path as soon as the announcement was made. Moreover, since Part D changed the lifetime price path of drugs for individuals of all ages, it is possible that the announcement affected consumption patterns for even those not yet eligible for Medicare. Though, the short-run effects are likely to be strongest for Medicare beneficiaries and individuals closest to Medicare eligibility. The consump-

[18]Lucarelli (2006) develops a dynamic model of drug demand in the spirit of Grossman (1972) to simulate the demand response to the implementation of Part D. However, like other Part D studies, he does not account for possible anticipatory demand responses in the pre-implementation period.

tion path in the pre-reform period could have shifted downwards or upwards after the announcement due to opposing intertemporal substitution and income effects. This is discussed in detail in the next section.

B. Differential Responses of Chronic and Acute Drugs

The life-cycle model suggests that we should observe a change in drug utilization by the elderly in response to the announcement of Part D. However, from an empirical standpoint, it will be difficult to disentangle aggregate changes in drug utilization caused by anticipatory behavior from other consumption fluctuations that may have occurred during this time period. I propose a way of testing for an anticipatory response (or equivalently, testing for forward-looking life-cycle behavior) that exploits the predicted differential responses of chronic and acute drugs to anticipated future prices. This will form the basis of my main difference-in-difference empirical strategy, since I can compare changes in utilization for drugs in which the announcement of Part D potentially had a larger or smaller effect.

For this analysis, the key difference between acute and chronic drugs is their average duration of use. Acute drugs (e.g. antibiotics) treat illnesses that are largely unpredictable, short in duration, and require immediate treatment; meanwhile, chronic drugs treat long-term illnesses. Put differently, it is typically the case that acute drugs produce a health benefit in only the current period, while chronic drugs can produce health benefits in many periods. Moreover, the persistence of chronic drug use means that current use is a good predictor of future use. Given these features, future prices should have a much larger effect on current use for

chronic drugs than for acute drugs. The effect of the future price reduction could be negative or positive.

First, there will be a negative anticipatory response to Part D if there are dominating intertemporal substitution effects: individuals will delay the use of some drugs until after the program is implemented, when the out-of-pocket price is lower. Furthermore, this effect should be concentrated among chronic drugs since there is less scope for shifting acute drug use to later time periods given the transitory nature of their health benefits. For example, in the intervening period between the announcement and implementation of Part D, individuals may have asked their physicians to delay the initiation of chronic treatments for which they were newly clinically eligible. Individuals may have also reduced their compliance with prescriptions for "less-essential" medications that they believed could be suspended temporarily without posing an immediate health risk.

It should be emphasized that in order for the intertemporal substitution effect to generate a pre-reform decline in utilization relative to the counterfactual trend, it must be the case that elderly who would have otherwise taken a drug or initiated a new treatment in the absence of Part D decided to postpone treatment after learning of the announcement. One concern is that, for drugs that are taken for an entire lifetime, this response would not fit a standard model of rational behavior. Individuals would find it optimal to purchase the drug when the expected lifetime path of prices for that treatment was higher (in the absence of the announcement), but not purchase the drug when the expected lifetime path of prices for that treatment was lower (after learning of the announcement). Nevertheless, most drugs are used for

a finite period of time, at least in expectation, because they fail to be effective with some probability, better drugs enter the market, or their usefulness is eventually outlived. Thus, given the uncertainty of treatment duration, it may be optimal, depending on the health costs, to defer use or experimentation with new treatments of unknown effectiveness to periods when the price of drugs is lower. Moreover, even in the case of a lifetime therapy, we can appeal to behavioral models of contextual price effects (Thaler, 1985) to explain a delay in the timing of purchases. If we consider the announcement of Part D as effectively introducing a new lower "reference price" for drugs, then purchasing drugs before implementation at a price that is higher than the reference price may be perceived as a "loss", which generates so-called transaction disutility. This disutility may consequently reduce drug use.

Second, there will be a positive anticipatory response to Part D if there are dominating income effects. Part D increased lifetime income by lowering the cost of drugs in each period. Since this income effect is distributed across the life-cycle, it could generate increases in both drug purchases and other consumption in any period after the *announcement*. Thus, individuals might begin drug therapies that they would not have otherwise started or initiate them earlier. Importantly, the magnitude of the income effect varies with the size of the expected benefit of Part D. Chronic drug users should anticipate a large subsidy from Part D given the persistence in their drug use; whereas purely acute drug users, facing uncertain future health shocks, may anticipate a much smaller subsidy in expectation.[19] Again,

[19]Stronger income effects can exist for acute drug use by chronic users than for acute drug use by purely acute drug users.

chronic drugs are predicted to be more responsive to the future price change than acute drugs since the income effect will be larger. Also, to the extent that individuals take into account the entire cost of a therapy before deciding whether to initiate a treatment, substitution between drug therapies and other consumption may lead to an increase in chronic drug use. These positive effects would be reinforced for drugs that exhibit strong complementarities in marginal health benefits across time periods. The intuition for this response is analogous to the model for "rational addiction" (Becker and Murphy, 1988). These drugs have the feature that a larger stock of past consumption raises the marginal health benefit from current consumption. Thus individuals who anticipate increasing drug use in the future (because of an anticipated future reduction in price), should increase use in the current period in order to increase the benefit in the next period. Models of habit formation and durable goods (e.g. Browning 1988; Hotz et al., 1991; Pollack, 1970) that also incorporate non-separability of preferences over time are also relevant for explaining patterns in chronic drug use.

To summarize, chronic drugs are predicted to be more responsive to the announcement of Part D than acute drugs and this response may be either negative or positive. This hypothesis will be rejected empirically if elderly are myopic (i.e. they make decisions based only on current prices, even though information about future prices is available), the health costs of delaying drug use exceed the utility gain, or the elderly are liquidity constrained and cannot consume out of increases in future income.

C. Empirical Tests of the Life-Cycle Model

Despite the large empirical literature that has tested the life-cycle model in other contexts, I am aware of only two studies of anticipatory responses to coinsurance price changes in the case of non-drug medical care. No studies have examined the anticipatory demand response to Part D. Long, Marquis, and Rogers (1998) use data from the Survey of Income and Program Participation to compare doctors visits and inpatient admissions among individuals who have just gained or lost insurance relative to those who are continuously insured or uninsured. They find no evidence of an anticipatory effect. However, their method relies on cross-sectional variation in insurance transition types and thus does not account for the likely endogeneity of health insurance initiation and discontinuation. Moreover, it cannot be determined whether these transitions were forecastable. Gross (2009) exploits the forecastable change in health insurance status that occurs when teenagers lose their family's coverage and become uninsured at age 19 to test whether individuals are forward looking in their demand for medical care. Using Medstat data, the study finds no evidence that teenagers "stock up" on health care before losing insurance. However, the author notes that finding an effect is hindered by the fact that so few teenagers in the sample use any medical care. Related to these studies, the RAND Health Insurance experiment has also been criticized for its implicit assumption of myopia in not accounting for within-year price variation for medical care. Kowalski (2009) argues that elasticities from the RAND Health Insurance study may be biased downwards because individuals may actually respond to year-end price–which

is zero if they anticipate reaching the stop-loss– rather than the current price.[20] My study overcomes many of the challenges that the previous studies have faced since the price change resulting from Part D is exogenous, the announcement was highly publicized, and drug use is a highly prevalent and high frequency outcome for the elderly.

2.3.2 Salience and Timing of the Part D Announcement

My test of anticipatory behavior relies on two assumptions: first, that the impending price reduction due to Part D was part of individuals' information sets at the time of the announcement; second, that the policy change was not anticipated before the announcement. In this section, I document that the announcement was highly salient and also that its timing was a surprise. Part D was signed into law as part of the MMA on December 8, 2003. But the program did not actually begin until January 1, 2006, more than 2 years later. This implementation date was stipulated by the MMA and thus was known in advance. Given the wide media coverage of the passage of the legislation, it is reasonable to assume that many elderly anticipated a reduction in their future drug expenditures. Figure 2.4 plots data from a Kaiser Family Foundation Health Poll of the trend in the fraction of elderly who followed the Medicare prescription drug benefit "very closely" or "somewhat closely." The poll demonstrates high awareness of the debate among the elderly with approximately 75 percent following news of the debate. Moreover, the elderly followed the debate

[20]Manning et al (1987) also had noted that the stop-loss may hamper their ability to estimate elasticities accurately.

most closely in the month that the law was signed and least carefully in the months after it was passed suggesting awareness that the debate had ended. Another KFF poll which quizzed[21] individuals about whether the bill had passed 2 months after it was signed into law found that only 32 percent of elderly ages 65+ could correctly respond that the bill had been signed into law, 41 percent were uncertain (among the non-elderly, only 21 percent responded correctly). This is less supportive of a strong awareness of the program announcement. However, even if the elderly were not fully aware of the passage of the MMA, their physicians may have been better informed. Furthermore, the size of the benefit was immediately known as news sources (e.g. Pear, 2003) reported precisely the coinsurance schedule that was ultimately implemented, as described in Section 2.2.

Finally, the timing of the announcement was largely unexpected. This is a necessary feature of the policy in order to pin down the time period in which to estimate the anticipatory effect. Adding prescription drug coverage to Medicare had been the subject of nearly two decades of debate and failed legislative proposals (see Oliver, et al., 2004 for a history of drug initiatives). The prescription drug legislation was highly controversial throughout the debate and press accounts suggest that it was far from certain that a bill would pass at any point in time. The final conference bill that passed in the House and Senate in late November did so with very thin margins, 220 to 215 and 54 to 44 respectively, after the longest known roll-call vote in the history of the House (Oliver, et al., 2004). Thus anticipatory responses are

[21]The poll asked: "You may have heard news about recent debates in Congress on a bill that would add a prescription drug benefit to Medicare. To the best of your knowledge, has this bill been passed by Congress and signed into law by President Bush, or not?

unlikely to have occurred before the final months of 2003.

2.4 Data and Descriptive Statistics

2.4.1 Data Description

In this paper, I use two sources of data on drug utilization: the Medicare Current Beneficiary Survey (MCBS) Cost and Use module for 2001-2006 and the Medical Expenditure Panel Survey (MEPS) Household Component and Prescribed Medicine Files for 1997-2007. Both surveys collect nationally representative data on non-institutionalized individuals' healthcare utilization and expenditures. The MCBS sample consists of only Medicare beneficiaries, while the MEPS surveys households of all ages. Importantly, both datasets provide detailed records of each prescription drug purchased (including refills) during the calendar year including the drug name, therapeutic drug class, strength and dosage, and expenditures by source of payment (e.g. Medicare, private insurance, self-pay). The MCBS will serve as the primary data source for the analysis since the sample size for the population of interest is more than twice as large as in the MEPS. One key advantage of the MEPS is that it samples non-disabled individuals under age 65. Since the near-elderly serve as an informative comparison group for the Medicare-eligible elderly, I will also use MEPS data in some of the analyses.

From the initial MCBS sample of 74,139 observations, I exclude individuals with incomplete drug utilization records for the calendar year since the outcome variable of interest is the total count of prescriptions filled during the year. This

involves dropping individuals who were not interviewed in every round, had partial year Medicare eligibility, or became institutionalized (20.6% of the sample). I also exclude individuals with missing demographic characteristics. The final MCBS sample of Medicare beneficiaries ages 66-85 includes 41,475 observations.[22] In many specifications, I use a sample of the youngest Medicare beneficiaries ages 66-74 which includes 20,072 observations.

One caveat is that, unlike other studies of Part D that use pharmacy claims records, the drug utilization data used in this paper is self-reported.[23] Thus a limitation of this data is that it is subject to reporting error. I can roughly estimate the severity of misreporting using the 2006 MCBS. In 2006, survey records were matched to Medicare administrative data for the first time for those enrolled in Part D. The MCBS identifies which drug records are extracted from the survey only, the claims only, or both the survey and claims. Among all prescription claims, 18.9% of prescription records are reported only in administrative claims and thus would have been absent from the survey data in previous years. Nevertheless, since the emphasis of my analysis is on changes in utilization and not on levels, the misreporting error will not confound my estimates if the magnitude of misreporting does not vary from year to year and is orthogonal to my explanatory variables of interest. It should be noted that in my analysis I exclude claims-only drug records

[22]I exclude individuals over age 85 due to the non-comparable measurement of drug utilization for the institutionalized population. A high proportion of elderly over 85 are institutionalized (28% relative to 8% for individuals aged 75-85 and 2% for individuals aged 66-74). For institutionalized beneficiaries, MCBS prescription records are collected in a separate community survey which is not directly comparable to the non-institutionalized records. Thus the sample of prescription records is not representative for this age group.

[23]Both the MCBS and MEPS implement survey collection techniques to maximize the quality of the reports, which include asking participants to save receipts and empty prescription bottles.

in the 2006 MCBS for comparability with previous years.

Despite this limitation, there are a number of advantages to using survey data over pharmacy claims. The survey data provides a nationally representative sample, richer demographic and health insurance status characteristics, and importantly, a long enough time frame to examine utilization patterns from before the announcement of Part D. Also, as noted in Ketcham and Simon (2008), Part D may have changed the extent to which people use multiple pharmacies or it may have induced people to use different pharmacies than their usual store. Thus, using data from a single pharmacy may make it difficult to produce accurate estimates of utilization changes along the extensive margin. This may be better captured in nationally representative survey data.

2.4.2 Descriptive Statistics

In table 2.1, descriptive statistics are reported for various sub-groups from the MCBS and MEPS. Column 4 presents characteristics of the elderly ages 66-74 from the MCBS, which is the sample used in most of the analyses. Prescription drug use is highly prevalent among this group. 92 percent of the elderly purchase at least one prescription each year– filling on average 28 prescriptions at a total cost of $1,789. In addition to receiving Medicare coverage, 11 percent of the sample are dually enrolled in Medicaid and 67 percent are covered by supplementary private insurance plans such as Medigap or retiree employer benefits. I estimate that 16 percent of elderly

did not have any drug insurance coverage prior to 2006.[24] This is a slightly lower estimate than other sources. For example, Levy and Weir (2009) find that 24% are drug-uninsured in the Health and Retirement Survey in 2004. Comparing the MCBS to the MEPS for the same age group (Columns 3 and 4) demonstrates that mean utilization and expenditures are slightly higher in the MCBS. This may be partially explained by differences in demographic characteristics across the samples which are correlated with drug use and insurance coverage. The MCBS sample is slightly older and more educated.

Drug utilization is lower for the comparison group of adults ages 50-58 (Column 1). Still, this group also has a high rate of prescription drug use (76 percent) and purchase nearly two-thirds as many prescriptions as the elderly. Total annual expenditures are 38% less and the proportion paid out-of-pocket is also lower. Naturally, the largest differences in demographic characteristics across the two age groups are in employment status and insurance coverage.

In columns 5 and 6, means are reported for individuals who filled at least one acute prescription (comparison group) or at least one chronic prescription (treatment group). Many individuals purchased both types of drugs and are included in both samples. The elderly are more likely to use a chronic drug than an acute drug (86% fill a chronic prescription versus 55% for acute) and fill on average 22 prescriptions of chronic drugs and 3 prescriptions of acute drugs per year. The total cost of a chronic

[24]The MCBS elicits detailed information for up to five insurance plans (private, private HMO, or Medicare HMO), including whether the plan covers drugs or offers a drug discount card. A person is defined as drug-insured if they report that at least one of their plans covers prescription drugs. If the individual only has access to a drug discount card they are not considered drug-insured. Individuals who receive drug coverage from a public program other than Medicaid or Medicare (such as the VA or a state-sponsored drug plan) are also defined as drug-insured.

drug prescription is 45% higher than an acute drug and the out-of-pocket share is lower. This may reflect the fact that the higher spending that results from chronic drug purchases pushes individuals past deductibles and into lower levels of cost-sharing. There are no significant differences in means of demographic characteristics across the two (overlapping) samples.

2.5 Empirical Framework

2.5.1 Baseline Model

I estimate the announcement effect of Part D on drug utilization by using a difference-in-difference estimator with group-specific linear trends. The basic strategy compares deviations from drug utilization trends for a treatment group that is more affected by the announcement of Part D with the deviation from trend for a comparison group that is less affected. As motivated by the conceptual framework, my main comparison is between chronic and acute drugs. Although, I also compare individuals who are age-eligible for Medicare with those who are age-ineligible. The key identifying assumption is that in the absence of the announcement, any utilization differences between treatment and comparison groups would continue along the same trend. I include group-specific linear trends because I find that chronic and acute drugs do not exhibit parallel utilization trends in the pre-announcement period.

In particular, I estimate variants of the following equation which includes the

announcement and implementation as separate policies:

$$Y_{itg} = \theta_0 + \theta_1 t + \theta_2 ANNOUNCE_t + \theta_3 IMPLEMENT_t + \theta_4 T_{ig} + \theta_5 (T_{ig} \times t) + \theta_6 (T_{ig} \times ANNOUNCE_t) + \theta_7 (T_{ig} \times IMPLEMENT_t) + X'_{it}\Gamma + \epsilon_{itg} \tag{2.1}$$

For the chronic and acute drug comparison, the outcome is the number of prescriptions (new and refill) purchased by individual i in year t in drug category g (where g is chronic or acute). That is, each individual receives two observations in the regression– one for the number of chronic drugs that they purchase and one for the number of acute drugs they purchase, including zeros. T_{ig} is an indicator which equals one if the observation is for chronic drugs, and zero if the observation is for acute drugs. For the age-eligible and age-ineligible comparison, I include two treatment indicators: $T1_i$ and $T2_i$.[25] $T1_i$ is an indicator for Medicare-eligible adults aged 66-74 and $T2_i$ is an indicator for Medicare-ineligible adults aged 59-64 who are close to the eligibility threshold. The omitted comparison group are adults aged 50-58 who are furthest from Medicare eligibility. In this specification, the outcome is the total number of prescriptions filled. Thus, each individual receives only one observation per year. I also consider the log of the number of prescriptions in some specifications.[26]

$ANNOUNCE_t$ is an indicator variable which turns on in 2004 and 2005,

[25]The age-eligible and age-ineligible model is as follows:

$$Y_{it} = \beta_0 + \beta_1 t + \beta_2 ANNOUNCE_t + \beta_3 IMPLEMENT_t + \beta_4 T1_i + \beta_5 T2_i + \beta_6 (T1_i \times t) + \beta_7 (T2_i \times t) + \beta_8 (T1_i \times ANNOUNCE_t) + \beta_9 (T2_i \times ANNOUNCE_t) + \beta_{10} (T1_i \times IMPLEMENT_t) + \beta_{11} (T2_i \times IMPLEMENT_t) + X'_{it}\Gamma + \epsilon_{it}$$

[26]To account for zeros in the data, the log transformation is log(number of prescriptions +1).

the time period between the announcement and implementation of Part D, and $IMPLEMENT_t$ is an indicator which turns on in 2006 after the program has been implemented. The omitted time period is 2001 to 2003. X_{it} is a vector of individual level control variables including male, age, age-squared, married, three education dummies, three race dummies, three region dummies, metro-area, employment status, Medicaid enrollment, and Medicare HMO enrollment.[27] Standard errors are clustered at the person level to allow for an arbitrary variance-covariance matrix across the two drug groups and over time. I allow for differential trends across treatment and comparison groups by interacting a linear time trend t (which takes on a value of 1 in 2001) with the treatment indicator. It should be noted that Equation 2.1 allows only for an intercept shift in trends for the announcement and implementation effects. While I cannot estimate a slope shift for the implementation effect given that there is only one year of post-implementation data, I do estimate slope shifts for the announcement effect in some specifications.

The outline of the analysis proceeds as follows. First, I use the difference-in-difference methodology to compare adults who are not yet age-eligible for Part D with those who are currently eligible. This is a natural starting point given that this is the cut of the data used by most previous studies of the utilization effect of Part D. However, even those who are not yet eligible for Medicare may anticipate future subsidized coverage and respond to the announcement. Those who are closest to age 65 are more likely to be responsive than those who are further away from eligibility.

[27] I do not include a control for drug insurance coverage because it may itself be an outcome of the announcement and implementation of Part D.

Thus, producing a valid difference-in-difference estimate requires a comparison age group that is far enough away from the eligibility cut-off so that they are less likely to respond to Part D, but near enough to the cut-off so that they have similar drug utilization patterns as the elderly. This criteria may be difficult to satisfy. For one, younger age-groups may use chronic and acute drugs in different proportions which may affect their responsiveness to current and future price changes. I select the age group from 50-58 as the comparison sample. However, it is not clear that the utilization patterns of this group are similar enough to the elderly to provide a valid estimate of counterfactual trends.

Given the limitations of the age-ineligible comparison group, the majority of the analysis exploits variation in the predicted impact of the announcement *within* the elderly, Medicare-eligible sample. I begin by estimating Equation 2.1 for the elderly in the aggregate by constraining the coefficients of the interaction terms and treatment indicator to be zero. I use this specification to estimate population program effects for the announcement and implementation of Part D. However, given that this strategy estimates these effects as only deviations from the pre-existing trend, the estimates will be biased if other aggregate shocks to utilization occur during this time period. To overcome this concern, in my main test for anticipatory effects, I exploit the differential predicted response of chronic and acute drugs to anticipated future price changes. As discussed extensively in Section 2.3.1, chronic drugs should be more affected by the announcement of Part D than acute drugs. The key variable of interest is the interaction between the announcement period and chronic drug indicator. A non-zero θ_6 is evidence of a causal announcement effect.

Chronic drugs may respond positively or negatively to the announcement.

In theory, another comparison could be made between the previously drug-insured and drug-uninsured elderly. We should observe a greater anticipatory response for individuals who ultimately enroll in Part D compared to those who do not enroll. Since I can only observe a given individual for three years in the MCBS, I cannot retrospectively follow individuals who enrolled in Part D and compare their announcement response with those who did not enroll in Part D. Previous drug insurance status has been shown to be a good proxy for eventual enrollment status (Levy and Weir, 2009) and has been used in other work (Kaestner and Kahn, 2010). However, as I will demonstrate in Section 2.7.2, there was a statistically significant 3.7 percentage point decline in drug insurance coverage following the announcement of Part D. Given this large change in coverage, it is likely that the composition of the treatment and comparison groups changed at time of the announcement, thereby making insurance status itself endogenous.

2.5.2 Defining Chronic and Acute Drugs

I use an empirical approach for categorizing drugs as chronic and acute based on observed treatment duration. This method exploits average treatment patterns in the population as opposed to clinical recommendations which may or may not be adopted. The classification method (which is illustrated in Appendix Figure A.1) proceeds as follows. First, I pool MCBS drug records for the elderly ages 65 and over from 2002-2003. I use data from before the announcement so as not

to confound underlying utilization patterns with the treatment effect of Part D. For each individual, I count the number of purchases of each drug in each year. I then combine these counts across people to construct empirical distributions of the number of prescriptions filled in a year for drugs in each therapeutic class. Each drug is assigned one of 38 possible First Data Bank drug class categories (32 classes have a positive number of prescriptions for the elderly). I exclude drugs with no therapeutic classification. For example, person 1 in the illustration filled 1 prescription of Amoxicillin and 2 prescriptions of Cefaclor (both Antiinfectives), and 5 prescriptions of Zocor (a Cardiovascular drug) in 2002. Thus, she contributes a 1 and 2 to the Antiinfectives distribution and a 5 to the Cardiovascular distribution. As is apparent in the figure, Antiinfectives are clearly an acute class since the vast majority of drugs are filled only once a year. Cardiovascular drugs, on the other hand, are more chronic in nature since they are filled many times a year to be used for a longer duration of treatment.

In the most conservative classification, I define a drug class as acute if the median of the empirical distribution of purchases by an individual per year is 2 or less and chronic if the median is greater than 2. I assign this classification to all drugs within the therapeutic class for each year of the survey. 11 out of 32 classes used by the elderly are classified as acute using this method, including Analgesics, EENT preparations, and Antiinfectives (as shown in Appendix Table A.1). Cardiovascular drugs, Diuretics, and Hypoglycemics are among the most frequently purchased chronic treatments. While this median rule guarantees that the majority of drugs in the therapeutic class are either chronic or acute, there is

clearly some measurement error since some drugs in chronic classes are actually acute and vice versa and some drugs can be used for both indications. The extent of the measurement error will vary across therapeutic classes depending on how heterogeneous treatment duration is within the class. In sensitivity analyses, I exclude drugs from the most heterogeneous classes from the sample by using more stringent classification rules. Appendix Figure A.1 presents the classification rules in order of increasing stringency. For example the ">75% in drug group" rule defines the drug class as acute if more than 75% of drugs in the class are filled 2 times or less and chronic if more than 75% of drugs in the class are filled more than 2 times. In this sensitivity analysis, therapeutic classes for which fewer than 75% of drugs can be classified as either acute or chronic (e.g. 45% acute, 55% chronic) are then dropped from the sample. I also validate the results of this classification method by comparing these empirical classifications with those made by physicians. I find a very close correspondence between both classification methods.[28]

[28]I compare the classification results generated by the empirical algorithm with classifications made by three family medicine physicians. I asked each physician to report whether drugs in each class were "somewhat more likely to be acute than chronic," "much more likely to be acute than chronic," "somewhat more likely to be chronic than acute," or "much more likely to be chronic than acute." I provided the physicians with the drug class name and several examples of the most commonly used drugs in each class from the MCBS (though, I asked that they think about the class broadly when making their classifications). In the most conservative classification (using the median classification rule), the empirical algorithm matches the physicians' classifications of chronic versus acute drug classes 85% of the time. The match rate improves as the classification rule for the empirical algorithm becomes more stringent. For example, using the ">65% in drug group" classification rule, the empirical classifications match the physician classifications 96% of the time.

2.6 Results

I begin the analysis by comparing drug utilization changes following the announcement and implementation of Part D for elderly who are eligible for Medicare relative to the near-elderly. Medicare-eligibility status is a natural first cut for identifying the announcement effect. This strategy has been used in most previous studies of Part D. I select adults ages 50-58 as the initial comparison group because they are far enough away from eligibility that they are unlikely to respond to the announcement, and Medicare beneficiaries ages 66-74 who are closest in age to the comparison group. Figure 2.5 plots aggregate trends in drug utilization for these two age groups in the MCBS and MEPS from 1997-2007. The two datasets provide largely comparable measures of drug utilization. For the elderly, the average number of prescriptions filled per year had been rising since 1997. Then immediately following the December 2003 Part D announcement there was a distinct leveling off and eventual decline in drug utilization. In contrast, no trend break after the announcement is observed for the near-elderly. After 2006, when Part D took effect, drug use for the elderly reverted upwards towards its pre-2003 trend. The pre-program "dip" in utilization for the elderly is consistent with a dominating intertemporal substitution effect, in which beneficiaries delay some drug use until after Part D is implemented. Consequently, the increase between 2005 and 2006 may constitute both the treatment effect of Part D and mean reversion. Thus, studies that use small windows of data around the implementation date could overstate the implementation effect.

While the striking graphical evidence is strongly suggestive of a negative announcement effect, we might be concerned that the 50-58 age group does not provide a sufficient measure of counterfactual drug utilization for the elderly. There are many important differences between adults above and below age 65: most notably in employment status, insurance coverage, and the type and quantity of drugs used. Thus, we cannot rule out the possibility that time-varying factors related to, for example, Social Security benefits, unemployment, Medicare benefits, the markets of certain drugs, and so forth could explain the pre-program decline in utilization for the elderly, since these factors are also likely to have a differential impact on the elderly and near-elderly.[29] For this reason, I look for identifying variation for the announcement effect *within* the elderly age group in the remainder of the paper, using the MCBS. I first consider distributional impacts of the announcement and variation in the intensity of the announcement effect by age and demographic groups. I then conduct my main test comparing the differential effects for chronic and acute drug utilization among the elderly. I revisit the age-ineligible comparison group in the final section to test whether those who are nearing eligibility are also responsive to the announcement relative to younger adults who are further from eligibility. Taken together, these tests aim to identify whether the decline in drug utilization observed in Figure 2.5 represents a causal response to the announcement of Part D.

[29] Also, as noted before, the near-elderly does not provide a valid counterfactual for estimating the implementation effect, since the pattern of utilization before implementation is not the same for elderly and near-elderly individuals.

2.6.1 Aggregate Drug Utilization Effects for the Elderly

A. Quantifying the Aggregate Effect

Before decomposing the announcement effect to identify sources of the utilization decline, I quantify the magnitude of the aggregate effect for the elderly. I also illustrate the possible severity of the anticipation bias by comparing estimates of the implementation effect that either take into account or do not take into account the pre-program decline in utilization. I estimate the announcement and implementation effects as deviations from the prior utilization trend in a simple interrupted time series model for the elderly sample. The specification is equivalent to setting the coefficients of the treatment interaction terms and treatment indicator of Equation 2.1 equal to zero as follows:

$$Y_{it} = \pi_0 + \pi_1 t + \pi_2 ANNOUNCE_t + \pi_3 IMPLEMENT_t + X'_{it}\Gamma + \epsilon_{it} \qquad (2.2)$$

Table 2.2 reports the the OLS results for variants of this equation.[30] The first three columns use total prescriptions as the dependent variable. In column 1, only the implementation indicator is included along with the time trend and controls, under the assumption that $\pi_2 = 0$. This specification is analogous to previous studies that estimate the contemporaneous treatment effect of Part D, ignoring possible anticipatory effects. In this case, the implementation effect is large, positive, and statistically significant at the 1% level, representing an average

[30]Note that in this analysis, I do not report estimates of elasticities, as other studies of Part D have done, because individuals responded to a *perceived* price change which is unknown. This perceived price change is a function of the information about Part D that individuals had before the program was implemented.

annual increase of 3 prescriptions or a 10.6% increase relative to the sample mean. This estimate– which is comparable to the effect size found in other studies of 4 to 10%– would suggest that Part D had a large effect on utilization in the first year of the program.

If the assumption of no anticipatory effects is correct, controlling for the announcement indicator should not change the estimate of π_3. On the contrary, I find that after adding the announcement indicator in column 3, the implementation effect shrinks from 3.0 to 0.9 and becomes statistically insignificant, although it is imprecisely estimated. This provides the first piece of evidence that there may be a large upward bias in the implementation effect if anticipatory responses are not taken into account. The announcement effect itself (π_2) is statistically significant and negative, representing a decline of 1.61 prescriptions (a 6% decline relative to the sample mean).[31] This announcement response is also economically important given that it is nearly equivalent to the average annual growth rate of utilization during this time period (the coefficient of the linear time trend is 1.79). In Panel B of Table 2.2, excluding Medicaid beneficiaries who may have different incentives for responding to the announcement[32] produces similar estimates.

When I repeat the above exercise with log prescriptions as the dependent variable in Columns 4-6, I find a smaller percent decline in utilization after the an-

[31]Estimating the interrupted time series with only the announcement indicator and time trend in Column 2 produces a similar estimate of the announcement effect as in Column 3. This implies that the utilization changes during this time period load more heavily on the announcement than on the implementation.

[32]Recall that Medicaid beneficiaries were switched from Medicaid drug coverage to Medicare coverage either automatically or with the option of choosing a plan. To the extent that Medicaid beneficiaries were aware of this change, it is not clear whether they would anticipate a decline or increase in benefit generosity from Part D.

nouncement. This announcement effect is statistically insignificant when both the announcement and implementation indicators are included in Column 6. Since the log transformation places more weight on smaller prescription counts, this difference in results suggests possible treatment effect heterogeneity, with the announcement having a larger effect for elderly with high levels of drug utilization. I investigate this claim further by estimating quantile regressions of the same interrupted time series model. I estimate block bootstrapped 95% confidence intervals at the person level to preserve the serial correlation structure of the error term. The estimated conditional quantile treatment effects and confidence intervals are presented graphically in Figure 2.6 for each quantile of the conditional distribution of total prescriptions. The negative announcement effect increases monotonically across quantiles and only becomes statistically significant beginning with the 75th quantile and above.[33] Then, in the far upper tail of the distribution, the effect again becomes statistically insignificant due to the large sampling variability in the very highest quantiles. In contrast, the implementation effect conditional on the announcement indicator is close to zero and statistically insignficant for every quantile of the distribution. This shows that, indeed, announcement effects are concentrated among elderly with high drug use. This result is also consistent with evidence presented later which shows that the announcement effect is driven by chronic drug use (by definition, chronic drugs require more prescription purchases).

The results in this section confirm the visual impressions from Figure 2.5 and

[33]If there are rank reversals, these estimates do not necessarily identify treatment effects for individuals at each unconditional quantile of the distribution.

suggest that the rise in utilization during the first year of Part D may largely represent a recovery from the anticipatory decline with little net increase in utilization generated by Part D. This causal interpretation will be investigated in detail in the following sections.

B. Alternative Outcomes and Specifications

In Table 2.3, I investigate whether a similar announcement response can be observed for alternative outcome measures – number of prescriptions conditional on use, total expenditures, and out-of-pocket expenditures. Again, I estimate the model in Equation 2.2. I also estimate a probit model for the probability that an individual purchases any prescriptions during the year. The announcement and implementation have no effect on the probability of any drug use as the estimates are approximately zero with small confidence intervals. This is not surprising given the nearly universal use of drugs among the elderly. However, these results may mask important changes in the initiation or discontinuation of individual drug products. Given that there is no utilization effect along the extensive margin, the estimates for prescriptions conditional on use are very similar to the unconditional estimates.

As would be predicted, the expenditure estimates in Columns 7 through 10 have the opposite pattern of the utilization results. Any decline in expenditures relative to trend resulting from the anticipatory utilization dip *reinforces* the predicted negative implementation effect of Part D on expenditures. Thus, unlike with utilization, failing to include the announcement effect biases the implementation effect *downwards* in absolute value. Focusing on log expenditure results, which account

for the skewness in the expenditure distribution, we can see that including the announcement effect increases the absolute size of the implementation effect slightly from -0.010 to -0.014 percent. The announcement effect represents a 0.3 percent decline which is statistically insignificant.[34] Out-of-pocket expenditures are likely to be more responsive than total expenditures since this is the outcome that is most directly targeted by Part D. Moreover, individuals who intertemporally substitute aim to reduce out-of-pocket costs. The announcement effect is much larger in this case, but is still statistically insignificant. After controlling for the announcement effect, the implementation effect changes from -12.9 to -15.6 percent.

Finally, in Appendix Table A.2, I estimate alternative specifications that control more flexibly for time trends. The estimates are mostly robust across specifications. First, I include a quadratic time trend. The coefficient on the linear term drops to -0.17 and its standard error increases sharply, suggesting a collinear relationship with the quadratic term. Given that 6 years of data provides too limited a range to estimate a quadratic trend precisely, the quadratic term is dropped in subsequent models. Second, I allow for a slope shift in the linear trend after the announcement in Column 2. I add the variable "Years Since Announce" to Equation 2.2, which is defined as the year minus 2003 in the announcement period, so that it takes on a value of 1 in 2004 and a 2 in 2005, and zero otherwise. Allowing for a slope shift produces an estimate of the announcement effect in 2004 (the linear combination of the coefficients of Announce + Years Since Announce) that is

[34]Some of the loss in precision may reflect measurement error for expenditures, which may be less accurately reported relative to the count of purchases.

nearly identical to the estimate from only a level shift. Third, I estimate the trend non-parametrically by including a full set of year dummies. One advantage of this specification is that a structural break is not imposed in any particular year. Still, the model identifies a trend break in 2003. The results in the bottom panel comparing the one-year change in utilization from 2003 to 2004 relative to the change from 2002 to 2003 indicate a statistically significant decline of 2.32 prescriptions after the announcement. Furthermore, there is no statistically significant difference in the change from 2002 to 2003 relative to the change from 2004 to 2005. This result provides further support for using a linear specification to approximate the time trend. In Appendix Table A.3, I also test whether the aggregate results are robust to estimating a negative binomial model which accounts for the count nature of the prescription data and its overdispersion. The negative binomial and OLS results are similar in magnitude and significance.

2.6.2 Treatment Heterogeneity by Age, Education, and Income

Next, I examine how aggregate anticipatory responses vary across age, income, and education groups. In the above sections, I have focused on the 66-74 age group which has been shown to experience a sharp decline in drug use after the announcement of Part D. I now expand the sample to include elderly ages 75-85. As noted in Section 2.4.1, I exclude elderly over age 85 due to the non-comparable measurement of prescription drug use for this age group. Columns 1 and 2 of Table 2.4 compare age groups 66-74 (repeated from table 2.2) and 75-85. Only the 66-74 age group

exhibits an anticipatory utilization response. The older age group has a large positive contemporaneous *implementation* effect, but no announcement effect. Thus, the younger age group appears to be more forward-looking in their drug utilization response. This is consistent with the fact that the health costs of deferring drug use is lower for younger individuals who are in better health and also that younger individuals have a longer time horizon over which to derive benefits from the delayed treatment. Furthermore, the younger age group is likely to have more cognitive awareness of the announcement and benefit design of Part D.

I also consider heterogeneous effects for other demographic groups. Since the announcement effect is only apparent for the 66-74 age group, I look within this age group for variation in the effect size by income and education levels. Comparing Columns 5 and 6, we can see that the negative announcement effect for the 66-74 age group is driven almost entirely by elderly with income below the median. This conforms to expectations, since these individuals are most liquidity constrained and also anticipate larger benefits from Part D, given the additional subsidies provided to low income beneficiaries. Finally, after controlling for income, individuals with a high school degree or less have a larger negative announcement and implementation effect than those with more than a high school degree. This result is somewhat surprising since access to information about the announcement might be expected to increase with higher levels of education. However, this difference might also reflect unobserved characteristics that are correlated with education, such as health status. A positive relationship between education and health has been widely documented in the literature (e.g. Lleras-Muney, 2005; Deaton and Paxson, 2001; Grossman and

Kaestner 1997). While individuals in poor health may find it more costly to delay treatments, they are also more frequent users of chronic drugs which, as discussed in the next section, have a greater scope for intertemporal substitution.

2.6.3 Chronic and Acute Difference-in-Difference Estimates

A. Basic Results

If the observed decline in drug utilization is the result of anticipatory behavior, we would expect to find differential utilization responses for chronic and acute drugs purchased by the elderly. In this section, I test this hypothesis for the 66-74 age group. Figure 2.7 plots sample means of total prescriptions for chronic and acute drugs in each year and predicted counterfactual trends in the post-announcement period.[35] I find differential trends in drug utilization that follow the pattern predicted by the life-cycle model. While both drug types exhibit smooth linear trends before the announcement of Part D, there is a substantial negative trend break after 2003 for chronic drugs, whereas acute drug utilization continues along its pre-existing trend. After Part D is implemented, utilization increases relative to the counterfactual trend for both acute and chronic drugs. This pattern is consistent with the hypothesis that chronic drug use is more responsive to anticipated future prices, while both chronic and acute drugs are responsive to contemporaneous prices at the time of implementation.

[35]The dashed lines represent pre-announcement period trends projected forward, which can be obtained by estimating the basic model, Equation 2.1, without controls. To implement this, I first estimate the coefficients in the basic model from Equation 2.1. Then I recode the announcement and implementation indicators as zeros for all observations and compute the predicted values for total prescriptions in each year.

Table 2.5 formalizes this graphical evidence by reporting the difference-in-difference regression results from estimating Equation 2.1 for chronic and acute drugs– that is, I compare the change in utilization for chronic drugs relative to acute drugs before and after the announcement and implementation of Part D. Columns 1 and 2 present results that use the most conservative method for classifying drugs as chronic or acute (the median assignment rule), while columns 3 and 4 use the more stringent 65% classification rule.[36] This split by classification rule is tantamount to comparing utilization trends for drugs in classes that are more likely to be chronic with drugs that are more likely to be acute versus comparing drugs in a subset of classes that are *much* more likely to be chronic with drugs that are *much* more likely to be acute. In the latter classification method, drug classes that are nearly equally likely to be either chronic or acute, are dropped from the sample and the total count of acute and chronic prescriptions purchased are recalculated for each person. Consequently, the mean number of chronic and acute prescriptions filled under the median rule is larger than the mean under the 65% rule (22.14 chronic and 3.26 acute drugs compared to 17.28 and 1.37). Clearly, there is a trade-off between reducing classification measurement error through increasingly stringent classifications and increasing the number of drug classes (and hence total prescriptions) included in the sample. The 65% classification rule, which includes 73% of the prescriptions in the original sample, provides a greater balance of these two objectives than other

[36]Recall that the median rule designates a therapeutic drug class as acute or chronic if more than 50% of the drugs in that class have ≤ 2 purchases per year or > 2 purchases per year, respectively. The 65% rule does the same, except that it requires that more than 65% of the drugs in the class fit either criteria. If the therapeutic drug class cannot satisfy either criteria (e.g. 45% of the drugs have ≤ 2 purchases and 55% have > 2), then the entire therapeutic drug class is dropped from the sample.

possible classification rules.

Consistent with the patterns depicted in the figure, I find in Table 2.5 a large decline in chronic drug use relative to acute drug use after the announcement of Part D using both classification methods. For the median classification method, the use of chronic drugs declined by 1.72 prescriptions in absolute terms compared to 0.16 prescriptions for acute drugs relative to pre-announcement trends (bottom panel of Column 2). This effect is only statistically significant for chronic drugs. The difference-in-difference estimate of the relative change in utilization for chronic drugs is -1.57 (a 7% decline relative to the mean for chronic drugs) and is also statistically significant at the 1% level. The DID estimate is similar for the 65% classification rule in Column 4, although it represents a slightly larger proportional change given the lower baseline mean of chronic prescriptions in this sample. To the extent that changes in acute drug use control for underlying aggregate shocks to health, insurance coverage, pharmaceutical prices (I consider each of these in turn in Section 2.7.2), and other possible confounding factors, the DID estimate is representative of the causal announcement effect. While this claim is fundamentally untestable, the fact that the announcement effect for acute drugs is both qualitatively small and not significantly different from zero alleviates major concerns of a potential bias from coincident aggregate shocks. As before, the negative announcement effect for chronic drugs suggests that Medicare beneficiaries delayed some drug use in anticipation of subsidized Part D coverage. Meanwhile, acute drug use does not respond to the announcement of the future price change as predicted.

I next turn to the implementation effects to estimate the treatment effect bias

from ignoring this anticipatory response. First, I estimate the model in Equation 2.1 assuming that there are no anticipatory effects for chronic and acute drugs by excluding the announcement indicator and the announcement-by-chronic interaction term. In other words, I assume that $\theta_2 = 0$ and $\theta_6 = 0$ (recall that the second assumption was rejected in the section above). In this misspecified model, the implementation response is positive, large, and statistically significant for chronic drugs, representing an absolute increase of 4.51 and 3.61 prescriptions for the median and 65% classification rules, respectively. However, when announcement controls are added in Columns 2 and 4, the implementation effect drops to 2.27 and 1.73 for the two classification rules. Thus, accounting for the negative announcement effect reduces the estimated implementation effect for chronic drugs by about one-half, suggesting a potentially large upward bias in previous studies that evaluate the first or second year impacts of the program. Meanwhile, there is a large increase in acute drug use relative to trend after the implementation of 23.6% (.323/1.37). This effect is stable across the specifications with and without announcement controls, as expected, given that we could not reject that $\hat{\theta_2} = 0$. In this section I have focused on the absolute change in utilization relative to trend after the implementation, rather than the DID estimate, because acute drug use may also be responsive to the implementation of Part D. This implementation estimate is causal if other aggregate shocks to utilization did not occur in 2006. Nevertheless, given the strong evidence of a negative anticipatory effect, we can still conclude that ignoring anticipatory responses leads to biased program effect estimates.

B. Robustness Tests

Moving beyond the basic results, I consider several alternative specifications as robustness tests. One concern with the announcement effect estimated in levels is that there is a large difference in baseline mean utilization between chronic and acute drugs. The absence of an announcement effect for acute drugs may be simply an artifact of the more limited scope for downward adjustment for these drugs. Furthermore, given the large difference in the average duration of treatment, it is not clear that differences in level effects alone can be interpreted as different treatment impacts. Delaying one acute treatment could lead to a decline in utilization of one or two prescriptions, whereas delaying a chronic treatment could lead to a decline of five or more prescriptions. To address this concern, I compare proportional changes for chronic and acute drugs using log prescriptions as the dependent variable in Columns 5-8 of Table 2.5. With the log specification, I also find that the announcement effect for acute drugs is statistically insignificant and close to zero, suggesting that the low mean for acute drugs is not driving the smaller effect size. Moreover, chronic drug utilization declined by a statistically significant 11.3% relative to acute drugs under the 65% classification rule. This is slightly larger than the announcement effect estimated in levels. For the median classification rule, the relative decline of chronic drugs after the announcement is not statistically significant.

The implementation effect for acute drugs in Columns 6 and 8 is large, positive, and statistically significant which is similar to the results in levels. For chronic drugs, the results for logs and levels differ. Unlike the level results, the log implementation effect is not statistically different from zero once controlling for the announcement

effect. However, given the large standard errors due to using only one year of post-implementation data,[37] I cannot confidently interpret this as evidence of no program effect. However, the results still suggest a substantial upward bias from the anticipatory response. Controlling for the announcement effect in column 8, the upper bound of the 95% confidence interval for the implementation effect is 12.8%. This is smaller than the point estimate from estimating the model excluding announcement controls, suggesting an upward bias. Comparing point estimates across the specifications with and without announcement controls, the estimated upward bias for chronic drugs is much larger in logs than in levels (558.2% in logs compared to 108.7% in levels for the 65% classification rule).

I next conduct additional sensitivity tests of the drug classification method. As previously noted, one limitation of the empirical classification method is that some chronic drugs will be classified as acute drugs and vice versa. This could bias the chronic announcement effect towards zero and the acute effect away from zero. In Table 2.6, I repeat the analysis of the above section for the 50%, 55%, 60%, 65%, 70%, and 75% classification rules for both level and log prescriptions. Moving from 50% to 75% reduces classification measurement error, but also lowers the total number of prescriptions included in the sample by construction. I do not report the results for the 80% classification rule and above since these rules exclude more than 93% of the original sample of prescriptions, omitting many classes of drugs widely used by the elderly such as Cardiovascular, Cardiac, and Psychotherapeutic drugs. The results for the announcement and implementation effects are extremely stable

[37] To date, 2006 is the most recent year of data available in the MCBS.

across classification methods. Importantly, the acute announcement effect is close to zero and statistically insignificant in every single level and log specification. For chronic drugs, the relative announcement effect is statistically significant in levels and increases slightly across specifications, though this pattern is non-monotonic. In the log specification, even though the relative announcement effect for chronic drugs is statistically insignificant for the median and 55% classification methods, the effect becomes significant for the more stringent 60% through 70% classification methods. This is reassuring evidence of a robust proportional effect.

I consider two additional specification tests. First, in Appendix Table A.4, I relax the linear time trend assumption for the median classification rule. I include a quadratic trend, allow for slope shifts in addition to the level shift, and estimate a non-parametric trend. These tests are analogous to those performed for the aggregate model in Section 2.6.1. As might be expected from inspection of Figure 2.7, the results are highly robust across specifications, lending support to the suitability of the linear time trend. Second, I estimate a negative binomial model. The marginal effects are reported in Appendix Table A.5. As with other non-linear models, the marginal effects are conditional on the independent variables and vary across observations. Figure 2.9 plots the marginal effects and z-statistics for the interaction of the announcement and chronic indicators for each person in the sample as a function of their predicted prescription count. Characteristics that predict higher drug use are associated with a larger negative chronic announcement effect (within the chronic and acute observations).[38] Computing marginal effects for interaction terms

[38]Acute and chronic observations have different, non-overlapping ranges of values for predicted

in non-linear models requires explicit calculation of the cross-partial or (in this case) "double-difference" of the conditional expectation function, rather than the single-difference as is appropriate for non-interacted variables (Ai and Norton, 2003). The main interaction term of interest in this study is the difference between chronic and acute drugs in the change in utilization before and after the announcement. This effect is expressed in conditional expectations notation as Ω_i in Equation 2.3 below.

$$\begin{aligned}\Omega_i = &\{E[Y_{itg}|T_{ig}=1, ANNOUNCE_t=1, X_{it}] - E[Y_{itg}|T_{ig}=1, ANNOUNCE_t=0, X_{it}]\} \\ &- \{E[Y_{itg}|T_{ig}=0, ANNOUNCE_t=1, X_{it}] - E[Y_{itg}|T_{ig}=0, ANNOUNCE_t=0, X_{it}]\}\end{aligned} \tag{2.3}$$

I compute the average marginal effect (weighted by population sampling weights) for the interaction term analytically as follows in Equation 2.4 and apply the Delta method to estimate standard errors.[39] The individual marginal effects and z-statistics in the figure are computed in an analogous way.

$$AME = \frac{\Delta E[Y_{itg}|X_{it}]}{\Delta T_{ig} \Delta ANNOUNCE_t} = \frac{1}{\sum_{n=1}^{N} \omega_i} \sum_{n=1}^{N} \omega_i \{\Omega_i\} \tag{2.4}$$

prescriptions. The announcement interaction effect becomes more negative at the high end of each range. This may partially reflect measurement error of the classification method in the median classification rule.

[39] Given that the conditional mean for the negative binomial is $exp(X'\beta)$, the actual computation of Ω_i is as follows using estimated coefficients:

$$\begin{aligned}\Omega_i = &\, exp(\theta_0 + \theta_1 t + \theta_2 + \theta_3 IMPLEMENT_t + \theta_4 + \theta_5 t + \theta_6 + \theta_7 IMPLEMENT_t + X'_{it}\Gamma) \\ &- exp(\theta_0 + \theta_1 t + \theta_3 IMPLEMENT_t + \theta_4 + \theta_5 t + \theta_7 IMPLEMENT_t + X'_{it}\Gamma) \\ &- exp(\theta_0 + \theta_1 t + \theta_2 + \theta_3 IMPLEMENT_t + X'_{it}\Gamma) \\ &+ exp(\theta_0 + \theta_1 t + \theta_3 IMPLEMENT_t + X'_{it}\Gamma)\end{aligned}$$

The average announcement and implementation marginal effects for chronic and acute drugs are very similar to the OLS results. The change in chronic drug utilization relative to acute use after the announcement is -1.34 compared to -1.42 in the OLS model and is statistically significant at the 1% level (Appendix Table A.5). As before, acute drug use does not respond significantly to the announcement.

One final concern is that the negative announcement effect could be driven by a single class of drugs that experienced a large unknown utilization shock after 2003 (e.g. a major product discontinuation). I estimate the contribution of each drug class to the main effect reported in Table 2.2 by running the basic model separately for each of the 32 therapeutic drug categories in the MCBS.[40] In Table 2.8, I report the coefficients for the announcement and implementation indicators for the 8 classes of chronic and acute drugs with the highest utilization in the MCBS. I find that the negative announcement effect is not driven by a single drug class, but is a widespread phenomenon. For example, among the top 8 chronic drug classes, there are significant negative anticipatory responses for Diuretics, Hypoglycemics, Psychotherapeutic drugs, and Gastrointestinal preparations. The recovery in utilization after Part D's implementation varies across classes. For Hypoglycemics (i.e. diabetes treatments), utilization rebounds fully from the pre-program decline with an additional net increase. Meanwhile, Psychotherapeutic drugs do not rebound fully. Some chronic drugs such as Cardiac drugs and Autonomic drugs are not responsive to the announcement of Part D. These drug classes also do not experi-

[40]Each regression uses as an outcome the total number of prescriptions purchased in each drug class, including zeros.

ence implementation effects. Thus, utilization patterns for these classes may reflect price-inelasticity to current and future prices, rather than failure of the intertemporal substitution hypothesis. In contrast, nearly all acute drug classes, including Antiinfectives, EENT preparations, and Antihistamines do not respond to the announcement. Taken together, the results presented in this section provide strong evidence of the prediction that chronic drugs are more responsive to future prices than acute drugs.

2.7 Additional Tests of the Anticipatory Effect

2.7.1 Mechanisms

As a further check of the hypothesis of life-cycle behavior, I look for independent evidence of the hypothesized mechanisms for an anticipatory response. Doctor visits are a necessary condition for starting a new therapy. Moreover, new therapies often involve follow-up appointments to check for side-effects and to adjust treatment regimens. Thus, if the announcement of Part D caused elderly to delay initiating new treatments, we should observe a decline in doctor visits relative to the counterfactual trend. I illustrate the trend in doctor visits graphically in Figure 2.10. There is a close correspondence between the trend in utilization and doctors' visits which is consistent with a decline in new therapies initiated. However, this evidence does not rule out other shocks that may have affected visits that are coincident with utilization trends. For example, over-time variation in the incidence of certain illnesses could also lead to coinciding trends.

I also take advantage of the panel nature of the data to estimate the effect of the announcement on the probability that treatments are initiated and the probability that treatments are discontinued. Decreasing initiation probabilities or increasing discontinuation probabilities are two possible mechanisms for a negative utilization response. I estimate these probabilities in a similar fashion as the labor supply transition probabilities estimated by Blundell et al. (2010). For example, using two-year panels, I can define an indicator which equals 1 if a person uses at least one drug in class j in period t conditional on not having used any drugs in that class in period $t-1$. Then I can estimate probabilities of initiating a treatment over time. The probability that a person discontinues a treatment in a given drug class is defined in the opposite way. In any given year, the probability that a person begins a treatment in a new drug class is approximately 5%. Discontinuing a class of treatment is more common with a probability of 25%. Trends of these transition probabilities are plotted in Figures 2.11 and 2.12. I find a pre-reform decline in the initiation probability of about 12% followed by a steep increase in 2006. While this pattern is consistent with elderly delaying new treatments, without access to more years of data it is difficult to determine whether this represents a break from the pre-existing trend. In regression results not reported, this change does not represent a statistically significant decline relative to trend. For tractability, I have estimated these probabilities at the drug class level, but this masks changes in the initiation probability of individual drug products. Estimates of discontinuation probabilities are noisier, making it is difficult to draw conclusions about pre-reform behavior. However, there appears to be a secular decline in treatment discontinuation over

the entire time period.

2.7.2 Alternative Explanations

The results presented in the above sections provide strong evidence that the decline in utilization following the announcement of Part D can be explained by an anticipatory delay in drug use. Below I examine the potential significance of two possible alternative supply-side explanations for the decline in drug use after 2003.

One alternative explanation is that pharmaceutical firms, responding to the anticipated reduced price-sensitivity of the elderly, may have found it optimal to increase drug prices. In order to avoid the potential political backlash from increasing prices after implementation, firms may have started to raise prices as soon as the announcement occurred. Thus a decrease in drug utilization could partially reflect a response to current price rather than anticipatory behavior by the elderly. I test for this by estimating whether prices increased after the announcement more for drugs that were differentially used by the Medicare population (which is similar in spirit to Duggan and Scott Morton, 2010). I estimate the following model:

$$Y_{jt} = \alpha + \sum_{t} \mu_t * MMS_j + \mu_t + \delta_j + \epsilon_{jt} \tag{2.5}$$

The outcome Y_{jt} is the price of drug j in year t and is computed by dividing total expenditures over total prescriptions in the MEPS. I include a full set of year fixed effects, drug fixed effects, and interactions of year fixed effects with the

Medicare market share (MMS). The MMS is the fraction of prescriptions that are purchased by Medicare beneficiaries for drug j in the 2002-2003 pooled MEPS. This regression is estimated at the drug level for the top 200 brand-name drugs in terms of 2003 sales, which are reported in the Drug Topics magazine. I include only the 154 drugs from this list that are present in each year of the MEPS from 2000-2006. Observations are weighted by the number of prescriptions filled. If suppliers respond as hypothesized, drugs that are differentially used by Medicare beneficiaries should see the greatest price growth. In results reported in Table 2.9, I find evidence of negative, but statistically insignificant, relative price growth for drugs with higher Medicare market share immediately after the announcement. Between 2003 and 2005, relative price growth increased by 5%, but is statistically insignificant. The major break in the relative price trend occurs only after the implementation of Part D with substantial negative relative price growth that is significant at the 10% level. The absence of a significant price hike among top drugs suggests that the decline in utilization did not result from price changes.

I also consider the possibility that insurers or employers discontinued drug coverage or reduced the generosity of coverage following the announcement. Increasing out-of-pocket costs could then explain a decline in drug use. As was noted earlier, in the aggregate, there is a pronounced secular trend of decreasing out-of-pocket drug costs for the elderly throughout the study period. This would suggest that utilization should have risen, rather than declined in the pre-reform period. On the other hand, drug coverage fell relative to trend after the announcement. Using a simple interrupted time series, I find a statistically significant 3.7 percentage point

decline in drug insurance coverage (Table 2.10). This is driven by declining drug coverage from Medigap plans and Medicare HMOs, while there was an increase in drug-coverage from self-purchased plans.

Given the limitations of the data, I cannot distinguish whether this decline was due to changes in offer rates by insurers and employers or changes in take-up. Reductions in take-up would be consistent with a demand-side anticipatory utilization response. Meanwhile, reductions in offer rates or plan generosity should have affected both chronic and acute drug use alike, which is inconsistent with the empirical findings. Moreover, it is unclear why these two types of plans would have an incentive to discontinue coverage or reduce its generosity prior to Part D. The MMA stipulated that Medigap plans discontinue their prescription drug plans beginning in 2006, while other plans would receive subsidies if they maintained coverage. Many Medicare HMOs became MA-PDs offering Part D benefits in 2006. Finally, as was discussed previously, only a small share of Medicare beneficiaries receive drug benefits from Medigap and Medicare HMO plans (9% and 11% respectively in 2002) and the majority of Medicare HMO plans cover only generic drugs. This suggests that changes in coverage are unlikely be significant contributors to the observed decline in drug use.

2.7.3 Anticipatory Effects Before Medicare Eligibility?

Finally, I return to the Medicare age-eligible and age-ineligible split to test whether the announcement of Part D affected consumption patterns for adults who

are not yet eligible for Medicare. This also formalizes the results from the elderly and near-elderly comparison in Section 2.6.1. I use the MEPS to compare utilization patterns for two groups of age-ineligible adults ages 50-58 and 59-64 with elderly ages 66-74 who are eligible for Medicare. Figure 2.8 illustrates the results of this comparison. It appears that utilization declines for the age-eligible and oldest age-ineligible groups after the announcement, while it increases for the youngest age group. The regression results in Table 2.11 confirm this observation, however due to the small sample size of the MEPS there is not enough power to estimate the effects precisely. Still, it appears that adults ages 59-64 may have experienced a negative anticipatory response to the announcement relative to the 50-58 age group. Individuals who were ages 62 to 64 at the end of 2003, could expect to become eligible for Medicare before or during the first year of Part D, while those ages 59 to 61 would become eligible shortly thereafter. It is possible that the anticipatory effects could be even stronger for the previously Medicare-ineligible than the currently eligible. Given the high rate of uninsurance preceding Medicare eligibility (Card, Dobkin, Maestas, 2008), many individuals could anticipate gaining not only drug coverage, but also coverage of doctor visits, which are complementary to drug use. In proportional changes, there was a statistically insignificant 9.8% decline in utilization after the announcement for the nearly-eligible group ages 59-64 relative to those ages 50-58. Moreover, there was a decline in use that was twice as large after the implementation. For individuals who were not yet eligible for Medicare, the implementation may have acted as a more salient "announcement" than the passage of the law. Comparing changes in utilization for the elderly with the age

50-58 group, there was also a negative relative announcement effect. This is statistically insignificant which is perhaps due to the small sample size of the elderly in the MEPS. Thus, the results provide suggestive evidence that individuals nearing Medicare eligibility also change their drug utilization patterns in anticipation of future coverage.

2.8 Conclusion

The advance announcement of Part D in late 2003, provides an opportunity to evaluate the effects of program announcements in addition to providing a test of life-cycle behavior in the context of drug demand. Economic theory makes ambiguous predictions about the effect of a forecastable future price change on the direction of the utilization effect. For chronic drugs that treat long term illnesses, the effect could be either positive or negative. While acute drugs that treat short duration medical events are unlikely to be affected by future price changes. The results of this study demonstrate a marked decline in drug use following the announcement of Part D of approximately 6% (or a decline of nearly 2 prescriptions per year), favoring a dominating intertemporal substitution effect. The shifting of drug use can be observed most strongly among elderly who are aged 66-74 and less-educated. Moreover, almost the entire decline in drug use is concentrated among elderly with below-median incomes who are the most liquidity constrained and who can anticipate the largest benefits from Part D. In contrast to elderly ages 66-74, I find no evidence of an aggregate anticipatory response for older age groups. A possible

explanation for this differential is the greater health costs associated with delaying drug use and lower cognitive awareness of the Part D announcement for older Medicare beneficiaries. Adults who are not yet eligible for Medicare may also respond to the anticipation of future coverage of both drugs and other complementary medical care such as doctor visits. I find suggestive evidence that Medicare-ineligible individuals who are closer to age 65 are more likely than those further from the eligibility cut-off to lower drug utilization in response to the announcement.

Since the negative anticipatory response can be observed across many drug classes, we can rule out the possibility that the anticipatory effect is driven by idiosyncratic shocks to a single class. In particular, I find strong evidence of a relative and absolute decline in total utilization for chronic drug classes compared to acute drug classes which is consistent with theoretical predictions. Despite, the lower baseline use of acute drugs, this result is found for proportional changes as well. The comparison of chronic and acute drugs is advantageous because any plausible alternative explanation must also explain this differential effect. Moreover, this comparison is less likely to be contaminated by the endogenous movement from chronic to acute drugs or by using a comparison group that is partially treated by the policy announcement. As I have argued, these are challenges that must be addressed when using other plausible treatment and comparison groups: age-eligible versus age-ineligible individuals and drug-uninsured versus drug-insured individuals.

I consider two possible supply-side anticipatory responses which could provide alternative explanations for the utilization decline. First, if the announcement of Part D caused insurers to eliminate drug insurance coverage, then a decline in use

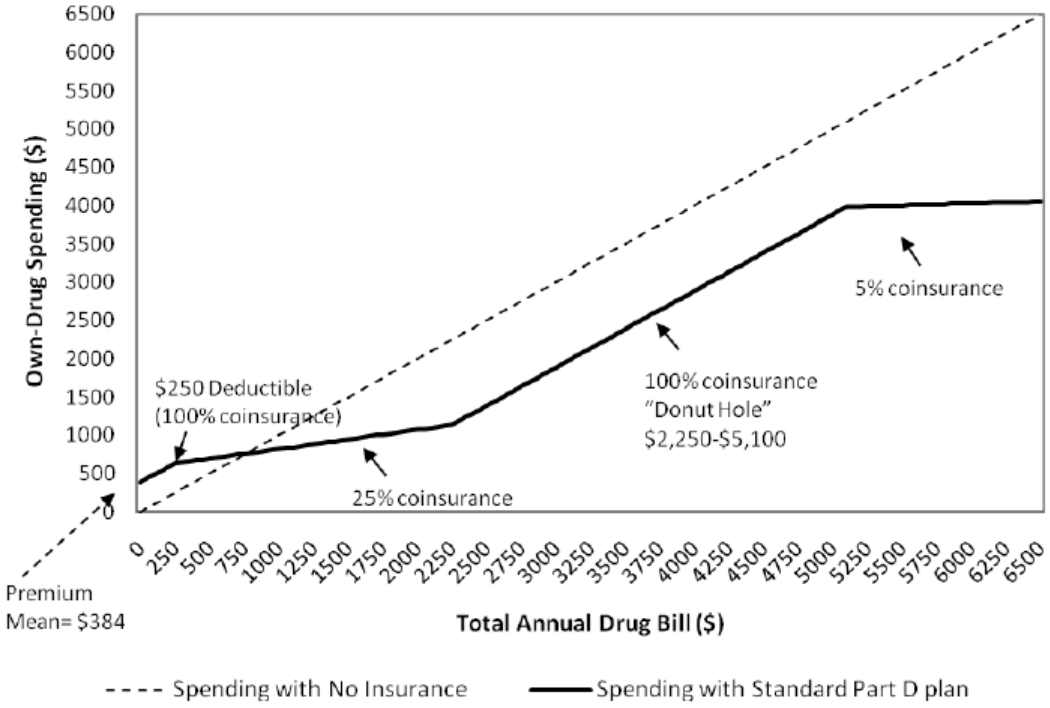
could be a response to a contemporaneous increase in out-of-pocket costs. I find, that there was an approximately 3.7 percentage point decline in insurance coverage after the announcement driven largely by a decline in drug coverage from Medigap plans and Medicare HMOs. While it is difficult to distinguish between anticipatory changes in offer rates and take-up, there is no clear rationale for why we should observe a decline in offerings by specifically these plans. Moreover, losing insurance should reduce both chronic and acute drug use, whereas voluntarily opting out of insurance is not inconsistent with a differential decline in the use of chronic drugs. I also consider possible anticipatory responses by pharmaceutical firms. Firms could increase prices in anticipation of the reduced price responsiveness of the elderly, which could cause a contemporaneous decrease in drug use. I do not find empirical support for the explanation that firms have increased prices. Given the weak evidence in favor of these alternative explanations, it seems likely that the announcement of Part D caused individuals to lower drug utilization, thus providing support for forward-looking behavior in drug demand.

If individuals lowered drug use after the announcement in order to defer use to the period when drugs would be subsidized, why don't we observe a full recovery of drug use after the program was implemented? First, given data limitations, only the first year implementation effect can be estimated using the MCBS. In the first year, there were many widely-publicized administrative problems (such as clogged phone lines, website malfunction, etc.) which may have hindered enrollment in the program. Moreover the penalty-free enrollment deadline was in May, causing many people to enroll for only the second half of the year. In subsequent years, enrollment

stabilized and it is possible that the full impact of changes to the timing of drug use will be more evident when examining long run effects. For this reason, I focus on the results for the announcement effect, rather than the implementation effect, for which the estimates are most precise. Also, it is likely that given some confusion about the benefits of Part D, some elderly who expected to participate in the program and reacted to the announcement did not ultimately enroll. Furthermore, it is possible that cost containment strategies employed by prescription drug plans were effective at reducing utilization growth.

Finally, the observed anticipatory decline in drug use has consequences for evaluating the program effect of Part D. The impact of Part D on the timing of drug utilization should be isolated from its impact on generating new drug use. I have shown that accounting for the negative announcement effect reduces the estimated total program effect by at least one-half. Thus, failing to account for anticipatory responses to the announcement in 2003 overstates the impact of Part D on drug utilization. In a similar way, anticipatory responses may also confound future evaluations of the 2010 health care reform and should be explicitly estimated.

Figure 2.1: Part D "Standard Plan" Benefit Design in 2006



Notes: This figure compares out-of-pocket spending for Part D beneficiaries enrolled in the Standard Plan and for the uninsured. Part D beneficiaries may receive further cost savings relative to the uninsured due to price negotiations between private plans and drug manufacturers and pharmacies.

Figure 2.2: Share of Total Drug Spending Paid Out-of-Pocket by Age Group, 2000-2006

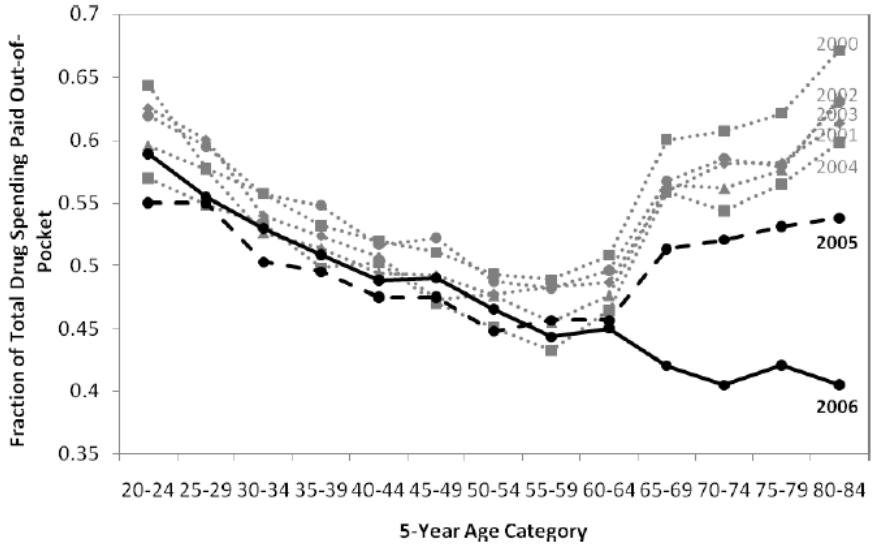
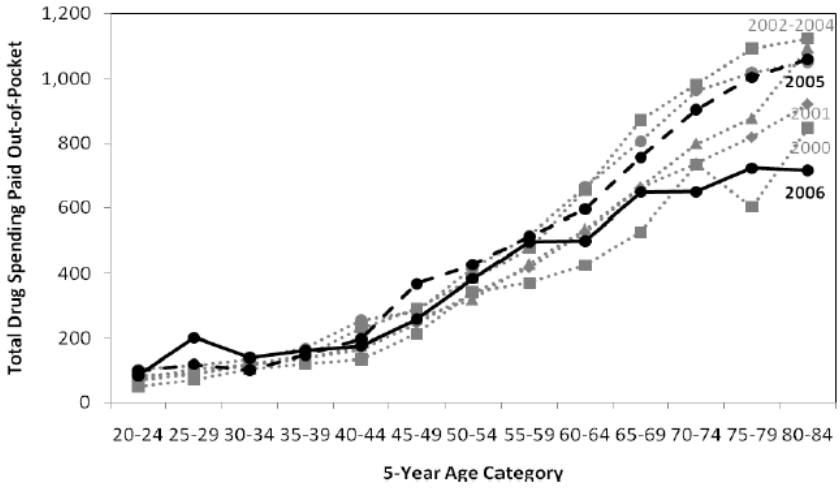
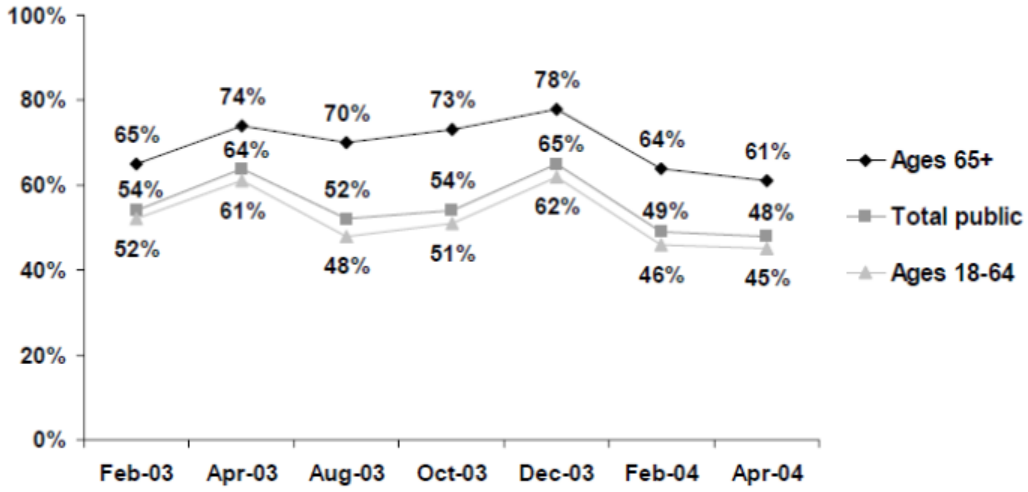


Figure 2.3: Level of Total Drug Spending Paid Out-of-Pocket by Age Group, 2000-2006



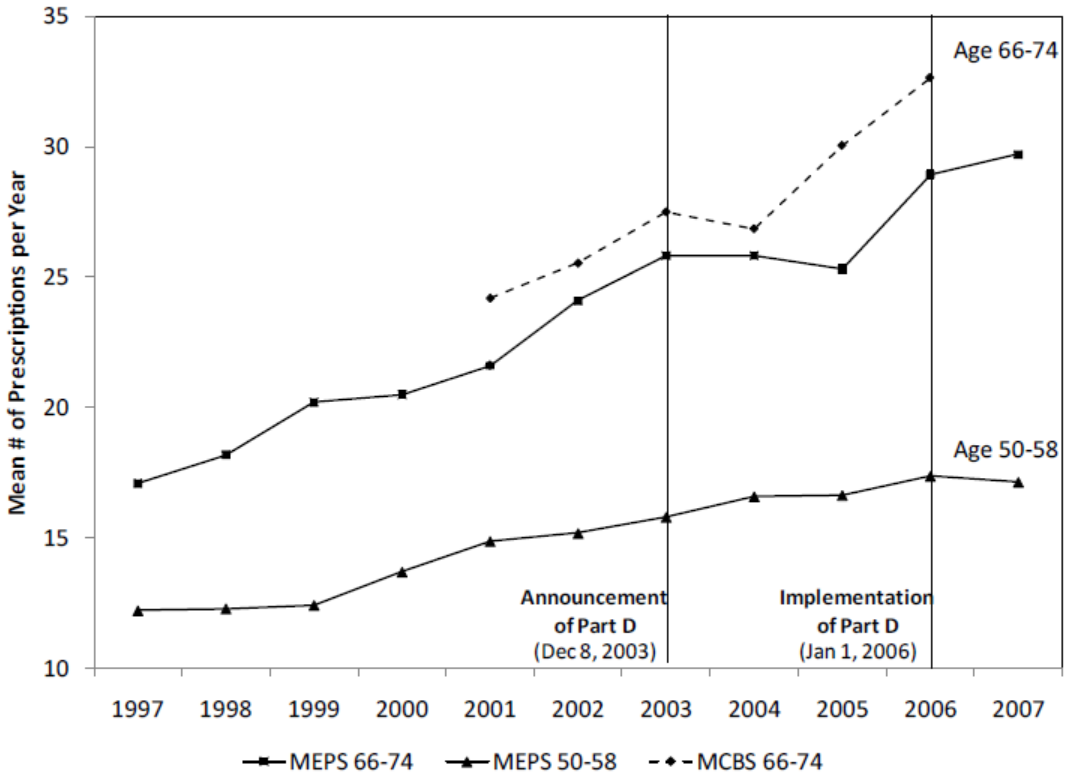
Notes: Author's calculation using MEPS 2000-2006. Means are weighted with population weights and spending levels are adjusted for inflation to 2006 dollars using the CPI-U.

Figure 2.4: Percent who said they followed news about the Medicare prescription drug debate "very closely" or "somewhat closely"



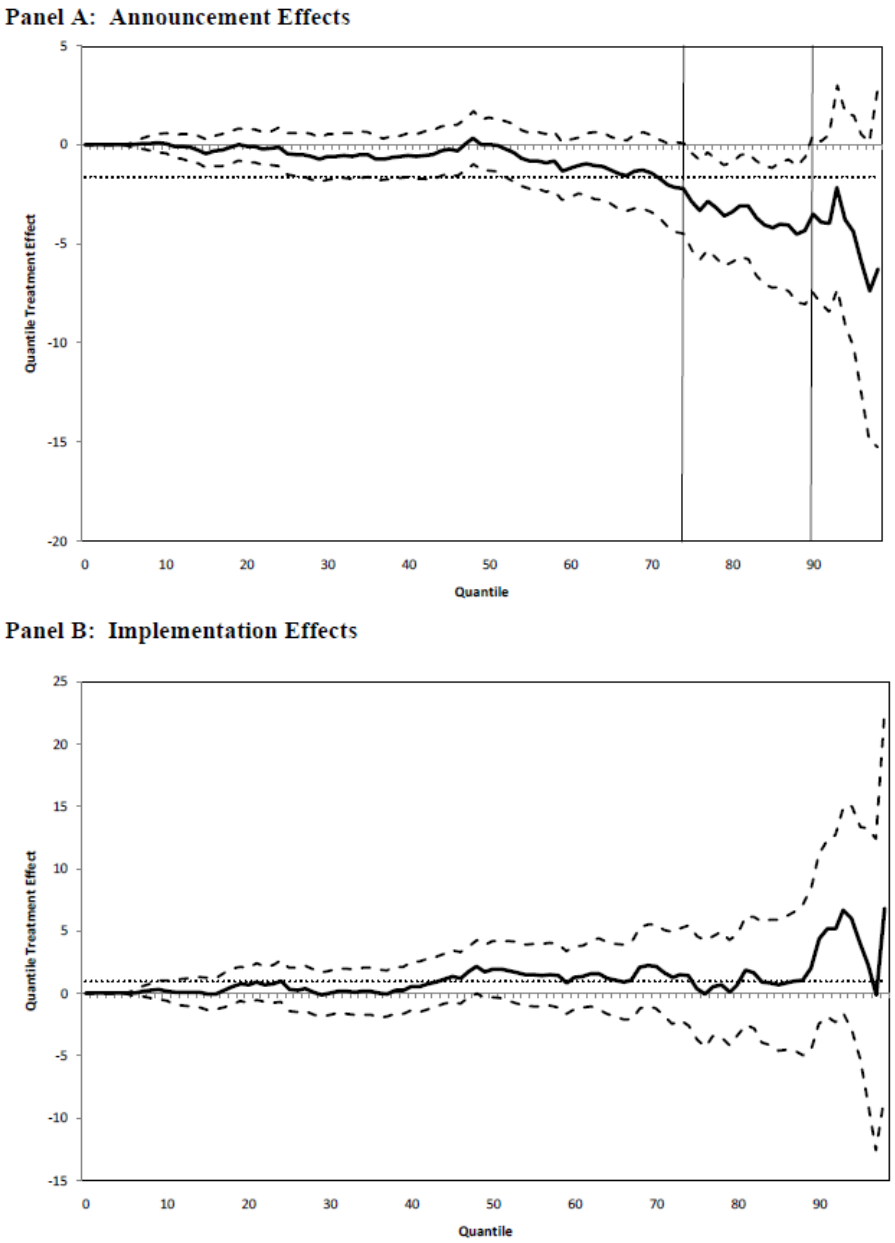
Source: Kaiser Family Foundation Health Poll Report, 2004

Figure 2.5: Mean Annual Drug Utilization in MEPS and MCBS



Notes: Author's calculation using MEPS 1997-2007 and MCBS 2001-2006, non-institutionalized population ages 66-74, weighted. Includes individuals who appear in the sample for 2 or more consecutive years.

Figure 2.6: Conditional Quantile Announcement and Implementation Effects



Notes: Solid line represents quantile announcement (implementation) effects for every quantile of the distribution of total prescriptions conditional on the implementation (announcement) and a full set of control variables. Dashed lines represent block bootstrapped 95% confidence intervals (750 replications) and the dotted line is the mean treatment effect. Regressions are weighted and Medicaid beneficiaries are included. MCBS 2001-2006.

Figure 2.7: Chronic and Acute Announcement and Implementation Effects

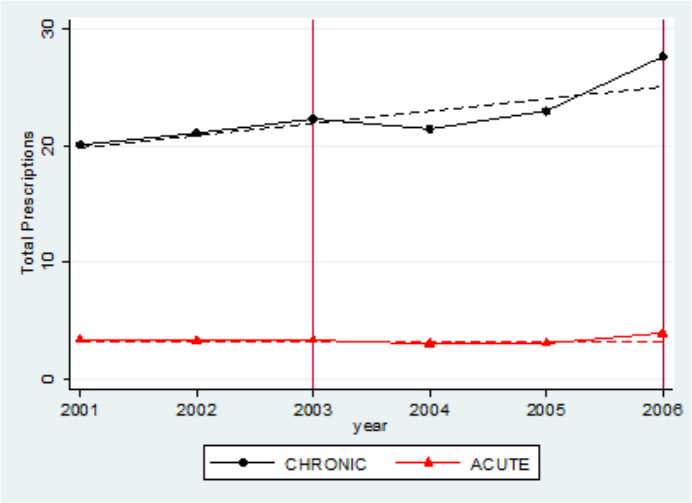
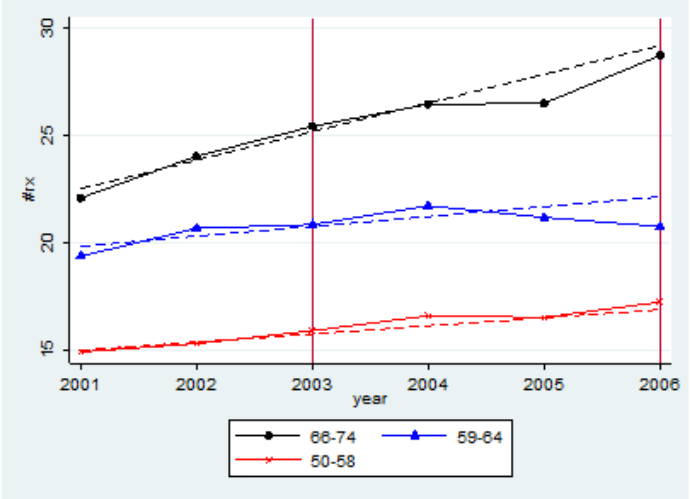


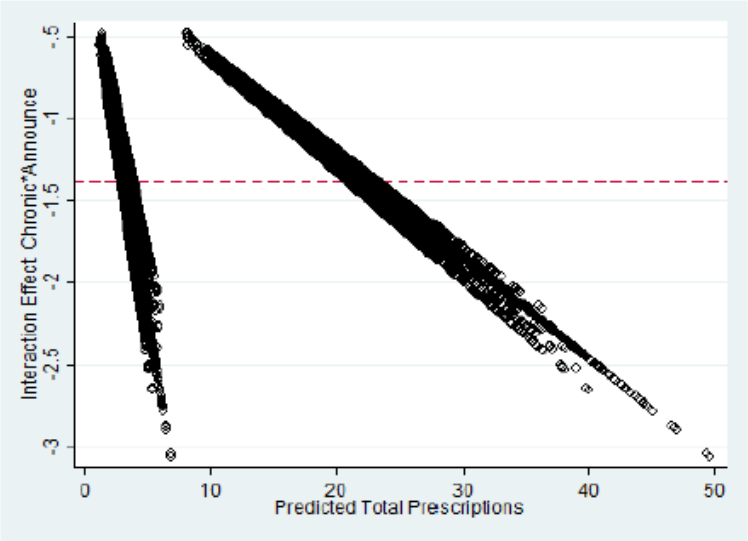
Figure 2.8: Age-Eligible and Age-Ineligible Announcement and Implementation Effects



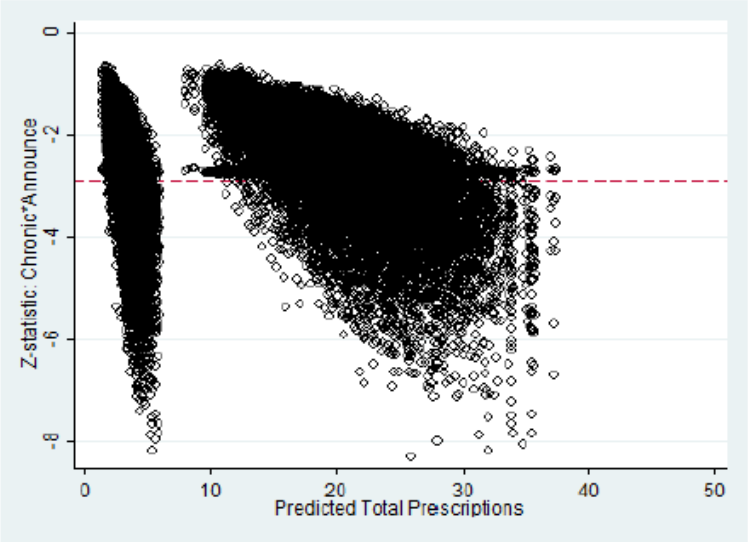
Notes: MCBS 2001-2006, weighted. The points represent weighted sample means. The dashed line shows the preannouncement predicted trends from the model described in Equation 2.1 excluding controls. Chronic and acute categories are defined by the median assignment rule and correspond to the results in Table 2.5. The age group graph corresponds to the results in Table 2.11.

Figure 2.9: Interaction Effects and Z-statistics as a Function of Predicted Total Prescriptions

Panel A: Marginal effects for the interaction of the Announce and Chronic drug indicator



Panel B: Z-statistics for the Interaction Effects



Notes: MCBS 2001-2006, weighted; The points represent the estimate of marginal effect of the interaction between the chronic and announce indicator and the corresponding z-statistic for each person in the sample ages 66-74.

Figure 2.10: Mean Doctor Visits, 2001-2006

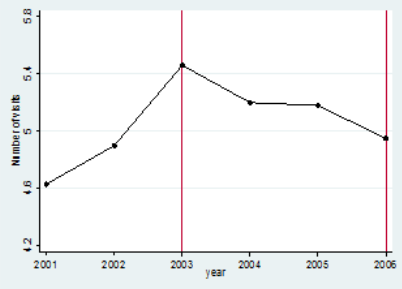


Figure 2.11: Probability of initiation (conditional on not filling a drug in the therapeutic class in t-1)

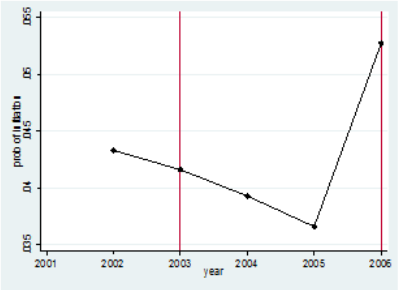


Figure 2.12: Probability of discontinuation (conditional on filling a drug in the therapeutic class in t-1)

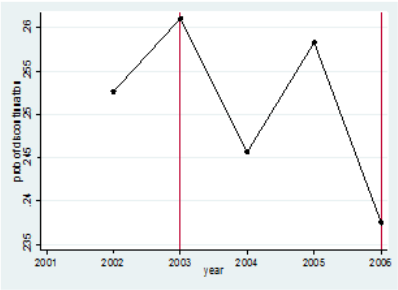


Table 2.1: Descriptive Statistics, 2001-2006

	MEPS			MCBS		
	Age 50-58	Age 59-64	Age 66-74	Age 66-74	Acute	Chronic
	(1)	(2)	(3)	(4)	(5)	(6)
Outcomes						
Any Prescriptions Filled	0.76	0.83	0.90	0.92	0.55	0.86
Avg. # of Prescriptions	16.18	20.83	25.63	28.21	3.26	22.14
Annual Drug Expenditures	1,106.60	1,354.87	1,600.07	1,789.24	161.57	1,379.97
Avg. Tot. Paid per Script (excl. 2006)	60.94	63.27	62.19	67.94	48.01	69.62
Fract. Paid Out-of-Pocket (excl. 2006)	0.47	0.47	0.56	0.45	0.54	0.43
Demographics						
Age	53.80	61.33	69.83	70.29	70.35	70.31
Male	0.48	0.48	0.45	0.45	0.41	0.43
Black	0.10	0.10	0.09	0.09	0.08	0.09
Hispanic	0.08	0.07	0.07	0.07	0.06	0.06
No College	0.46	0.53	0.62	0.56	0.55	0.56
Some College	0.23	0.19	0.17	0.26	0.26	0.26
College	0.31	0.28	0.21	0.19	0.19	0.19
Employed	0.81	0.61	0.25	0.19	0.17	0.18
Insurance Coverage						
Medicare	0.05	0.08	0.99	1.00	1.00	1.00
Medicare HMO	-	-	-	0.17	0.17	0.17
Medicaid	0.07	0.06	0.10	0.11	0.12	0.11
Private Insurance	0.79	0.78	0.59	0.67	0.69	0.68
Drug Insurance* (excl. 2006)	0.81	0.78	0.52	0.79	0.81	0.81
Observations	20,719	9,617	10,358	20,072	11,229	17,362
# of Unique Persons	12,934	6,262	6,544	10,079	6,840	9,036
# of Prescriptions	352,889	211,718	278,753	573,720	66,874	450,909

Notes: Means are weighted and pooled for 2001-2006 unless otherwise noted. In columns 5 and 6, unconditional means are shown for outcome variables and the remaining variables show means conditional on purchasing an acute or chronic drug. Drug classes that could not be classified as either acute or chronic are excluded in columns 5 and 6.

Table 2.2: Aggregate Announcement and Implementation Effects

Dependent variable:	Total Prescriptions			Log (Total Prescriptions)		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Full Sample						
Announce		-2.1140***	-1.6064***		-0.0475***	-0.0342
		(0.430)	(0.621)		(0.018)	(0.030)
Implement	2.9943***		0.9064	0.0682**		0.0237
	(0.724)		(1.115)	(0.030)		(0.050)
t	1.2970***	1.9667***	1.7788***	0.0514***	0.0666***	0.0616***
	(0.177)	(0.157)	(0.240)	(0.008)	(0.007)	(0.011)
Observations	20,072	20,072	20,072	20,072	20,072	20,072
Panel B: Excluding Medicaid Beneficiaries						
Announce		-1.8173***	-1.5008**		-0.0489**	-0.0407
		(0.426)	(0.631)		(0.019)	(0.031)
Implement	2.5134***		0.5658	0.0674**		0.0146
	(0.717)		(1.126)	(0.032)		(0.053)
t	1.0534***	1.6197***	1.5027***	0.0470***	0.0622***	0.0591***
	(0.178)	(0.156)	(0.245)	(0.009)	(0.007)	(0.012)
Observations	17,763	17,763	17,763	17,763	17,763	17,763

Notes: *** p<0.01, ** p<0.05, * p<0.1. Clustered standard errors at the person level. Regressions are weighted and include a full set of control variables. MCBS 2001-2006; Ages 66-74.

Table 2.3: Aggregate Announcement and Implementation Effects-Alternative Outcomes

Dependent variable:	Total Prescriptions		Prescriptions Conditional on Use		Any Use		Log (Total Expenditures)		Log (OOP Expenditures)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Announce		-1.6064***		-1.7570***		-0.0017		-0.0027		-0.021
		(0.621)		(0.655)		(0.008)		(0.057)		(0.037)
Implement	2.9943***	0.9064	3.1684***	0.8822	-0.0007	-0.0029	-0.0103	-0.0139	-0.1285***	-0.1559***
	(0.724)	(1.115)	(0.757)	(1.169)	(0.007)	(0.012)	(0.056)	(0.094)	(0.034)	(0.061)
t	1.2970***	1.7788***	1.3117***	1.8396***	0.0042**	0.0047*	0.1324***	0.1332***	0.0938***	0.1001***
	(0.177)	(0.240)	(0.183)	(0.254)	(0.002)	(0.003)	(0.015)	(0.021)	(0.009)	(0.014)
Observations	20,072	20,072	18,478	18,478	20,072	20,072	20,072	20,072	20,072	20,072

Notes: *** p<0.01, ** p<0.05, * p<0.1. Clustered standard errors at the person level. Regressions are weighted and include a full set of control variables. Medicaid beneficiaries are included. MCBS 2001-2006; Ages 66-74.

Table 2.4: Aggregate Announcement and Implementation Effects-Heterogeneous Effects

Dependent Variable:	Total Prescriptions					
Sub-sample:	Age 66-74	Age 75-85	<=HS Educ	>HS Educ	<=Median Income	>Median Income
	(1)	(2)	(3)	(4)	(5)	(6)
Announce	-1.6064***	0.1568	-1.9196**	-1.1771	-2.2038**	-0.8591
	(0.621)	(0.592)	(0.866)	(0.876)	(0.995)	(0.879)
Implement	0.9064	3.6495***	1.2097	0.5484	0.3662	1.3171
	(1.115)	(1.005)	(1.584)	(1.543)	(1.721)	(1.557)
t	1.7788***	1.3679***	2.0380***	1.3764***	2.4371***	1.0584***
	(0.240)	(0.218)	(0.333)	(0.341)	(0.363)	(0.340)
Observations	20,072	21,403	11,352	8,720	10,384	9,688

Notes: *** p<0.01, ** p<0.05, * p<0.1. Clustered standard errors at the person level. Regressions are weighted and include a full set of control variables. Medicaid beneficiaries are included. Columns 3-6 are estimated for elderly ages 66-74. Median income is computed separately for each year. MCBS 2001-2006.

Table 2.5: Chronic and Acute Announcement and Implementation Effects

Dependent variable:	Total Prescriptions				Log(Total Prescriptions)			
Classification Method:	>50% in drug group		>65% in drug group		>50% in drug group		>65% in drug group	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Chronic*Announce		-1.5667***		-1.4160***		-0.0543		-0.1127***
		(0.509)		(0.444)		(0.035)		(0.036)
Chronic*Implement	3.5449***	1.5100	3.2464***	1.4074*	0.0088	-0.0618	0.0404	-0.1060*
	(0.602)	(0.924)	(0.534)	(0.807)	(0.035)	(0.058)	(0.036)	(0.060)
Announce		-0.1567		-0.0322		-0.0179		0.0191
		(0.159)		(0.090)		(0.026)		(0.020)
Implement	0.9651***	0.7608***	0.3655***	0.3230**	0.1547***	0.1314***	0.1031***	0.1278***
	(0.163)	(0.264)	(0.091)	(0.151)	(0.025)	(0.042)	(0.019)	(0.033)
Chronic	16.2625***	15.4795***	12.9526***	12.2450***	1.4718***	1.4447***	1.5046***	1.4483***
	(0.474)	(0.495)	(0.404)	(0.418)	(0.031)	(0.033)	(0.033)	(0.034)
t	-0.0494	-0.0023	-0.0048	0.0049	-0.0187***	-0.0133	-0.0128***	-0.0185**
	(0.041)	(0.058)	(0.023)	(0.033)	(0.006)	(0.009)	(0.005)	(0.007)
Chronic*t	0.5777***	1.0473***	0.6883***	1.1127***	0.0419***	0.0582***	0.0551***	0.0889***
	(0.144)	(0.196)	(0.124)	(0.168)	(0.009)	(0.013)	(0.010)	(0.013)
Announce + Chronic*Announce		-1.7234***		-1.4481***		-0.0722**		-0.0936***
		(0.526)		(0.441)		(0.031)		(0.032)
Implement + Chronic*Implement	4.5100***	2.2707**	3.6119***	1.7304**	0.1635***	0.0697	0.1435***	0.0218
	(0.622)	(0.957)	(0.531)	(0.802)	(0.032)	(0.053)	(0.033)	(0.054)
Observations	40,144	40,144	40,144	40,144	40,144	40,144	40,144	40,144

Notes: *** p<0.01, ** p<0.05, * p<0.1. Clustered standard errors at the person level. Regressions are weighted and include a full set of control variables. Medicaid beneficiaries are included. The bottom panel presents linear combinations of the coefficients and their standard errors to show absolute announcement and implementation effects for chronic drugs. The classification method presented is either the median assignment rule (more than 50% of drugs in the therapeutic class are either chronic or acute) or the assignment rule in which more than 65% of drugs in the class are either chronic or acute (classes with fewer than 65% of drugs in both groups are excluded). MCBS 2001-2006; Ages 66-74.

Table 2.6: Robustness across Classification Methods – Panel A: Total Prescriptions

Dependent variable:	Total Prescriptions					
Classification Method:	>50%	>55%	>60%	>65%	>70%	>75%
	(1)	(2)	(3)	(4)	(5)	(6)
Chronic*Announce	-1.5667***	-1.3048***	-1.3587***	-1.4160***	-0.9821**	-0.3056*
	(0.509)	(0.480)	(0.441)	(0.444)	(0.387)	(0.180)
Chronic*Implement	1.5100	1.4968*	0.8539	1.4074*	1.4556**	0.5967*
	(0.924)	(0.875)	(0.798)	(0.807)	(0.705)	(0.321)
Announce	-0.1567	-0.1628	-0.1315	-0.0322	-0.0282	-0.0229
	(0.159)	(0.157)	(0.141)	(0.090)	(0.082)	(0.074)
Implement	0.7608***	0.7448***	0.7365***	0.3230**	0.2732**	0.2794**
	(0.264)	(0.261)	(0.235)	(0.151)	(0.137)	(0.120)
Chronic	15.4795***	14.2163***	10.9330***	12.2450***	10.1698***	1.7792***
	(0.495)	(0.470)	(0.417)	(0.418)	(0.365)	(0.159)
Announce + Chronic*Announce	-1.7234***	-1.4676***	-1.4902***	-1.4481***	-1.0102***	-0.3285**
	(0.526)	(0.494)	(0.447)	(0.441)	(0.383)	(0.167)
Implement + Chronic*Implement	2.2707**	2.2416**	1.5903**	1.7304**	1.7288**	0.8761***
	(0.957)	(0.902)	(0.810)	(0.802)	(0.698)	(0.304)
Observations	40,144	40,144	40,144	40,144	40,144	40,144
Chronic: Mean Prescriptions	22.14	20.62	17.46	17.28	14.63	4.22
Acute: Mean Prescriptions	3.26	3.26	2.85	1.37	1.24	1.24
Total # of Prescriptions	517,783	486,815	413,313	379,278	322,131	110,391

Table 2.7: Robustness across Classification Methods – Panel B: Log Total Prescriptions

Dependent variable:	Log(Total Prescriptions)					
Classification Method:	>50%	>55%	>60%	>65%	>70%	>75%
	(1)	(2)	(3)	(4)	(5)	(6)
Chronic*Announce	-0.0543	-0.0526	-0.0777**	-0.1127***	-0.0993***	-0.0643*
	(0.035)	(0.035)	(0.037)	(0.036)	(0.036)	(0.033)
Chronic*Implement	-0.0618	-0.0638	-0.1112*	-0.1060*	-0.0908	-0.0481
	(0.058)	(0.059)	(0.061)	(0.060)	(0.060)	(0.055)
Announce	-0.0179	-0.018	-0.0159	0.0191	0.0192	0.0187
	(0.026)	(0.026)	(0.025)	(0.020)	(0.020)	(0.020)
Implement	0.1314***	0.1315***	0.1324***	0.1278***	0.1260***	0.1239***
	(0.042)	(0.042)	(0.041)	(0.033)	(0.032)	(0.032)
Chronic	1.4447***	1.3715***	1.1692***	1.4483***	1.3089***	0.2467***
	(0.033)	(0.034)	(0.035)	(0.034)	(0.034)	(0.030)
Announce + Chronic*Announce	-0.0722**	-0.0706**	-0.0936***	-0.0936***	-0.0802**	-0.0456*
	(0.031)	(0.031)	(0.032)	(0.032)	(0.032)	(0.028)
Implement + Chronic*Implement	0.0697	0.0677	0.0212	0.0218	0.0352	0.0758
	(0.053)	(0.053)	(0.054)	(0.054)	(0.054)	(0.047)
Observations	40,144	40,144	40,144	40,144	40,144	40,144
Chronic: Mean Prescriptions	22.14	20.62	17.46	17.28	14.63	4.22
Acute: Mean Prescriptions	3.26	3.26	2.85	1.37	1.24	1.24
Total # of Prescriptions	517,783	486,815	413,313	379,278	322,131	110,391

Notes: *** p<0.01, ** p<0.05, * p<0.1. Clustered standard errors at the person level. Regressions are weighted and include a full set of control variables. Medicaid beneficiaries are included. The bottom panel presents linear combinations of the coefficients and their standard errors to show absolute announcement and implementation effects for chronic drugs. Each column represents a classification assignment rule of increasing stringency as described in Section 5.1. MCBS 2001-2006; Ages 66-74.

Table 2.8: Announcement and Implementation Effects for Top Chronic and Acute Therapeutic Classes

Panel A: Top 8 Chronic Drug Classes

Dependent Variable:	Total Prescriptions		Mean # of	Total # of
	Announce	Implement	Prescriptions	Prescriptions
Drug Class:	(1)	(2)	(3)	(4)
Cardiovascular	-0.3079*	0.9655***	5.8901	120,965
	(0.173)	(0.310)		
Cardiac drugs	-0.0665	0.1012	1.9975	41,022
	(0.108)	(0.185)		
Diuretics	-0.2064**	-0.1903	1.7612	36,169
	(0.091)	(0.154)		
Hypoglycemics	-0.2360**	0.5286**	1.7488	35,916
	(0.113)	(0.209)		
Autonomic drugs	-0.0941	0.1252	1.6904	34,716
	(0.086)	(0.149)		
Psychotherapeutic drugs	-0.2197**	0.1103	1.5812	32,474
	(0.104)	(0.180)		
Gastrointestinal preparations	-0.2556***	0.0419	1.5326	31,474
	(0.092)	(0.156)		
Antiarthritics	-0.1277*	-0.1048	1.0987	22,563
	(0.068)	(0.118)		

Panel B: Top 8 Acute Drug Classes

Dependent Variable:	Total Prescriptions		Mean # of	Total # of
	Announce	Implement	Prescriptions	Prescriptions
Drug Class:	(1)	(2)	(3)	(4)
Analgesics	-0.1340**	0.1826	0.7640	15,690
	(0.063)	(0.112)		
EENT preparations	0.0312	0.2302*	0.7469	15,339
	(0.075)	(0.119)		
Antiinfectives	-0.0143	0.048	0.5549	11,395
	(0.038)	(0.062)		
Antihistamines	-0.0359	-0.0187	0.4190	8,604
	(0.046)	(0.075)		
Antiinfectives, miscellaneous	-0.0402	0.0826	0.2980	6,120
	(0.034)	(0.055)		
Skin preparations	0.0127	0.0554	0.2546	5,228
	(0.034)	(0.052)		
Muscle relaxants	-0.0091	0.0275	0.1408	2,891
	(0.020)	(0.034)		
Cough and cold preparations	0.0377*	0.1265***	0.1352	2,776
	(0.021)	(0.033)		

Notes: *** p<0.01, ** p<0.05, * p<0.1. Clustered standard errors at the person level. Columns 1 and 2 are coefficients from 16 regressions of total prescriptions for the drug class on the announcement and implementation indicators, a linear time trend, and a full set of control variables. Regressions are weighted and Medicaid beneficiaries are included. The mean number of prescriptions include zeros. MCBS 2001-2006; Ages 66-74.

Table 2.9: Announcement and Implementation Effects for Pharmaceutical Prices by Medicare Market Share

Dependent Variable:	Price	Log(Price)
	(1)	(2)
MMS*Year2001	-1.2987	-0.1006
	(10.673)	(0.134)
MMS*Year2002	5.5946	-0.0473
	(10.021)	(0.118)
MMS*Year2003	-4.5142	-0.1458
	(10.656)	(0.115)
MMS*Year2004	-4.4471	-0.1957*
	(10.376)	(0.119)
MMS*Year2005	-1.2343	-0.1451
	(10.988)	(0.117)
MMS*Year2006	-16.3658	-0.2405*
	(11.754)	(0.123)
(MMS*2004-MMS*2003) - (MMS*2002-MMS*2001)	-6.8262	-0.1033
	(10.349)	(0.142)
(MMS*2005-MMS*2003) - (MMS*2003-MMS*2001)	6.4955	0.0458
	(14.005)	(0.161)
(MMS*2006-MMS*2005) - (MMS*2002-MMS*2001)	-22.0248*	-0.1488
	(12.032)	(0.147)
Weighted by #Rx 02-03	Y	Y
Year Fixed Effects	Y	Y
Drug Fixed Effects	Y	Y
Observations	924	924

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors in parentheses. This regression is estimated at the drug-year level from Equation 4. The MMS is the fraction of prescriptions that are purchased by Medicare beneficiaries for each drug in the 2002-2003 pooled MEPS. The bottom panel presents linear combinations of the coefficients and their standard errors. MEPS 2000-2006.

Table 2.10: Announcement Effects for Drug Insurance Coverage

Dependent variable:	I(Drug-Insured=1)						
	All Drug Insured		Employer Sponsored Insurance	Self-Purchased Private (excl. Medigap)	Medigap	Private HMO	Medicare HMO
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Announce	-0.0317***	-0.0365***	-0.0035	0.0235**	-0.0203***	0.0144	-0.0158***
	(0.010)	(0.011)	(0.013)	(0.009)	(0.007)	(0.010)	(0.005)
t	0.0179***	0.0190***	0.0073	-0.0079**	0.0079***	-0.0118***	0.0055***
	(0.004)	(0.004)	(0.005)	(0.003)	(0.002)	(0.004)	(0.002)
Excluding Medicaid Beneficiaries	N	Y	Y	Y	Y	Y	Y
Observations	16,876	14,981	14,981	14,981	14,981	14,981	14,981

Notes: *** p<0.01, ** p<0.05, * p<0.1. Clustered standard errors at the person level. The regressions are weighted and include a full set of control variables. The coefficients are from an OLS regression of an indicator for drug insurance (for each type of drug insurance) on the announcement indicator and a linear time trend. MCBS 2001-2005.

Table 2.11: Announcement and Implementation Effects for Age-Eligible (Age 66-74) and Age-Ineligible (Age 50-58 and Age 59-64)

Dependent variable:	Total Prescriptions		Log (Total Prescriptions)	
	(1)	(2)	(3)	(4)
Age66-74*Announce		-0.7612		-0.0542
		(1.424)		(0.073)
Age66-74*Implement	0.1053	-0.8883	-0.0403	-0.1112
	(1.305)	(2.180)	(0.063)	(0.110)
Age59-64*Announce		-0.2371		-0.0982
		(1.429)		(0.078)
Age59-64*Implement	-1.3254	-1.6354	-0.0789	-0.2073*
	(1.169)	(2.091)	(0.068)	(0.116)
Announce		0.081		0.049
		(0.729)		(0.043)
Implement	0.0975	0.2036	-0.0025	0.0617
	(0.611)	(1.113)	(0.038)	(0.065)
Announce + Age66-74*Announce		-0.6802		-0.0052
		(1.223)		(0.059)
Implement + Age66-74*Implement	0.2028	-0.6846	-0.0428	-0.0496
	(1.153)	(1.874)	(0.050)	(0.089)
Announce + Age59-64*Announce		-0.156		-0.0492
		(1.187)		(0.063)
Implement + Age59-64*Implement	-1.2279	-1.4318	-0.0814	-0.1457
	(0.996)	(1.718)	(0.056)	(0.094)
Observations	40,694	40,694	40,694	40,694

Notes: *** p<0.01, ** p<0.05, * p<0.1. Clustered standard errors at the person level. Regressions are weighted and include a full set of control variables. Medicaid beneficiaries are included. The coefficients are from estimating Equation 1 with the age-eligible and two age-ineligible groups. Age group main effects and linear trends are included but not reported to conserve space. The bottom panel presents linear combinations of the coefficients and their standard errors to show absolute announcement and implementation effects for the 66-74 and 59-64 age groups. MEPS 2001-2006.

Chapter 3

Perverse Reverse Price Competition: Average Wholesale Prices and Medicaid Pharmaceutical Spending (with Mark Duggan and Judith Hellerstein)

3.1 Introduction

Generic drugs comprise an increasing share of total prescriptions dispensed in the U.S., rising from nearly 50 percent in 1999 to 75 percent in 2009 (Berndt and Aitken, 2010). The generic drug market has typically been viewed as approximately competitive with price approaching marginal costs. Consequently, the widespread availability of generic drugs is largely perceived as extremely beneficial to consumers and, more specifically, to those who bear the burden of paying for these treatments. However, one important aspect of the generic drug market that has been largely unexplored is how drug procurement by the government and private insurers impacts price competition among generic drugs. In particular, procurement rules may distort generic drug prices away from marginal cost by perversely rewarding higher-priced generics with greater market share.

The main objective of this paper is to investigate how generic drug manufacturers compete in the presence of the procurement rules of the Medicaid program. We examine the impact of procurement on competition by evaluating how drug pur-

chasing patterns responded to a plausibly exogenous negative price shock generated by a significant government intervention. During the period under study, Medicaid accounted for nearly 20% of all prescription drug expenditures in the U.S. Thus, distortions in the market for drugs purchased by Medicaid have potentially large impacts on overall drug spending, and health care spending more generally.

How might procurement distort generic competition? To fix ideas, first consider a stylized market without procurement. In the absence of government and private insurance, all consumers pay pharmacies in cash for drugs purchased. Consumers are indifferent between bioequivalent generic drugs that are produced by one manufacturer versus another[1], but purchase the drug from the pharmacy with the lowest price. In turn, pharmacies stock and dispense the version of the generic drug that is least costly, choosing among possibly dozens of manufacturers. Competition among generic manufacturers for pharmacy market share drives the equilibrium price down to marginal cost.

The actual market for generic drugs differs from this textbook scenario for two main reasons. First, there are very few cash-paying consumers in the pharmaceutical market. The last few decades have seen a dramatic rise in prescription drug insurance coverage. The fraction of drug spending paid for by public and private payers has grown from 34% in 1980 to nearly 80% in 2000, and 92% in 2010 (Berndt and Aitken, 2010). Thus cash-paying consumers currently account for only 8% of payments. The remainder of consumers are virtually price insensitive, since they

[1]Since generic drugs are regulated to be identical in their chemical structure and strength, a specific manufacturer is rarely designated in a physician's written prescription for a generic drug.

typically pay a copay that is fixed across equivalent generic drugs. Consequently, consumers play little role in generic price competition, leaving public and private payers– who account for the lion's share of payments to pharmacies- as the main driving force.

Second, the largest purchasers of generic drugs do not pay pharmacies' posted prices. Government and private insurers set their own rules to determine how much to reimburse pharmacies for drugs dispensed to their beneficiaries. For Medicaid, like other payers, reimbursement for each prescription is based on a benchmark price called the average wholesale price (AWP)[2], which is published in several pricing catalogues. For each product, this list price is reported to the catalogues by generic manufacturers themselves, and until recently has been subject to essentially no independent verification of its resemblance to the actual average price that pharmacies pay manufacturers to acquire drugs. As a result, generic manufacturers have had an incentive to compete for pharmacy market share by reporting AWPs that are much greater than actual average prices, as higher "spreads" lead to larger pharmacy profits. Put another way, since higher AWPs generate higher reimbursement for pharmacies, manufacturers might report higher and higher AWPs in order to induce pharmacies to stock their drug rather than a competitor's drug. This may partially explain why differences between published and actual average prices of more than 1000% have been uncovered in recent government audits for some generic drugs. Thus, competition among manufacturers may increase rather than

[2]A few states use the Wholesale Acquisition Cost (WAC) or the minimum of the WAC and AWP as their reimbursement benchmark. In 2000 – the year of the policy intervention we study– Massachusetts and Rhode Island were the only two states using the WAC as their primary reimbursement benchmark (National Pharmaceutical Council, 2000).

reduce the prices on which Medicaid reimbursement is based, leading to inflated Medicaid spending.

To empirically investigate the impact of Medicaid procurement on price competition, we examine an intervention that caused a sharp decline in price for a well-defined set of generic drugs. In the late 1990s, an investigation by the Department of Justice (DOJ) and the National Association of Medicaid Fraud Control Units (NAMFCU) "revealed a pattern of misrepresentations by some drug manufacturers of the average wholesale prices and wholesale acquisition costs of certain of their products."[3] As a result of this audit, in May 2000, states were advised to reduce the AWP used to reimburse pharmacies by as much as 95% for approximately 400 generic and off-patent injectable, infusion, and inhalation drug products. This intervention provides a useful setting for our study because it differentially targeted drugs within classes of bioequivalent products, allowing for a comparison of drug purchases before and after the intervention for DOJ targeted drugs and their competitors. For example, in the market for Acetylcysteine, the DOJ recommended that Medicaid lower the AWP to the audited price for several products produced by three manufactuers, but did not make any price recommendations for the other seven manufacturers of this drug. Finding shifts in purchases away from targeted drugs towards their competitors as the relative price of targeted drugs declined would be evidence that pharmacies respond to the perverse incentives of the Medicaid program by stocking products with the highest AWPs.

Using more than a decade of Medicaid State Drug Utilization Data from the

[3]Office of the Attorney General, Medicaid Fraud Control Unit, State of New York, 2000.

Centers for Medicare and Medicaid Services (CMS), we demonstrate that actual reimbursement per prescription purchased declined substantially for targeted drugs following the 2000 DOJ recommendations, relative to competitor products whose AWPs were not targeted by the DOJ. Also after the intervention, we find evidence of a decline in the number of targeted drug prescriptions dispensed by pharmacies and an increase in the number of competitor drugs dispensed. Overall, the market share for targeted drugs fell by about 45% through 2004 relative to the baseline year. While the findings in this paper are preliminary, they suggest that pharmacies substituted away from drug products whose prices were reduced. This is inconsistent with a standard model of price competition in which lower-priced drugs capture higher market share. Thus, these findings provide preliminary evidence that Medicaid procurement incentives could lead generic manufacturers to compete by overstating AWPs and thus reducing the cost savings from these drugs.

The remainder of the paper proceeds as follows. Section 3.2 provides detailed background about the AWP and related literature. Section 3.3 discusses the intervention. Section 3.4 describes the data and sample restrictions. Section 3.5 outlines the empirical framework and presents the descriptive statistics and results. Section 3.6 concludes.

3.2 Background

3.2.1 AWP Basics

AWP-based reimbursement was first introduced by California's Medicaid program in 1969, at a time when public and private insurers were just beginning to pay for prescription drugs (insurers accounted for only 15% of drug spending in that year).[4] Its predecessor, "cost-based" reimbursement, reimbursed pharmacies the exact amount they were charged by manufacturers to purchase the drug plus a fixed dispensing fee to cover labor and capital costs. This method led to a multitude of reimbursement amounts for identical products. More importantly, since pharmacies would receive the same (zero) profit from reimbursement regardless of their acquisition costs, drug manufacturers could in turn set high prices without reducing pharmacies' demand for their product.

In contrast, the AWP approach provided an opportunity for pharmacies to profit from reimbursement through the existence of a "spread." The spread is the difference between the reimbursement amount (which is computed as a fixed proportion of each drug's AWP) and the pharmacy's actual acquisition costs. A positive or negative spread is a profit or a loss for the pharmacy. The Medicaid program anticipated that pharmacies would seek to maximize this spread by searching for the lowest priced versions of generic drugs. In turn, this would induce price competition among manufacturers, which would thereby allow Medicaid to lower their reimbursement amounts over time. Other states and private insurers soon adopted

[4]Calculations from CMS National Health Accounts.

California's model and publishers such as the Red Book, Blue Book, Medi-Span, and First Data Bank, entered the market to meet the demand for data on average wholesale prices (See Danzon, et al 2005 for a more detailed summary of the history of the AWP).

A typical AWP-based reimbursement rule used by public and private payers is defined as follows. For each drug product j dispensed to a recipient of insurance type i, the pharmacy receives a reimbursement amount equal to[5]:

$$Reimbursement_{ij} = (1 - \alpha_i) \cdot AWP_j + DispensingFee_i \tag{3.1}$$

Where $0 \leq \alpha_i \leq 1$. Thus a pharmacy earns a profit of $\Pi_{ij} = Reimbursement_{ij} - p_j^a$, where p_j^a is the acquisition cost of drug j from the manufacturer.

Given that α_i and the dispensing fee are fixed across drugs, the AWP_j and p_j^a are clearly the crucial factors in determining which generic version the pharmacy will stock. In pricing catalogues, the AWP is reported for each drug at the level of the National Drug Code (NDC). The NDC is a unique eleven-digit identification number assigned by the FDA to every drug product distributed in the U.S. The first 5 digits uniquely identify the manufacturer, the next 4 digits identify the product code (which include the strength, dosage form, and formulation of the drug), and the final 2 digits identify the package size and type. Thus a single NDC code uniquely defines one drug produced by one particular manufacturer and is denoted by j in the equation above.

[5]This rule generates what is known as the "Estimated Acquisition Cost" (EAC) and reimbursement is based on the minimum of the EAC or "Usual and Customary Charge" (U&C).

For the Medicaid program, there are a few caveats regarding the parameters of the reimbursement rule. First, α_i may vary by drug type. Reimbursement rules typically specify two separate α_i: one for brand name drugs and another for generic drugs (the former is typically smaller).[6] Second, the AWP is sometimes replaced by a lower, government-determined price for certain generic drugs. These prices are known as the Federal Upper Limit (FUL) or the Maximum Allowable Cost (MAC). FULs, which were first established by the federal government in 1987, denote the maximum price at which the Medicaid program will reimburse pharmacies for certain drugs. All generic drugs that have three or more manufacturers are subject to these limits, which are set at 150 percent of the lowest published AWP among all equivalent versions of the drug (GAO, 2006). Over 200 drugs are on the FUL list (OIG, 2001), though reports by the OIG suggest that not all eligible generic drugs receive FULs (OIG Feb 2004, OIG Dec 2004). MACs are similar reimbursement limits for generic drugs that are set by each individual state's Medicaid program. These limits tend to be wider in scope and are more aggressive than FULs (Abramson, 2004). Third, there is also large variation in the α_i and the dispensing fee across states: in the first quarter of 2011, the Medicaid α_i ranged from 0.05 and 0.50.

3.2.2 The AWP and Medicaid Procurement Incentives

In the previous section we have outlined the four main parameters governing transactions between payers, pharmacies, and manufacturers (AWP_j, p_j^a, α_i, $DispensingFee_i$). In this section we briefly discuss how manipulation of these pa-

[6] See State Medicaid reimbursement rules available on CMS website.

rameters can lead to a perverse form of price competition in which manufacturers compete to offer pharmacies the highest-priced version of a generic drug. We focus on the Medicaid program, but an analogous discussion can be applied to other payers.

First, recall that pharmacies earn a profit from Medicaid reimbursement if it generates a positive "spread"– that is, the reimbursement (which is proportional to the AWP_j as in Equation 3.1) exceeds the cost that pharmacies pay to acquire the drug from manufacturers p_j^a. Thus, when choosing among bioequivalent NDCs, pharmacies prefer to stock the NDC that generates the largest spread. In turn, manufacturers have an incentive to compete for pharmacy market share by offering the largest possible spread between AWP_j and p_j^a. They can do so by bidding up the AWP_j and bidding down the p_j^a.

While it may have been the original intent of Medicaid's reimbursement policy that the AWP reflect some average of the actual prices that pharmacies pay wholesalers, in practice, manufacturers self-report the AWP_j to the pricing catalogues. These self-reports are not subject to any independent verification (GAO, 2002). Consequently, as the GAO (2002) explains, the Average Wholesale Price "is neither an average nor a price that wholesalers charge ... it is a number that manufacturers derive using their own criteria; there are no requirements or conventions that AWP reflect the price of any actual sale of drugs by a manufacturer." Thus, since this price can be easily manipulated, manufacturers have an incentive to compete for pharmacy market share by simply reporting higher AWPs.[7]

[7]If the AWP reflected some average of the actual prices that pharmacies pay wholesalers, then

Under the Medicaid procurement rules and AWP self-reports, we would expect there to be a positive spread (i.e. AWP inflation) in equilibrium. A zero spread ($AWP_j = p_j^a$) is not sustainable, since if one manufacturer sets an AWP_j such that $AWP_j = p_j^a$, then another firm can report $AWP_j' > AWP_j = p_j^a$ and capture the entire market.

Numerous studies conducted by the CBO, GAO, and OIG (e.g. CBO 2004, OIG 2002, GAO, 2001) have uncovered spreads for brand and generic drugs which are consistent with the theory above. In one particularly striking study, the OIG (2002) conducted an audit of approximately 200 randomly selected pharmacies in eight states to obtain 8,728 invoice prices for prescription drugs. The authors found that on average pharmacies' actual drug acquisition price was 83% of the published AWP for brand name drugs, 56% of the AWP for generic drugs without an FUL, and 28% of the AWP for drugs *with* an FUL. Since generic drugs are only subject to an FUL when there are 3 or more manufacturers, this suggests that the spread can be largest in the least concentrated drug markets. If pharmacies' demand responds positively to the spread, increasing levels of competition among manufacturers could lead to *increases* in Medicaid drug spending.

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increasing actual wholesale prices requires that the manufacturer trade off the gain from additional Medicaid purchases against the loss of purchases from other payers. In cases where Medicaid payments are a small share of sales, manufacturers may find it profit-maximizing to compete by bidding down the p_j^a rather than bidding up the AWP_j.

3.2.3 Related Literature

While the issue of AWP inflation has received considerable attention in government reports and a series of recent lawsuits filed by CMS, it has not been explored in the empirical health economics literature. The most similar study to the present one is Duggan and Scott-Morton (2006) which examines the impact of Medicaid procurement on prices for brand name drugs. However, the case for generics differs from the case for monopolistic brands because Medicaid procurement distorts generic prices through its impact on competition. A second related literature examines the impact of Medicaid and Medicare reimbursement rules on real (e.g. Gruber et al, 1999) and nominal (e.g. Dafny, 2005) medical utilization. This literature documents substitution by providers towards procedures that are more generously reimbursed by public payers. For example, Gruber (1999), uses variation in the Medicaid reimbursement differential between Cesarean and normal childbirth across states to show that the rate of Cesarean births increases in states with larger reimbursement differentials. Dafny (2005) finds that differentials in Medicare reimbursement by the severity of diagnosis lead hospitals to falsely recode patients' diagnoses to the most profitable, high severity codes. While reimbursement methods that distort actual treatment decisions could potentially have health consequences, distortions in the choice of manufacturers for generic drugs will not affect health. In this sense, our study is more similar to Dafny (2005).

3.3 Intervention

3.3.1 Details of the DOJ Intervention

This study focuses on a national investigation of AWPs by the DOJ and NAMFCU in the late 1990s. The agencies collected actual wholesale pricing data from wholesalers' catalogues which "revealed a pattern of misrepresentations by some drug manufacturers of the average wholesale prices and wholesale acquisition costs of certain of their products." The actual wholesale prices were substantially lower than published AWPs. In an effort "to ensure that Medicaid drug prices are based on true information," NAMFCU sent a letter in February 2000 to all state Medicaid pharmacy directors notifying them of the misrepresentations.[8] Beginning on May 1, 2000, First Data Bank (one of the major suppliers of AWP pricing data) provided states with revised AWPs based on the pricing data uncovered in this investigation for approximately 400 NDCs representing about 50 drug products.[9] States were strongly encouraged by NAMFCU to use these revised AWPs in place of the published AWPs in calculating reimbursements for these drugs.

The 400 NDCs represent about 50 unique drugs and had accounted for $306 million of Medicaid spending in 1999.[10] These drugs are all generic and off-patent injectable, infusion, and inhalation products, including about a dozen oncology drugs

[8]For example, see Office of the Attorney General, Medicaid Fraud Control Unit, State of New York, 2000.

[9]It was also indicated that First Data Bank would update the prices for the 400 NDCs from data from actual wholesale catalogs every 6 months. Though, some states reported concern that First Data Bank would not update the prices unless the DOJ and NAMFCU continued to provide them with wholesale catalogue prices (OIG, 2001).

[10]This figure is calculated by the authors using Medicaid State Drug Utilization Data for all states except Arizona. Arizona is the only state that does not participate in the Medicaid Drug Rebate Program and therefore does not report drug utilization data to CMS.

and blood clotting factors. The full list of drugs targeted by the DOJ is reported in Appendix Table B.1. The extent to which the AWP had been inflated for these drugs is staggering. For the top 20 drugs on this list, published AWPs[11] range from 1.1 to 654 times the size of the actual wholesale price (the median is 7.2 times the actual price). For example, in one case, a manufacturer had reported an AWP of $462.19 for a pack of 25, 6 ml vials of Clindamycin Phosphate, while the investigation found that the actual wholesale price was $162.00.

While all states received the recommendation to adopt the AWP revisions, the use of these new prices was voluntary. A survey conducted by the Office of the Inspector General (2001) found that as of January-March 2001, 30 states had incorporated some or all of the revised AWPs into their reimbursements to pharmacies.[12] The remaining states continued to be supplied with the original, manufacturer-reported AWPs from First Data Bank. States that did not adopt the revisions expressed concern that the new prices were too low, potentially compromising beneficiaries' access to these drugs (OIG, 2001). It is worth noting that states that had not adopted the revisions at the time of the survey may have decided to adopt them later or, in any case, adopted them in a de facto manner when catalogues stopped publishing the manufacturer-reported AWPs for targeted drugs in later years.[13] Conversely, some early adopter states may have eventually discontinued

[11] We compare the DOJ-revised AWPs with the AWPs published in the 1999 Red Book.

[12] The states that adopted the revisions as of 2001 are: AK, CT, DC, DE, FL, HI, ID, IL, IN, KS, KY, MD, MN, MO, MT, ND, NH, NJ, NM, NV, NY, OK, OR, PA, SC, UT, VT, WA, WI, WV.

[13] For instance, the Red Book began to publish revised AWPs for some of the targeted NDCs in 2002.

the use of the AWP revisions.[14]

This intervention provides a "quasi-experiment" to study the impact of Medicaid procurement on price competition for generic drugs. It generated a large and sudden reduction in the price on which Medicaid reimbursement is based for a select set of NDCs within classes of bioequivalent products. Prices for the bioequivalent competitors of targeted NDCs were not directly affected by the DOJ intervention. Thus, for the approximately 50 "treated" drug markets, we can examine how the pharmacy market share for targeted NDCs changed following the reduction in the relative price of these drugs. We proxy for pharmacy market share using Medicaid utilization.[15]

Following the discussion in Section 3.2, we predict that the incentives embedded in Medicaid procurement rules should induce pharmacies to substitute away from the drugs that experienced the price reduction and towards their competitors' products, leading to a decline in the market share for targeted drugs. This contrasts with a standard model of price competition in which the price decline would be predicted to cause an increase in pharmacy market share. Consequently, if pharmacies reward higher-priced drugs with greater market share, this provides an incentive for manufacturers to compete by bidding up the price.

[14]Finally, even among states that adopted the revisions, not all revised prices for targeted drugs were implemented. For example, some hemophilia groups claimed that treatment would be discontinued if prices were not increased, leading some states to exclude blood clotting factors from the revisions (OIG, 2001).

[15]Recall that the consumers should be price insensitive across bioequivalent generics produced by different manufacturers. Thus shifts from the utilization of one manufacturer's version to another's version reflect changes in the purchasing patterns of pharmacies.

3.4 Data Sources

3.4.1 Data

The main source of data for this paper is the Medicaid State Drug Utilization Data published by CMS.[16] The data series tracks Medicaid spending and utilization by NDC for each state and quarter from 1991 through the present.[17] NDCs only appear in the data if they have non-zero Medicaid utilization in the state and quarter. We aggregate the quarterly data to annual totals. We focus primarily on two variables: the number of prescriptions and the total reimbursement. From this we construct reimbursement per prescription which is simply total reimbursement divided by the number of prescriptions. Given that Medicaid reimbursement is largely determined by the AWP, reimbursement per prescription and AWP should be strongly correlated. The data set also includes information about NDC characteristics such as the drug name, FDA approval date, whether it is a generic or brand name drug, and the unit type (e.g. capsule, tablet, milliliter).

We obtain the list of the approximately 400 NDCs that were targeted by the DOJ from a Program Memorandum issued by the Department of Health and Human Services (HHS, 2000).[18] This document contains the actual average wholesale price found in the DOJ audit for each NDC. We use the eleven-digit NDCs reported in this Memorandum to identify the targeted NDCs in the State Drug Utilization Data.

[16]State Drug Utilization Data is available from the CMS website:
https://www.cms.gov/MedicaidDrugRebateProgram/SDUD/list.asp

[17]The quarterly data is highly noisy. This variation may reflect when claims are processed rather than actual patterns in drug use.

[18]This memorandum was issued to notify the Medicare program of the alternative source of average wholesale data that was uncovered by the DOJ and NAMFCU on behalf of Medicaid.

We find that 379 of the 411 NDCs had non-zero Medicaid utilization, the year before the intervention.

In this study, we are interested in comparing changes in purchasing patterns for the targeted NDCs and their competitors whose prices were not revised by the DOJ. This requires identifying the set of competitor drugs. In choosing which drugs to stock and dispense, pharmacies may substitute between one generic and another (or a generic and an off-patent brand name drug)[19] if they are bioequivalent. Thus each NDC competes with all bioequivalent NDCs for pharmacy market share. NDCs are bioequivalent if all of the following are true: the NDCs share the same active ingredient name, strength, and therapeutic equivalence rating as reported in the FDA publication, "Approved Drug Products with Therapeutic Equivalence Evaluations" (also known as the Orange Book).

As a measure of bioequivalence, we define the market for each targeted drug as the set of NDCs that share the same active ingredient name. Thus, the analysis sample includes the approximately 400 NDCs that received revised AWPs and all other NDCs (generics and off-patent brands) that have the same active ingredient names as those in the revised group. We use the drug name variable to extract these NDCs from the State Drug Utilization Data.[20] For generic drugs, the drug name is the same as the active ingredient name. For off-patent brand name drugs

[19]This sometimes requires permission from the prescribing doctor, but is at at the discretion of the pharmacy in states with mandatory generic substitution laws.

[20]This process is aided by linking the utilization data by NDC code to CMS's Drug Product File in order to determine the full name of each drug product. In the full US sample, the Drug Product File can be linked to about 55% of the NDCs in the utilization data. For NDCs that are not matched with a full drug name, we use the abbreviated drug name that appears in the utilization data.

(both active and discontinued), we use the Orange Book to obtain the list of brand names that have the same active ingredient name as the targeted NDCs. Using this market definition, there are 379 targeted NDCs and 2,839 competitor NDCs with positive Medicaid utilization in 1999. While this market definition is conservative in that it does not exclude any potential competitors of the targeted drugs,[21] it almost certainly overstates the true choice set that pharmacies face when choosing which versions of generic drugs to stock.

3.4.2 Sample Restrictions

We restrict our analysis to the period from 1994-2004, since changes in the composition of drugs over time make data further away from the intervention year less comparable. Our initial sample is constructed at the state-by-NDC-by-year and includes 795,694 observations. We make three other sample restrictions. First, we exclude five states from the analysis: Arizona, Alabama, Ohio, Tennessee, and Texas. Arizona does not report drug utilization data to CMS because it does not participate in the Medicaid Drug Rebate Program. The other four states do not use AWP as their primary reimbursement methodology (OIG, 2001). This reduces the sample by 66,190 observations. Second, we drop NDCs for hemophilia drugs (Anti-Inhibitor Coagulant Complex, Factor VIII, Factor IX). Hemophilia patient groups opposed the DOJ recommended prices leading some states to implement alternative prices for these drugs (OIG, 2001). These three biological drugs are

[21] Moreover, it allows for the possibility that there can be substitution across different formulations of a drug (e.g. liquid version for tablet) with the permission from the prescribing doctor.

unusually expensive relative to the other drugs targeted by the DOJ. They have the highest mean reimbursement per prescription among drugs in the sample, ranging from $10,493 to $16,369 in 1999. Winrho SDF, which was next most expensive drug in the sample, has a mean reimbursement per prescription of $2,080 which is substantially lower than the cost of the hemophilia drugs. Removing these drugs from the sample reduces total reimbursement by 15.59% and total prescriptions by 0.04%. Finally we exclude 5 observations that are extreme outliers in reimbursement per prescription, which are likely reporting errors.[22] The final state-by-NDC-by-year sample includes 723,991 observations. Since the intervention occurred at the NDC level, we aggregate the data across states to form nationwide totals for each NDC. There are 32,203 NDC-by-Year observations in the final analysis sample.

3.5 Empirical Evidence of Reverse Price Competition

3.5.1 Descriptive Statistics and Graphical Evidence

In Table 3.1, descriptive statistics are reported separately for NDCs that were targeted in the DOJ investigation and for their bioequivalent competitors. Means of variables are presented for units aggregated at the NDC level, except for the drug group market share variable, which is aggregated at the "drug group" level (the class of drugs that share the same drug name). The means are shown for 1999 and the change between 1999 and 2002. Columns 1 and 2 reveal that the DOJ-targeted

[22]We remove the following outlier observations because they differ substantially from the same NDCs in other states and years: NDC=00074115170, ME, 1996; NDC=59075065220, WA, 2003; NDC=39769001101, IL, 1991; NDC=49669162301, AR, 2002; NDC=00026064920, VA, 1999.

NDCs were on average about 3 times more expensive than other bioequivalent NDCs. The targeted NDCs were also Medicaid market leaders with an average market share that was more than 5 times as large as their competitors' market shares. However, since there were fewer targeted NDCs, they made up a smaller share of the overall market. Targeted drugs were also much more likely to have a liquid formulation than competitor drugs, which may explain some of the initial price differential.

The data show a decline in both mean Medicaid reimbursement per prescription and the number of prescriptions dispensed for targeted NDCs from 1999 to 2002, coinciding with the timing of the DOJ intervention. In the same time period, reimbursements per prescription and sales increased for competitor NDCs. This provides suggestive evidence that pharmacies may have shifted away from purchasing targeted drugs following the intervention. We also estimate the probability that NDCs were discontinued or introduced between 1999 and 2002.[23] As a comparison, we compute these probabilities between 1995 and 1998. Manufacturers may have strategically discontinued products that were targeted by the DOJ and then re-introduced similar products with new NDC codes, which would then be classified in this analysis as "competitor NDCs". Targeted NDCs had a substantially higher discontinuation rate after the DOJ intervention compared with the discontinuation rate in the pre-period (6.1% versus 0.4%). In contrast, the discontinuation rate for competitor NDCs did not change substantially over the two time periods, though it was higher than for targeted NDCs (perhaps due to the fact that these products

[23]The discontinuation probability is measured as the probability that an NDC has zero sales in 2002 (and all subsequent years) conditional on having positive sales in 1999. Similarly, the probability of NDC introduction from 1999-2002 is the probability that an NDC has positive sales in 2002 conditional on having zero sales in 1999 (and all earlier years).

had lower market share and there is more churning among smaller products). Interestingly, the rate of introduction of new competitor NDCs increased from 39% between 1995-1998 to 56% between 1999-2002 for manufacturers who had some of their NDCs in the same drug class targeted by the DOJ (Column 3), while the introduction rate was stable across time for all other competitor NDCs produced by non-targeted manufacturers (Column 4). This evidence is consistent with strategic product introduction and discontinuation by manufacturers.

We further examine whether the 1999-2002 change in reimbursement and prescriptions varied across competitor drugs depending on whether or not their manufacturer was targeted by the DOJ for other NDCs within the same drug class. We might expect that competitor NDCs produced by targeted manufacturers would respond differently to the intervention for a few reasons. First, because these manufacturers were under increased scrutiny by the DOJ, they may have halted price growth or even lowered prices on non-targeted products in attempt to avoid further investigation. Second, targeted manufacturers may have been able to more readily switch marketing and production efforts between their targeted and non-targeted products within a drug class, leading to a large increase in non-targeted "competitor" products. However, this effect would be mitigated if pharmacies were reluctant to purchase non-targeted products from the manufacturers who were under the most scrutiny.

Columns 3 and 4 report means for competitor NDCs produced by targeted and non-targeted manufacturers. Competitor NDCs produced by targeted firms are very similar to targeted NDCs (Column 1) in terms of observable characteristics,

such as baseline reimbursement per prescription and unit type. Moreover, similar to targeted NDCs, competitor NDCs of targeted firms saw a small decline in their average reimbursement following the intervention. In contrast, reimbursement per prescription increased by 25 percent for competitor NDCs of non-targeted firms. NDCs from non-targeted firms also experienced substantially larger growth in the average number of prescriptions purchased relative to competitor NDCs from targeted firms (a 56% versus 0.5% increase). While the means presented in this table are consistent with the hypothesized effects of the DOJ intervention, the changes in outcomes from 1999-2002 may also reflect differences in trends across targeted and competitor NDCs.

Figure 3.1 documents how reimbursement per prescription, prescription purchases, and market share evolved from 1991 through 2004 for targeted and competitor NDCs. Prior to the intervention, mean reimbursement per prescription had been rising rapidly since 1991 for both sets of drugs. For targeted drugs, reimbursement per prescription flattened and then declined sharply after the intervention in 2000. The leveling off before 2000 may reflect an early response to the intervention, given that the DOJ's planned policy was outlined to Medicaid State Pharmacy Directors at a national conference in the summer of 1999.[24] The drop in reimbursement per prescription was not a one-time change, but continued to decline until 2003. This is consistent with more and more states adopting the revisions or adopting them more stringently throughout the post-intervention period. In contrast, there was no corresponding break in the reimbursement trend for the competitor NDCs with

[24]Office of the Attorney General, Medicaid Fraud Control Unit, State of New York, 2000.

average reimbursement continuing to increase through 2004. This is consistent with there having been a real impact of the DOJ recommendations on actual Medicaid reimbursement trends– reducing the price of targeted drugs relative to competitors' prices.

In panel B, trends for the quantity of prescriptions purchased mimic the reimbursement trends. For targeted and competitor NDCs there are trend reversals following the intervention: prescriptions for targeted NDCs declined and increased for competitor NDCs. Moreover, the overall market share of targeted drugs declined after the intervention (Panel C). This is strong suggestive evidence of a substitution effect. For competitor NDCs, the reversal in trend for prescriptions (and targeted market share) occurred one year prior to the intervention. We examine this further in Figure 3.2 by separately plotting the trends in prescriptions for competitor NDCs manufactured by targeted firms and non-targeted firms. The figure shows a noticeable uptick in prescriptions in 1999 for competitor NDCs manufactured by targeted firms. At the same time, reimbursement per prescription had evolved smoothly in the years right before the intervention. One possible explanation for this sudden jump in prescriptions purchased is that targeted firms may have anticipated the intervention and tried to circumvent a loss in market share by shifting sales to non-targeted products within each drug class. If this uptick represents an anticipatory response by targeted firms, then measuring the change in prescriptions using 1999 as the base year (as in Table 3.1) understates the effect of the intervention for competitor drugs.

3.5.2 Regression Results

A. Reimbursement per Prescription

To estimate the magnitude of the changes in reimbursement per prescription and prescriptions purchased, we turn to regression analysis. We begin by documenting the impact of the DOJ intervention on average reimbursement per prescription. If DOJ prices are incorporated into state reimbursement policies, we would expect to observe a reduction in the actual Medicaid reimbursement paid per prescription for the targeted NDCs after May 2000. The differential impact of the intervention on reimbursement per prescription for targeted NDCs relative to competitor NDCs is a necessary criteria for using the intervention to identify the impact of price on pharmacy purchasing decisions.

We first estimate a non-parametric model to test whether there is a structural break in the targeted NDC trend relative to the competitor NDC trend around the time of the intervention in 2000:

$$log(Y_{jt}) = \gamma_t + \mu_j + \sum_{t=1994}^{2004} \beta_t(Target_j \cdot \gamma_t) + \epsilon_{jt} \tag{3.2}$$

The dependent variable is the log of reimbursement per prescription for NDC j in year t. We estimate proportional changes in reimbursement per prescription to allow for comparisons across drugs with widely different initial reimbursement levels. $Target_j$ is an indicator variable which equals one for targeted NDCs and zero for competitor NDCs. We control for permanent differences across NDCs and common year effects with NDC μ fixed effects and year γ fixed effects, respectively.

Standard errors are clustered at the drug group level (again, the group of NDCs that share the same drug name) to allow for correlation across NDCs within drug groups and serial correlation over time.

The main coefficients of interest are the full set of interactions of year dummies with the Target indicator. Each β_t gives the change in the targeted-competitor reimbursement difference between year t and the reference year of 1999. These coefficients are reported in Column 1 of Table 3.2. Targeted and competitor NDCs appear to have had the same reimbursement trends before the intervention, as reflected in the statistically insignificant coefficients prior to 2000. However, after states adopted DOJ prices in 2000, there was a very large and statistically significant relative decline in reimbursement per prescription for targeted drugs. This differential continued to widen until 2003 and then leveled off. Between 2000 and 2004, reimbursement per prescription fell by 26% for targeted drugs relative to the 4 year change between 1994 and 1998 ($exp([\beta_{04} - \beta_{00}] - [\beta_{98} - \beta_{94}]) - 1$).

Based on the results from the non-parametric specification, we next estimate a more parsimonious difference-in-difference model which imposes a trend break in 2000. This model allows us to estimate an average intervention effect instead of making point-by-point comparisons (as above) which may be sensitive to the points selected. We allow targeted and competitor NDCs to have separate linear trends. Moreover we allow for both level and slope shifts after 2000, as motivated by the graphical evidence in Figure 3.1 which showed that the drop in the reimbursement per prescription was not a one-time change but continued to decline over time. The

estimating equation is:

$$\begin{aligned} log(Y_{jt}) = \gamma_t + \mu_j + \alpha_1 Target_j \cdot Post_t \\ + \alpha_2 Target_j \cdot t + \alpha_3 Target_j \cdot Post_t \cdot (t - 2000) + \epsilon_{jt} \end{aligned} \tag{3.3}$$

$Post_t$ is an indicator that equals 1 in 2000-2004 and zero in 1994-1999. t is a linear time trend and $(t - 2000)$ equals 1 in 2001, 2 in 2002 and so forth. Standard errors are again clustered at the drug group level. The intercept shift for targeted NDCs relative to competitor NDCs is measured by α_1 and the differential slope shift by α_3. The key identifying assumption is that reimbursement per prescription for targeted and competitor NDCs would have continued along the same trends in the absence of the intervention. Combining these coefficients as $\alpha_1 + 4 * \alpha_3$ gives the effect of the intervention on targeted NDCs through 2004. The results from estimating Equation 3.3 are reported in Column 2 of Table 3.2. There is a statistically significant differential intercept and slope shift for targeted NDCs after 2000. The intervention appears to have reduced average reimbursement per prescription by 44% through 2004 ($exp(\alpha_1 + 4 * \alpha_3) - 1$). This estimate is larger than the non-parametric estimate because the point-by-point comparison did not account for an intercept shift.

In column 3, we examine the robustness of this result to adding linear trends that vary by drug group δ x Target cells. Given the limited statistical power, we do not use NDC-specific time trends in this analysis – which would be the most flexible specification. We continue to constrain the slope and intercept shifts to be the same

for all targeted NDCs and for all competitor NDCs. This model, which adds flexible time trends to Equation 3.3, is estimated as follows:

$$\begin{aligned} log(Y_{jt}) = \gamma_t + \mu_j + Target_j \cdot \delta_g \cdot t + \omega_1 Target_j \cdot Post_t \\ + \omega_2 Target_j \cdot Post_t \cdot (t - 2000) + \epsilon_{jt} \end{aligned} \tag{3.4}$$

The results are highly robust to the more flexible specification. The results suggest that reimbursement per prescription declined by 50% from 2000 to 2004 for targeted NDCs relative to competitor NDCs ($exp(\omega_1 + 4 * \omega_2) - 1$). Next, as a sensitivity test, we exclude competitor NDCs whose manufacturers were targeted by the DOJ for other NDCs within the same drug class – thus comparing targeted NDCs with competitor NDCs whose firms were not targeted for other drug products. As discussed earlier, these competitor NDCs may respond differently to the intervention given the likelihood that they are under heightened scrutiny and also because they can shift market share between their targeted drugs and their own non-targeted bioequivalent products. The results in Column 4 show that the intercept shift and slope shift are slightly larger after excluding these NDCs. The total effect of the intervention on reimbursement per prescription through 2004 is 54% for targeted NDCs, which is only slightly larger than the previous estimate using the full sample.

It should be noted that the results presented in this section are lower bound estimates of the DOJ's impact on reimbursement per prescription for two reasons. First, the estimates represent aggregate effects. Since not every state adopted DOJ prices or implemented the full set of recommended changes, the aggregate effect

is smaller than the impact on states that were fully compliant with DOJ recommendations. For example, in Columns 5 and 6, we compare the time pattern of reimbursement per prescription for the 30 states that reported in an OIG survey (2001) that they had incorporated some or all of the recommended AWP changes as of January-March 2001 (Column 5) with all other states (Column 6). During the first few years after the intervention, the differential decline in reimbursement for targeted NDCs was much larger for early adopter states. Over time, the decline for the two groups of states converged, as more states adopted the recommendations.

The second reason why these estimates represent a lower bound is that we can only observe prices for drug products with positive sales. If the products with the largest price declines were more likely to be discontinued by manufacturers or generate zero sales, our estimate would understate the impact of the intervention on reimbursement per prescription since the largest price reductions are selected out of the sample.

B. Prescriptions Purchased

Given the substantial differential impact of the DOJ intervention on reimbursement per prescription for targeted NDCs, we next examine how pharmacy purchasing decisions responded to this plausibly exogenous price shock. There are three main mechanisms through which the intervention could have caused changes in the quantity and composition of generic drugs used by Medicaid beneficiaries. First, pharmacies may have reduced their purchasing of targeted NDCs (whose relative price declined) and substituted towards competitor NDCs (whose relative price increased)–thereby

lowering the pharmacy market share of targeted drugs. Second, pharmacies may have responded to the profit loss from lower reimbursements by reducing Medicaid beneficiaries' access to the drug groups included in the DOJ investigation. This would cause a decline in the total prescriptions dispensed for these drug groups. Finally, manufacturers may have strategically discontinued products that were targeted by the DOJ and re-introduced these products (with minor modifications) as new NDC codes in attempt to undo the effects of the intervention. If this type of product exit and entry occurred, we would observe a decline in targeted market share.

In this preliminary analysis, we do not attempt to separately identify the relative contribution of each of these mechanisms to the change in prescription quantities. Instead, we use measures that summarize the total intervention effect: market share of targeted drugs and total prescriptions purchased in the drug group. In order to capture changes to market share that occur through the introduction or discontinuation of NDCs, we aggregate the data to drug group-by-year cells. Again we begin by estimating a non-parametric model to identify trend breaks in the time series:

$$MKTSHARE_{gt}^{T} = \varphi_g + \sum_{t=1994}^{2004} (\beta_t \cdot \gamma_t) + \epsilon_{gt} \tag{3.5}$$

The dependent variable is the market share for targeted drugs, computed as the number of prescriptions of targeted NDCs in drug group g in year t divided by the total number of prescriptions in drug group g in year t. Observations with zero

targeted market share are included. However we exclude any drug group-by-year observations for years in which there are no sales for both targeted and competitor NDCs in the drug group. This leaves 480 drug group-by-year observations. We control for drug group φ fixed effects and cluster standard errors at the drug group level. The market share results could be a misleading indicator of the change in targeted prescriptions purchased if the total market size is also changing as a result of the intervention. We verify later in this section that market size did not change abruptly at the time of the intervention.

Column 1 of Table 3.3 reports the full set of β_t coefficients which trace out the time pattern of market share for targeted drugs from 1994-2004 relative to market share in 1999. The negative coefficients before and after the reference year reflect an increasing trend in targeted market share during the pre-period and a decreasing trend following the intervention. Although the change between 1999 and 2000 is not statistically significant, the negative coefficients in 1998 and 2000 suggest that the trend reversal may have begun in 2000 as predicted. The decline in targeted market share accelerated through 2004. A joint significance test rejects that all of the negative coefficients following the intervention are equal to zero at the 10% level. Relative to the 4-year change from 1994 to 1998, the intervention led to a statistically significant decline in targeted market share of 16.1 percentage points from 2000 and 2004 ($[(\beta_{04} - \beta_{00}) - (\beta_{98} - \beta_{94})]$=-0.161 with a standard error of 0.058). This suggests that pharmacies substituted towards the competitor products after the intervention.

Next, we estimate to a linear trend break model which is analogous to Equa-

tions 3 and 4:

$$MKTSHARE_{gt}^{T} = \alpha_0 + \alpha_1 Post_t + \alpha_2 t + \alpha_3 Post_t \cdot (t - 2000) + \epsilon_{gt} \tag{3.6}$$

The results in Column 2 of Table 3.3 show that there was a statistically significant slope change in the trend for targeted market share in 2000 and a statistically insignificant intercept shift. The market share for targeted drugs fell by 18.4 percentage points by 2004 ($\alpha_1 + 4 * \alpha_3$), a 45% decline relative to the baseline mean of 40.9% in 1999. This is similar to the effect size found in the non-parametric specification. In Column 3 of Table 3.3, we add drug group specific linear time trends. The more flexible specification produces slightly larger effects. Treated market share declined by 18.8 percentage points by 2004 (a 46% reduction).

In Column 4, we estimate the same model using NDC-by-year level data in order to facilitate a comparison of the market share change with the price change at the NDC level (which was presented in Table 3.2). We create a balanced panel of NDCs by adding observations with zero market share to the sample. This allows us to capture the effects of NDC entry and exit on market share. We find that the policy driven price shock reduced the market share of targeted NDCs by on average 2.1 percentage points through 2004. Relative to the baseline mean of 4.8%, this amounts to a total reduction in targeted market share of 44%, which is almost identical to the estimates using drug group level data. Combining this estimate with our lower bound estimate of the price effect for targeted NDCs from Column 3 of Table 3.2, we compute the indirect least squares estimate of the price elasticity

of (pharmacy) demand as 0.88 (-0.44/-0.50). The key finding is that this price elasticity is positive, which is inconsistent with a standard competitive market in which the lowest-priced drugs capture the highest pharmacy market share.

In Columns 5 and 6, we also compare trends in targeted market share for early adopter states (as identified by the OIG survey) and all other states. Consistent with the time patterns for reimbursement per prescription presented in Table 3.2, targeted market share declined earlier and was greater in magnitude for early adopter states. In fact, for the states that hadn't adopted the revisions at the time of the survey in 2001, market share did not begin to decline until 2003. This mimics the results for reimbursement per prescription, which showed that reimbursement for targeted NDCs declined primarily in the later years of the post-intervention period for the states that hadn't initially adopted the revisions.

In Columns 7 and 8, we decompose the change in competitor drugs' market share by whether or not the NDCs were manufactured by targeted firms. We re-estimate the most flexible version of equation 3.5 using competitor market shares for targeted firms or non-targeted firms as the dependent variables. Consistent with the descriptive statistics and visual evidence discussed earlier, there was a smaller increase in market share for competitor drugs manufactured by targeted firms (3.0 percentage point increase through 2004 versus 15.9 percentage points). This may reflect pharmacies reluctance to substitute towards competitor NDCs from the firms that have a higher chance of being targeted in future DOJ investigations.

Finally, in Column 9 we estimate Equation 3.6 using the log of total prescriptions (i.e. the number of prescriptions for targeted plus competitor NDCs) in drug

group g in year t as the dependent variable. This provides a preliminary test of whether the intervention reduced Medicaid beneficiaries' access to DOJ-targeted drug groups. The results show that the slope and intercept shifts in 2000 were small and statistically insignificant. Thus it appears that the quantity of total prescriptions dispensed to Medicaid beneficiaries continued along its pre-intervention trend. This provides suggestive evidence that pharmacies did not respond to the policy driven price shock by reducing access to drugs for Medicaid beneficiaries. The finding that market size did not change following the intervention also makes interpreting the market share results as changes in prescriptions purchased more straightforward, as noted above.

3.6 Conclusion

The DOJ recommendations to reduce the AWP for approximately 400 drug products provides an opportunity to study the impact of Medicaid procurement on generic drug price competition. Current procurement rules provide perverse incentives for pharmacies to purchase and stock the highest-priced generic drugs. Consequently, manufacturers of generic drugs may compete for pharmacy market share by perversely bidding up the price on which Medicaid reimbursement is based.

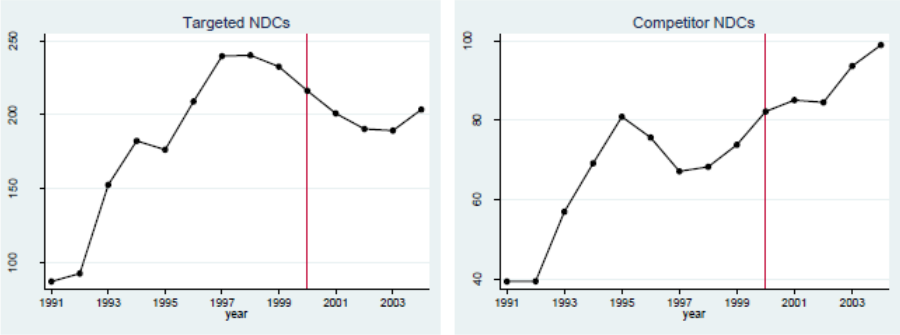
We find that Medicaid reimbursement per prescription declined substantially for targeted drugs following the DOJ intervention. In response to this negative price shock, pharmacies reduced their demand for targeted drugs and increased their demand for competitor drugs. Overall, the market share for targeted drugs

fell by about 45% through 2004. Thus, pharmacies appear to have substituted away from the drugs that experienced a price reduction towards those drugs whose prices were unaffected by the intervention. This demand response is inconsistent with a typical competitive market in which the lowest-priced drugs capture the highest market share.

In future work, we will extend this analysis in several ways. First, we will exploit heterogeneity in exposure to the intervention across states by collecting more detailed information about when each state adopted the DOJ revisions and the duration of their implementation. Second, we will estimate heterogenous effects across drug groups, with respect to their Medicaid market share, size, and other characteristics. Third, we will more directly estimate the price elasticity for pharmacy demand. Finally we will study how the intervention may have led to strategic product discontinuations and introductions by targeted manufacturers.

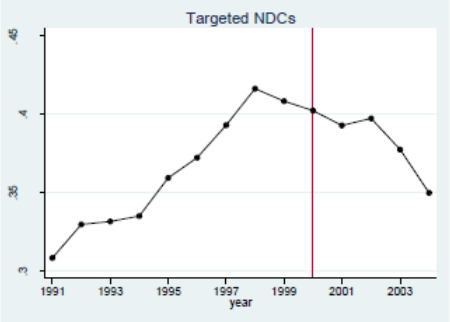
Figure 3.1: Trends in Reimbursement per Prescription and Prescriptions Purchased, 1991-2004

Panel A: Mean Reimbursement per Prescription for Targeted and Competitor NDCs



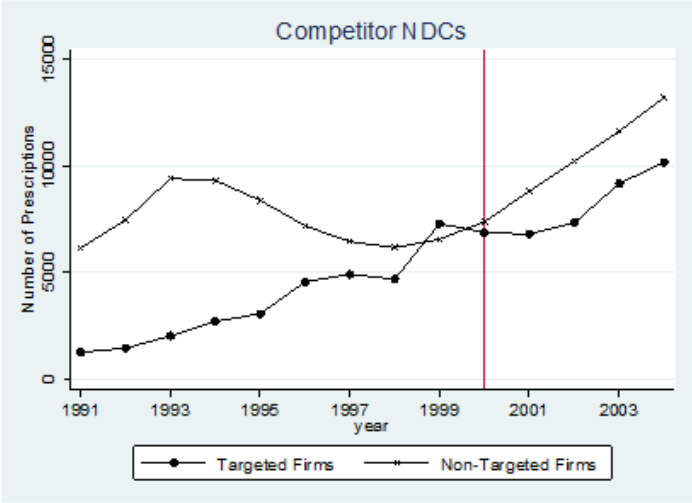
Panel B: Mean Number of Prescriptions for Targeted and Competitor NDCs

Panel C: Mean Targeted Market Share at the Drug Group Level



Notes: Figure 3.1 plots raw means of reimbursement per prescription and number of prescriptions using NDC-level data and mean targeted market share using drug group level data. Means for NDCs that were targeted by the DOJ and their competitor NDCs are plotted separately. The sample excludes data from the five states that do not use the AWP as their primary reimbursement methodology, hemophilia drugs, and outliers as described in the text in section 3.4.2.

Figure 3.2: Trends in Prescriptions for Competitor NDCs, Targeted Firms vs. Non Targeted Firms



Notes: Figure 3.2 plots raw means of number of prescriptions using NDC-level data for "competitor" NDCs. Means are plotted separately for competitor NDCs that were manufactured by firms whose other products in the drug class had been targeted by the DOJ with competitor NDCs that were manufactured by firms whose products were not at all targeted in the drug class. The sample excludes data from the five states that do not use the AWP as their primary reimbursement methodology, hemophilia drugs, and outliers as described in the text in section 3.4.2.

Table 3.1: Descriptive Statistics, 1999-2002

	Targeted NDCs	Competitor NDCs		
		All	Targeted Firms	Non-Targeted Firms
Sample:	(1)	(2)	(3)	(4)
Reimbursement per Prescription, 1999	232.61	73.86	166.67	50.49
Change in Reimbursement per Prescription, 1999-2002	-42.13	10.72	-16.66	12.73
Number of Prescriptions, 1999	7,994.12	6,699.91	7,288.44	6,551.67
Change in Number of Prescriptions, 1999-2002	-358.17	2,803.16	37.15	3,662.51
NDC Market Share, 1999	0.050	0.009	0.011	0.009
Change in NDC Market Share, 1999-2002	0.001	0.002	0.001	0.002
Drug Group Market Share, 1999*	0.409	0.635	0.188	0.566
Change in Drug Group Market Share, 1999-2002	-0.011	-0.018	0.004	-0.032
Probability of NDC Discontinuation, 1999-2002	0.061	0.276	0.112	0.317
(1995-1998 in brackets)	[0.004]	[0.235]	[0.105]	[0.261]
Probability of NDC Introduction, 1999-2002	0.818	0.370	0.562	0.342
(1995-1998 in brackets)	[0.782]	[0.363]	[0.392]	[0.357]
Fraction Generic, 1999	0.670	0.787	0.639	0.841
Year of FDA Approval, 1999	1991.3	1989.6	1990.7	1989.2
Fraction Unit Type, 1999:				
ML	0.737	0.466	0.763	0.358
Gram	0.024	0.046	0.029	0.053
Capsule	0.000	0.023	0.016	0.025
Tablet	0.000	0.374	0.110	0.470
Other	0.239	0.091	0.082	0.094
Total Prescriptions, 1999	2,869,890	18,980,848	4,154,410	14,826,438
Total Reimbursement, 1999	155,787,680	389,058,079	107,581,483	281,476,595
NDC x Year Observations, 1999	359	2,833	570	2,263

Notes: Variable means are reported using NDC-level data with the exception of drug group market share*, which uses drug group-level data. The probability of NDC discontinuation from 1999-2002 is the probability that an NDC has zero sales in 2002 (and all subsequent years) conditional on having positive sales in 1999. Similarly, the probability of NDC introduction from 1999-2002 is the probability that an NDC has positive sales in 2002 conditional on having zero sales in 1999 (and all earlier years). The sample excludes data from the five states that do not use the AWP as their primary reimbursement methodology, hemophilia drugs, and outliers as described in the text in section 3.4.2.

Table 3.2: Effects of DOJ Intervention on Reimbursement per Prescription, 1994-2004

Dependent variable:	Log (Reimbursement per Prescription)					
					Early Adopter States	All Other States
	(1)	(2)	(3)	(4)	(5)	(6)
Target*Post*(t-2000)		-0.071*	-0.092**	-0.110**		
		(0.041)	(0.038)	(0.047)		
Target*Post		-0.292***	-0.325***	-0.343***		
		(0.055)	(0.052)	(0.061)		
Target*Year1994	0.109				0.124	-0.018
	(0.139)				(0.140)	(0.148)
Target*Year1995	0.025				0.089	-0.039
	(0.120)				(0.128)	(0.148)
Target*Year1996	0.100				0.101	0.024
	(0.122)				(0.127)	(0.134)
Target*Year1997	0.127				0.128	0.081
	(0.107)				(0.105)	(0.125)
Target*Year1998	0.059				0.047	0.008
	(0.048)				(0.043)	(0.063)
Target*Year2000	-0.175***				-0.310***	-0.099**
	(0.039)				(0.060)	(0.046)
Target*Year2001	-0.421***				-0.660***	-0.215***
	(0.076)				(0.112)	(0.057)
Target*Year2002	-0.525***				-0.630***	-0.437***
	(0.087)				(0.111)	(0.090)
Target*Year2003	-0.543***				-0.583***	-0.484***
	(0.088)				(0.099)	(0.102)
Target*Year2004	-0.528***				-0.548***	-0.507***
	(0.090)				(0.100)	(0.108)
Year FE	Y	Y	Y	Y	Y	Y
NDC FE	Y	Y	Y	Y	Y	Y
Drug Group x Target x t	N	N	Y	Y	N	N
Excl. Targeted Firms	N	N	N	Y	N	N
F-test	11.390				7.800	10.420
	[0.000]				[0.000]	[0.000]
NDC x Year Obs	32,203	32,203	32,203	26,899	29,038	29,254

Notes: *** p<0.01, ** p<0.05, * p<0.1. Clustered standard errors at the drug group level. Column 1 reports results from estimating equation 3.2 using NDC-level data and Columns 2-4 report results from estimating equation 3.3 and 3.4. Column 2 also includes a linear time trend interacted with Target which is not reported. Column 3 adds Drug Group x Target specific linear trends, and Column 4 excludes observations from "competitor" NDCs manufactured by targeted firms. Column 5 reports estimates from equation 3.2 using data from states that were early adopters and Column 6 reports estimates for all other states. The F-test tests the joint significance of Target*Year2000 through Target*Year2004 (p-value in brackets). Sample restrictions are described in section 3.4.2.

Table 3.3: Effects of DOJ Intervention on Prescriptions Purchased, 1994-2004

Dependent variable:	Targeted Market Share						Competitor Market Share		Log (Total Prescriptions)
					Early Adopter States	All Other States	Targeted Firms	Non-Targeted Firms	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Post*(t-2000)		-0.036**	-0.037**	-0.004**			0.002	0.035**	-0.034
		(0.014)	(0.015)	(0.002)			(0.010)	(0.016)	(0.035)
Post		-0.04	-0.04	-0.005			0.022	0.019	-0.007
		(0.027)	(0.029)	(0.003)			(0.018)	(0.024)	(0.061)
Year1994	-0.110**				-0.117**	-0.095*			
	(0.050)				(0.051)	(0.050)			
Year1995	-0.080*				-0.084*	-0.071			
	(0.046)				(0.047)	(0.047)			
Year1996	-0.052				-0.056	-0.042			
	(0.041)				(0.041)	(0.042)			
Year1997	-0.014				-0.018	-0.015			
	(0.027)				(0.027)	(0.032)			
Year1998	-0.001				-0.007	0.01			
	(0.023)				(0.021)	(0.025)			
Year2000	-0.006				-0.021	0.008			
	(0.017)				(0.021)	(0.017)			
Year2001	-0.015				-0.039	0.021			
	(0.023)				(0.026)	(0.024)			
Year2002	-0.011				-0.047	0.033			
	(0.028)				(0.031)	(0.030)			
Year2003	-0.039				-0.062*	-0.011			
	(0.033)				(0.034)	(0.039)			
Year2004	-0.058				-0.088**	-0.025			
	(0.041)				(0.040)	(0.046)			
Drug Group FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Drug Group x t	N	N	Y	Y	N	N	Y	Y	Y
NDC-level regression	N	N	N	Y	N	N	N	N	N
F-test	2.230				1.460	1.980			
	[0.068]				[0.223]	[0.101]			
Mean Dependent Variable (1999)	0.409			0.048	0.423	0.384	0.141	0.450	
Observations	480	480	480	4,088	480	480	480	480	480

Notes: *** p<0.01, ** p<0.05, * p<0.1. Clustered standard errors at the drug group level. Column 1 reports results from estimating equation 3.5 using drug group-level data and Columns 2-4 and 7-9 report results from estimating equation 3.6. Column 2 is estimated at the drug group level and includes a linear time trend whose coefficient is not reported. Column 3 adds drug group specific linear trends. Column 4 reports results from estimating equation 3.6 at the NDC-level for a balanced panel of NDCs (i.e. observations with zero market share are included). Using NDC FE instead of drug group FE produces identical coefficients. Columns 5 and 6 report results from equation 3.5 using the subset of states that were early adopters and all other states. Columns 7 and 8 report results using the market share of competitor drugs (for targeted firms or non-targeted firms) as the dependent variable. Column 9 reports results for the outcome of total prescriptions at the drug group level. The F-test tests the joint significance of Year2000 through Year2004 (p-value in brackets). Sample restrictions are described in section 3.4.2.

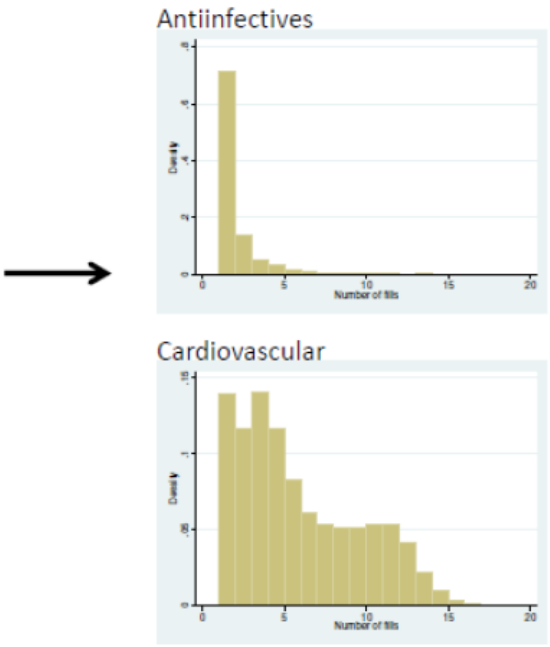
Appendix A

Appendix for Chapter 2

Figure A.1: Illustration of how Drugs are Classified into Chronic or Acute Categories

ID	Year	Drug Name	Drug Class	# of fills
1	2002	AMOXICILLIN	Antiinfectives	1
1	2002	CEFACLOR	Antiinfectives	2
1	2002	CEFACLOR	Antiinfectives	2
1	2002	ZOCOR	Cardiovascular	5
1	2002	ZOCOR	Cardiovascular	5
1	2002	ZOCOR	Cardiovascular	5
1	2002	ZOCOR	Cardiovascular	5
1	2002	ZOCOR	Cardiovascular	5
2	2002	LIPITOR	Cardiovascular	7
2	2002	LIPITOR	Cardiovascular	7
2	2002	LIPITOR	Cardiovascular	7
2	2002	LIPITOR	Cardiovascular	7
2	2002	LIPITOR	Cardiovascular	7
2	2002	LIPITOR	Cardiovascular	7
2	2002	LIPITOR	Cardiovascular	7
3	2003	ACETAMINOPHEN/CODEINE	Analgesics	1
3	2003	PERCOCET	Analgesics	1
3	2003	PROPOXY-N/APAP	Analgesics	1
3	2003	TERAZOL	Antiinfectives	1
3	2003	ZOCOR	Cardiovascular	6
3	2003	ZOCOR	Cardiovascular	6
3	2003	ZOCOR	Cardiovascular	6
3	2003	ZOCOR	Cardiovascular	6
3	2003	ZOCOR	Cardiovascular	6
3	2003	ZOCOR	Cardiovascular	6

MCBS 2002-2003 pooled, Ages 65+



Classification assignment rules in order of increasing stringency:

	Drug Class is…		
Classification Method	Acute if:	Chronic if:	Excluded from sample if:
>50% in drug group (Median rule)	if median<= 2	if median> 2	No exclusions
>55% in drug group	if 55th percentile<=2	if 45th percentile> 2	Neither statement is true
>60% in drug group	if 60th percentile<=2	if 40th percentile> 2	Neither statement is true
>65% in drug group	if 65th percentile<=2	if 35th percentile> 2	Neither statement is true
>70% in drug group	if 70th percentile<=2	if 30th percentile> 2	Neither statement is true
>75% in drug group	if 75th percentile<=2	if 25th percentile> 2	Neither statement is true

Notes: In the first step, I generate empirical distributions of the number of prescriptions filled in a year for drugs in each therapeutic class. These distributions are generated by counting the number of purchases of each drug for each person/year in the pre-announcement period. For example, person ID number 1 would contribute a 1 and a 2 to the distribution of fills for the Antiinfectives class and a 5 to the Cardiovascular class. In the second step, I assign a chronic or acute designation to each therapeutic class by using the rules listed in the above table applied to the empirical distribution of each class. Finally, I assign this classification to all drugs in the class for all years of the survey.

Table A.1: Classification of Therapeutic Categories

Therapeutic Drug Class	Proportion with <=2 fills	Mean # of fills (2002-2003)	Std Deviation of fills (2002-2003)	# of prescriptions filled (2002-2003)	Chronic?*
EENT preparations	0.60	3.15	3.07	15,127	N
Analgesics	0.64	3.12	3.39	12,461	N
Antiinfectives	0.86	1.70	1.65	8,789	N
Antihistamines	0.58	3.19	3.02	6,356	N
Antiinfectives, miscellaneous	0.82	1.91	2.00	4,906	N
Skin preparations	0.79	2.04	2.07	4,263	N
Cough and cold preparations	0.80	2.05	2.26	2,428	N
Muscle relaxants	0.67	2.85	3.00	1,627	N
Anesthetics	0.88	1.77	3.22	203	N
Psychotherapeutic drugs	0.75	2.00	1.41	16	N
Misc. medical supp., devices, & other	1.00	1.25	0.46	10	N
Cardiovascular	0.26	5.44	3.72	86,696	Y
Cardiac drugs	0.27	5.59	3.90	44,709	Y
Diuretics	0.27	5.38	3.74	35,892	Y
Autonomic drugs	0.21	5.88	3.85	29,957	Y
Gastrointestinal preparations	0.47	4.03	3.51	27,208	Y
Psychotherapeutic drugs	0.34	5.08	3.81	25,831	Y
Hypoglycemics	0.20	6.21	4.17	22,743	Y
Antiarthritics	0.45	4.08	3.40	19,167	Y
Blood	0.28	5.64	4.10	17,159	Y
Hormones	0.43	4.13	3.71	16,735	Y
Thyroid preparations	0.18	5.97	3.77	16,352	Y
Antiasthmatics	0.42	4.44	3.77	12,884	Y
Electrolyte, caloric, & fluid rep.	0.34	4.93	3.65	11,606	Y
CNS drugs	0.32	5.21	3.85	5,950	Y
Sedative and hypnotic drugs	0.42	4.59	3.75	4,197	Y
Vitamins, all others	0.36	4.54	3.45	3,672	Y
Antineoplastics	0.33	5.26	3.81	2,552	Y
Antiparkinson drugs	0.30	5.75	4.35	2,373	Y
Diagnostic	0.31	4.81	3.69	77	Y
Anti-obesity drugs	0.47	3.94	3.25	67	Y
Pre-natal vitamins	0.17	5.33	4.13	32	Y

Notes: All figures are from the pooled MCBS 2002-2003 for elderly ages 65+. * This classification of chronic and acute drugs is for the median classification rule.

Table A.2: Aggregate Announcement and Implementation Effects-Alternative Specifications

Dependent variable:	Total Prescriptions				Log (Total Prescriptions)			
Specification:	Linear with level shift	Linear with level and slope shift	Quadratic	Non-parametric	Linear with level shift	Linear with level and slope shift	Quadratic	Non-parametric
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Announce	-1.6064***	-3.5157***	-2.9121***		-0.0342	-0.0309	-0.0400	
	(0.621)	(0.834)	(0.682)		(0.030)	(0.037)	(0.031)	
Implement	0.9064	2.4147*	-3.5316**		0.0237	0.0211	0.0042	
	(1.115)	(1.364)	(1.657)		(0.050)	(0.062)	(0.068)	
Year 2002				0.8695**				0.025
				(0.444)				(0.023)
Year 2003				2.8029***				0.1246***
				(0.576)				(0.027)
Year 2004				2.4165***				0.1403***
				(0.639)				(0.031)
Year 2005				5.7221***				0.1992***
				(0.716)				(0.032)
Year 2006				9.2464***				0.3202***
				(0.757)				(0.032)
t	1.7788***	1.4017***	-0.1693		0.0616***	0.0623***	0.053	
	(0.240)	(0.288)	(0.726)		(0.011)	(0.014)	(0.033)	
t-squared			0.3903***				0.0017	
			(0.134)				(0.006)	
Years Since Announce*		1.9040***				-0.0033		
		(0.702)				(0.030)		
Announce + Years Since Announce		-1.6117***				-0.0342		
		(0.620)				(0.029)		
(Yr 2004- Yr 2003) - (Yr 2003- Yr 2002)				-2.3198***				-0.0838**
				(0.763)				(0.036)
(Yr 2005- Yr 2004) - (Yr 2003- Yr 2002)				1.3722*				-0.0406
				(0.783)				(0.034)
(Yr 2006- Yr 2005) - (Yr 2003- Yr 2002)				1.5909*				0.0215
				(0.856)				(0.036)

Notes: *** p<0.01, ** p<0.05, * p<0.1. Clustered standard errors at the person level. Regressions are weighted and include a full set of control variables. Medicaid beneficiaries are included. The bottom panel presents linear combinations of the coefficients and their standard errors. * The variable "Years Since Announce" is defined as the year minus 2003 in the announcement period, so that it takes on a value of 1 in 2004 and a 2 in 2005, and zero otherwise. The linear combination of the coefficients of Announce + Years Since Announce provides the estimate of the announcement effect in 2004. MCBS 2001-2006; Ages 66-74; N=20,072.

Table A.3: Aggregate Announcement and Implementation Effects-Negative Binomial (Marginal Effects)

Dependent Variable:	Total Prescriptions					
Model:	Negative Binomial			OLS		
	(1)	(2)	(3)	(4)	(5)	(6)
Announce		-1.8263***	-1.5640**		-2.1140***	-1.6064***
		(0.385)	(0.622)		(0.430)	(0.621)
Implement	2.5853***		0.4761	2.9943***		0.9064
	(0.714)		(1.120)	(0.724)		(1.115)
t	1.3165***	1.8860***	1.7878***	1.2970***	1.9667***	1.7788***
	(0.185)	(0.1485)	(0.257)	(0.177)	(0.157)	(0.240)
Observations	20,072	20,072	20,072	20,072	20,072	20,072

Notes: *** p<0.01, ** p<0.05, * p<0.1. Clustered standard errors at the person level. Regressions are weighted and include a full set of control variables. Medicaid beneficiaries are included. Columns 4-6 are identical to Table 2.2. MCBS 2001-2006; Ages 66-74.

Table A.4: Chronic and Acute Aggregate Announcement and Implementation Effects-Alternative Specifications

Dependent variable:	Total Prescriptions				Log (Total Prescriptions)			
Spec:	Linear with level shift	Linear with level and slope shift	Quadratic	Non-parametric DD	Linear with level shift	Linear with level and slope shift	Quadratic	Non-parametric DD
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Chronic*Announce	-1.5667***	-2.1789***	-2.0091***		-0.0543	0.0001	-0.0259	
	(0.509)	(0.667)	(0.549)		(0.035)	(0.045)	(0.038)	
Chronic*Implement	1.5100	1.9925*	0.0065		-0.0618	-0.1047	0.0348	
	(0.924)	(1.128)	(1.312)		(0.058)	(0.070)	(0.083)	
Announce	-0.1567	-0.2241	-0.2162		-0.0179	-0.0168	-0.0186	
	(0.159)	(0.204)	(0.172)		(0.026)	(0.033)	(0.028)	
Implement	0.7608***	0.8145**	0.5585		0.1314***	0.1305**	0.1291**	
	(0.264)	(0.319)	(0.374)		(0.042)	(0.051)	(0.061)	
Chronic	15.4795***	15.7209***	16.1812***	16.7414***	1.4447***	1.4232***	1.3996***	1.5010***
	(0.495)	(0.547)	(0.753)	(0.367)	(0.033)	(0.037)	(0.050)	(0.025)
Chronic*Year2002				0.6445*				0.0422
				(0.371)				(0.026)
Chronic*Year2003				1.8532***				0.1378***
				(0.475)				(0.031)
Chronic*Year2004				1.1176**				0.1437***
				(0.525)				(0.035)
Chronic*Year2005				2.6545***				0.1583***
				(0.578)				(0.037)
Chronic*Year2006				6.5321***				0.2310***
				(0.644)				(0.037)
t	-0.0023	-0.0157	-0.0914		-0.0133	-0.0131	-0.0143	
	(0.058)	(0.070)	(0.168)		(0.009)	(0.011)	(0.027)	
t-squared			0.0178				0.0002	
			(0.030)				(0.005)	
Years Since Announce*		0.0673				-0.0011		
		(0.157)				(0.025)		
Chronic*t	1.0473***	0.9267***	0.3881		0.0582***	0.0689***	0.1005***	
	(0.196)	(0.238)	(0.587)		(0.013)	(0.016)	(0.038)	
Chronic*t^2			0.1321				-0.0085	
			(0.106)				(0.007)	
Chronic*Time Since Announce*		0.6102				-0.0542		
		(0.558)				(0.035)		
Chr*Announce + Chr*Yrs Since Announce		-1.5687***				-0.0542		
		(0.5081)				(0.0348)		
(Chr*2004-Chr*2003) - (Chr*2003-Chr*2002)				-1.9444***				-0.0897**
				(0.6179)				(0.0430)
(Chr*2005-Chr*2004) - (Chr*2003-Chr*2002)				0.3282				-0.0809**
				(0.6299)				(0.0405)
(Chr*2006-Chr*2005) - (Chr*2003-Chr*2002)				2.6689***				-0.0229
				(0.7073)				(0.0432)

Notes: *** p<0.01, ** p<0.05, * p<0.1. Clustered standard errors at the person level. Regressions are weighted and include a full set of control variables. Medicaid beneficiaries are included. Year fixed effects are included in columns 4 and 8. The bottom panel presents linear combinations of the coefficients and their standard errors. * The variable "Years Since Announce" is defined as the year minus 2003 in the announcement period, so that it takes on a value of 1 in 2004 and a 2 in 2005, and zero otherwise. MCBS 2001-2006; Ages 66-74; N=40,144.

Table A.5: Chronic and Acute Aggregate Announcement and Implementation Effects-Negative Binomial (Marginal Effects)

Dependent Variable:	Total Prescriptions (>50% in group)		Total Prescriptions (>65% in group)		Total Prescriptions (>50% in group)		Total Prescriptions (>65% in group)	
Model:	Negative Binomial				OLS			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Chronic*Announce		-1.3902***		-1.3429***		-1.5667***		-1.4160***
		(0.510)		(0.450)		(0.509)		(0.444)
Chronic*Implement	3.5534***	1.5716*	2.9433***	1.0381	3.5449***	1.5100	3.2464***	1.4074*
	(0.625)	(0.953)	(0.536)	(0.822)	(0.602)	(0.924)	(0.534)	(0.807)
Announce		-0.5975		-0.16124		-0.1567		-0.0322
		(0.577)		(0.530)		(0.159)		(0.090)
Implement	3.7194***	2.8062***	2.4047***	2.1605**	0.9651***	0.7608***	0.3655***	0.3230**
	(0.698)	(1.095)	(0.602)	(1.008)	(0.163)	(0.264)	(0.091)	(0.151)
Chronic	16.8814***	16.7514***	13.9825***	13.7410***	16.2625***	15.4795***	12.9526***	12.2450***
	(0.846)	(0.893)	(0.744)	(0.805)	(0.474)	(0.495)	(0.404)	(0.418)
t	-0.3401**	-0.1600	-0.2168*	-0.1682	-0.0494	-0.0023	-0.0048	0.0049
	(0.151)	(0.209)	(0.126)	(0.190)	(0.041)	(0.058)	(0.023)	(0.033)
Chronic*t	0.6343***	1.0547***	0.7541***	1.1610***	0.5777***	1.0473***	0.6883***	1.1127***
	(0.152)	(0.209)	(0.133)	(0.183)	(0.144)	(0.196)	(0.124)	(0.168)
Observations	40,144	40,144	40,144	40,144	40,144	40,144	40,144	40,144

Notes: *** p<0.01, ** p<0.05, * p<0.1. Clustered standard errors at the person level. Regressions are weighted and include a full set of control variables. Medicaid beneficiaries are included. The classification method used is the median assignment rule (more than 50% of drugs in the therapeutic class are either chronic or acute). Marginal effects for interaction terms in the negative binomial model are computed as the double difference as described in Section 6.3. Columns 3 and 4 are identical to Table 2.5. MCBS 2001-2006; Ages 66-74.

Appendix B

Appendix for Chapter 3

Table B.1: Descriptive Statistics for Generic Drug Groups Targeted by the DOJ, 1999

Generic Drug Group	Targeted by DOJ?	Mean Reimbursement per Prescription	Total Medicaid Reimbursement Amount ($1000s)	Total Number of Prescriptions	Total Reimbursement Amount for Drug Group ($1000s)
ALBUTEROL SULFATE	N	17.4	87,179	5,560,183	146,139
ALBUTEROL SULFATE	Y	32.2	58,960	1,976,543	146,139
FACTOR VIII	N	13036.1	951	40	97,551
FACTOR VIII	Y	10493.2	96,600	9,697	97,551
LORAZEPAM	N	36.0	79,891	2,468,956	81,076
LORAZEPAM	Y	76.9	1,185	26,785	81,076
FUROSEMIDE	N	8.4	33,289	6,188,582	33,478
FUROSEMIDE	Y	19.8	190	8,977	33,478
SODIUM CHLORIDE	N	35.0	17,917	650,140	27,337
SODIUM CHLORIDE	Y	72.3	9,421	141,677	27,337
CROMOLYN SODIUM	N	53.0	14,172	227,836	25,917
CROMOLYN SODIUM	Y	55.2	11,746	225,219	25,917
ZOFRAN	N	416.9	20,641	45,407	24,882
ZOFRAN	Y	331.8	4,240	30,931	24,882
IMMUNE GLOBULIN	N	1323.3	2,335	4,179	24,284
IMMUNE GLOBULIN	Y	1981.5	21,949	10,743	24,284
FACTOR IX	Y	10159.7	18,621	1,744	18,621
TOBRAMYCIN SULFATE	N	189.2	17,132	172,124	18,081
TOBRAMYCIN SULFATE	Y	261.7	949	3,707	18,081
ACYCLOVIR SODIUM	N	75.4	16,681	366,494	16,957
ACYCLOVIR SODIUM	Y	734.3	277	1,161	16,957
LUPRON	N	1209.6	7,153	9,819	15,871
LUPRON	Y	1160.1	8,718	12,380	15,871
CLINDAMYCIN PHOSPHATE	N	53.5	13,040	324,187	13,462
CLINDAMYCIN PHOSPHATE	Y	150.7	422	3,337	13,462
VANCOMYCIN HYDROCHLORIDE	N	184.4	3,474	16,676	13,365
VANCOMYCIN HYDROCHLORIDE	Y	194.3	9,892	45,095	13,365
CIMETIDINE HYDROCHLORIDE	N	21.2	11,690	664,202	11,706
CIMETIDINE HYDROCHLORIDE	Y	45.6	16	521	11,706
DIAZEPAM	N	24.2	11,119	1,083,068	11,141
DIAZEPAM	Y	15.2	22	1,331	11,141
CALCITRIOL	N	97.4	8,906	136,094	9,263
CALCITRIOL	Y	290.1	358	24,056	9,263
METHOTREXATE SODIUM	N	38.2	9,014	173,751	9,261
METHOTREXATE SODIUM	Y	17.4	247	20,216	9,261
KYTRIL	N	606.3	4,206	7,812	6,780
KYTRIL	Y	786.5	2,575	12,653	6,780
DEXTROSE	N	57.6	483	9,476	6,246
DEXTROSE	Y	70.2	5,764	105,501	6,246
AMPHOTERICIN B	N	839.2	5,253	5,400	6,027
AMPHOTERICIN B	Y	120.6	774	2,634	6,027
HEPARIN LOCK FLUSH	N	39.7	3,665	139,014	4,506
HEPARIN LOCK FLUSH	Y	15.1	841	62,470	4,506
METAPROTERENOL SULFATE	N	26.7	3,365	134,173	4,181
METAPROTERENOL SULFATE	Y	55.1	816	12,522	4,181
GENTAMICIN SULFATE	N	17.6	3,639	340,752	3,737
GENTAMICIN SULFATE	Y	28.0	98	4,477	3,737
WINRHO SDF	Y	2080.4	3,696	3,162	3,696
LEUCOVORIN CALCIUM	N	166.3	2,536	21,744	3,256
LEUCOVORIN CALCIUM	Y	322.1	720	3,039	3,256
ANZEMET/DOLASETRON MESYLATE	N	658.8	779	1,349	3,007
ANZEMET/DOLASETRON MESYLATE	Y	341.4	2,227	49,613	3,007
CYCLOPHOSPHAMIDE	N	123.8	1,348	8,444	2,690
CYCLOPHOSPHAMIDE	Y	930.0	1,342	4,092	2,690
DOXORUBICIN HYDROCHLORIDE	N	703.1	721	703	2,461
DOXORUBICIN HYDROCHLORIDE	Y	356.0	1,740	4,641	2,461

Table B.2: Descriptive Statistics for Generic Drug Groups Targeted by the DOJ, 1999 (Cont'd)

Generic Drug Group	Targeted by DOJ?	Mean Reimbursement per Prescription	Total Medicaid Reimbursement Amount ($1000s)	Total Number of Prescriptions	Total Reimbursement Amount for Drug Group ($1000s)
ACETYLCYSTEINE	N	72.5	874	12,852	2,081
ACETYLCYSTEINE	Y	76.9	1,207	15,538	2,081
ETOPOSIDE	N	617.7	1,725	2,488	2,074
ETOPOSIDE	Y	326.5	349	1,064	2,074
DEXAMETHASONE	N	14.6	2,025	144,932	2,063
DEXAMETHASONE	Y	15.0	38	2,731	2,063
IRON DEXTRAN	N	127.4	317	2,614	1,898
IRON DEXTRAN	Y	165.1	1,581	11,768	1,898
CISPLATIN	N	614.0	1,160	1,976	1,160
CISPLATIN	Y	720.8	1	1	1,160
AMIKACIN SULFATE	N	329.2	288	739	1,146
AMIKACIN SULFATE	Y	389.1	859	3,100	1,146
FLUOROURACIL	N	61.5	1,090	15,166	1,139
FLUOROURACIL	Y	27.5	50	3,338	1,139
METHYLPREDNISOLONE SODIUM SUCCINATE	N	32.6	640	23,669	1,064
METHYLPREDNISOLONE SODIUM SUCCINATE	Y	51.8	424	9,749	1,064
TESTOSTERONE CYPIONATE	N	41.3	51	1,286	938
TESTOSTERONE CYPIONATE	Y	50.6	887	15,208	938
PENTAMIDINE ISETHIONATE	N	165.4	629	5,647	816
PENTAMIDINE ISETHIONATE	Y	203.4	188	1,099	816
ANTI-INHIBITOR COAGULANT COMPLEX	N	9135.9	62	12	743
ANTI-INHIBITOR COAGULANT COMPLEX	Y	16368.5	680	51	743
BLEOMYCIN SULFATE	Y	713.5	684	791	684
TESTOSTERONE ENANTHATE	N	57.0	438	6,268	452
TESTOSTERONE ENANTHATE	Y	27.8	14	503	452
VINCRISTINE SULFATE	N	77.8	48	644	150
VINCRISTINE SULFATE	Y	59.8	101	1,887	150
MITOMYCIN	N	430.8	103	163	148
MITOMYCIN	Y	535.0	45	93	148
HYDROCORTISONE SODIUM SUCCINATE	N	23.3	35	1,668	99
HYDROCORTISONE SODIUM SUCCINATE	Y	23.8	65	3,281	99
CYTARABINE	Y	95.7	76	1,188	76
VINBLASTINE SULFATE	N	135.6	12	171	48
VINBLASTINE SULFATE	Y	51.3	36	1,068	48

Notes: The sample excludes data from the five states that do not use the AWP as their primary reimbursement methodology and outliers as described in the text in section 3.4.2.

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