Abstract

Title of Dissertation: THE EFFECT OF MEDICAID DISEASE MANAGEMENT PROGRAMS ON MEDICAID EXPENDITURES

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Disease Management (DM) programs for Medicaid patients with chronic diseases have become very popular, with a majority of states having introduced some type of DM program in the last decade. These programs provide interventions designed to assist patients and their health care providers appropriately manage their chronic health condition(s) according to established clinical guidelines. Cost-containment has been a key justification for the creation of DM programs, despite mixed evidence that DM actually saves money for the Medicaid program or for society as a whole.

While most studies on the impact of DM focus on estimating the impact of a single DM program, Chapter 2 estimates the average, national impact of state Medicaid DM programs by linking a detailed survey of state Medicaid programs to the nationally representative Medical Panel Expenditure Survey. Difference-in-difference models are used to test the hypothesis that medical expenditures change after a DM program is implemented, exploiting variation in the timing at which state Medicaid programs implemented DM programs. DM coverage also varies within states over time due to variation in program eligibility by disease, insurance category,

and/or county of residence. Although the models estimate the effect of DM imprecisely, point estimates are stable across multiple specifications and indicate that DM programs for common chronic diseases may decrease total medical expenditures, potentially by 10 percent or more.

Chapter 3 evaluates one DM program in the state of Georgia using a proprietary data set. By exploiting a natural experiment that delayed the introduction of high-intensity services for several thousand high and moderate risk patients, the research identifies the causal impacts of the program's interventions on total Medicaid expenditures, categories of health care utilization, and other indicators. These patients are observationally similar to those who received interventions at the beginning of the program. For example, I find the interventions lowered health costs and hospital utilization, after controlling for unobservable individual characteristics. Health expenditures were lowered about 4.4 percent for patients with positive expenditures. Heterogeneous treatment effect analysis indicates that the savings were largest at the most expensive tail of the distribution. *JEL Classification Codes:* I12, I18, H51

THE EFFECT OF MEDICAID DISEASE MANAGEMENT PROGRAMS ON MEDICAID EXPENDITURES

by

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Dedication

For my wife, Dayna,
with whom I shared each step of this journey

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List of Abbreviations

ABD	Aged, blind, and disabled
AHRQ	Agency for Healthcare Research and Quality
ATE	Average treatment effect
ATET	Average treatment effect on the treated
CAD	Coronary artery disease
CHF	Congestive heart failure
CHIP	Children's Health Insurance Program
COPD	Chronic obstructive pulmonary disease
CDPS	Chronic Illness and Disability Payment System
CMS	Centers for Medicare & Medicaid Services
DM	Disease Management
DMO	Disease Management Organization
ED	Emergency department
FE	Fixed effect
GDP	Gross domestic product
GEE	Generalized estimating equation
GEC	Georgia Enhanced Care
GLM	Generalized linear model
MEPS	Medical Expenditure Panel Survey
MMC	Medicaid Managed Care
N	Number [of]
NLLS	Nonlinear least squares
OLS	Ordinary least squares
PCCM	Primary care case management

PCP	Primary care physician
PMPM	Per-member-per-month
PQI	Prevention Quality Indicators
SSI	Supplemental Security Income

Chapter 1: Introduction

Medicaid serves as America's major public health insurance program for low-income individuals and has become a dominant aspect of the United States health care system and government expenditure. In recent decades, federal and state governments have experienced dramatic growth in annual Medicaid spending. Concerned about the fiscal sustainability of the program, yet reluctant to either dramatically reduce the number of individuals eligible for benefits or to eliminate services, states have implemented a variety of reforms intended to improve the cost efficiency of the program. States have specifically looked for cost savings via the small fraction of individuals who account for a disproportionately large fraction of health care spending, particularly the chronically ill. Furthermore, a growing body of research indicates that significant numbers of individuals, especially those with chronic illnesses, receive inadequate health care. Inappropriate management of chronic conditions is linked to a number of expensive, undesirable outcomes, including more bouts of acute illness, avoidable hospitalizations, complications from co-morbidities and lengthy hospital inpatient stays.

Policy makers have increasingly turned to Disease Management (henceforth DM) to address both of these important concerns – cost containment and deficiencies in the quality of health care – in the Medicaid program. DM programs have the broad objective of assisting patients and their health care providers to appropriately manage their chronic health conditions according to established clinical guidelines. Proponents of DM argue that services provided by the program will assist in proper management of chronic health conditions such as proper medication use and preventive treatments. This could subsequently decrease hospitalization and other medical expenditures, ultimately providing policy makers with the "best of both worlds:" *higher* achievement of health quality for program enrollees at a *lower* cost.

Although cost-containment has been a key justification for the creation of DM programs, there is actually mixed evidence that DM actually saves money for the Medicaid program or for

society as a whole. Comprehensive literature reviews from the private insurance sector failed to find conclusive evidence that DM reduces net medical expenditures. (CBO 2004; Goetzel et al. 2005; Mattke, Seid, and Ma 2007) However, it is unlikely that the financial impact of DM in the private sector is comparable in the Medicaid setting, due to the fact that Medicaid recipients are demographically different than individuals insured in the private sector and have different health care utilization patterns. Given that Medicaid's DM programs have been evaluated in only a few cases, sometimes with questionable research designs, the financial effect of Medicaid DM programs remains an unanswered question.[1]

Despite limited evidence of cost savings, Medicaid DM programs for the chronically ill have become a popular component of state Medicaid programs throughout the United States over the last decade and a half. Figure 1 plots the number of states with a Medicaid Disease Management program, showing a significant increase in DM coverage in the late 1990s and early 2000s. At the end of 2008, 36 states with at least one DM program accounted for 82 percent of the total Medicaid population in the United States. DM program size can vary across states. Some programs have fewer than 1,000 people in the program, while more than 25 percent of Medicaid recipients may be "eligible" for services in other states (with perhaps 5 to 25 percent of eligible patients actively receiving DM services). More recently, the federal health reform law passed in 2010 implements or encourages the introduction of DM programs for selected segments of the U.S. health market.

This dissertation addresses the question, "How do Medicaid DM programs affect medical expenditure and utilization patterns for program participants?" In light of the policy debate and unresolved questions in the literature, I focus on the financial effects of DM. The following chapters empirically evaluate the effect of Medicaid DM programs on medical expenditures from two different perspectives. Chapter 2 estimates the effect of Medicaid chronic disease

[1] The existing literature on DM programs is discussed below in Section 2.2, Section 3.2, and elsewhere.

management programs using nationally representative survey data. This offers a new perspective on the research question, but has limitations in the absence of detailed data on program activities. Chapter 3 performs an evaluation of a single DM program: the Georgia Enhanced Care program, which was introduced in Georgia in the fall of 2005. Both research approaches benefit from the presence of individuals who do not receive DM that are similar to the groups who do receive DM, providing plausible counterfactuals for what would have occurred in the absence of the DM programs being studied.

The remainder of Chapter 1 provides further background information on Medicaid DM and summarizes the results of Chapter 2 and Chapter 3.

1.1 The policy environment: Medicaid and chronic disease

Medicaid serves as America's major public health insurance program for low-income individuals and has become a dominant aspect of the United States health care system and government expenditure. The policies of Medicaid are of substantial importance because it (i) serves as the major source of health care for a large fraction of American individuals and (ii) consumes a large fraction of the U.S. federal and state budgets. An estimated 50.3 million people received health insurance coverage from Medicaid in June 2010.[2] In 2009, Medicaid accounted for 373.9 billion dollars in expenditures, or about 15 percent of all health care expenditures in the United States and 2.6 percent of GDP.[3] Medicaid and the Children's Health Insurance Program (CHIP) are expected to expand dramatically in future years because of modifications to program enrollment criteria passed into law with the Patient Protection and Affordable Care Act of 2010. The Congressional Budget Office (2010) projects that Medicaid and CHIP enrollment will

[2] Children comprise 26.5 million (53 percent) of these enrollees. About 12.1 million (24 percent) were classified as aged or disabled. Enrollment estimates are from the Kaiser Family Foundation (2011b). Some 58.7 million individuals, roughly 20 percent of all Americans, were enrolled in Medicaid for some length of time during the federal fiscal year 2007. (KFF 2011a)

[3] Total national health expenditures were an estimated 2.49 trillion dollars in calendar year 2009 (Martin et al. 2011). Medicaid estimate excludes the Children's Health Insurance Program (CHIP).

expand by 16 million, relative to baseline projections, with an incremental cost of at least $80 billion per year after 2016.

In recent decades, federal and state governments[4] have experienced dramatic growth in annual Medicaid expenditure due to expansions to the eligible population, increases in the utilization of health care services, increases in the costs associated with the provision of health care, the introduction of new medical technologies and procedures, and other factors. (Gruber 2000; CMS 2005) As seen in Figure 2, the federal government's share of Medicaid expenditures has grown from a relatively small program to about 7.9 percent of total federal government outlays. Unlike Medicare, which provides health care for retirement-aged Americans and is completely federally funded, state governments spend a significant share of their budgets on Medicaid; Medicaid accounts for 21.1 percent of all state government expenditures, making it one of the two largest expenditure categories in state government budgets (tied with primary and secondary education).[5] Growth in the program has been seen as a major fiscal burden to many state governments, with expenditure growth unsustainably outpacing that of state revenue, inflation, and GDP. (e.g., Burgess 2004)

Reluctant to dramatically reduce the size of the population eligible for benefits or eliminate services, states have implemented a variety of reforms intended to increase the cost efficiency of the program. States have specifically looked for cost savings among a small fraction of individuals who account for a disproportionately large fraction of health care spending. Nationwide, the 3.6 percent most expensive enrollees accounted for roughly half of Medicaid expenditures in FY2001. (Sommers and Cohen 2006)

[4] Both federal and state governments jointly fund Medicaid, with the federal government paying approximately two-thirds of total Medicaid expenditures (excluding CHIP). (Martin et al. 2011) The federal government's share represents about 44 percent of all federal grants to state and local governments. Federal spending estimates and data for Figure 2 from the U.S. Office of Management and Budget (2011, Table 16.1).

[5] Estimates from NASBO (2010, pp. 4-5). Data in FY2009 show Medicaid is slightly smaller than the "primary and secondary education category." The report expects Medicaid to be the largest category in FY2010.

One categorical population within Medicaid that receives much focus is the aged, blind, and permanently disabled (ABD) population, who normally qualify for Medicaid in conjunction with Supplemental Security Income (SSI) benefits. As seen in Figure 3, expenditure on this population has grown dramatically since 1985, despite relatively modest growth in the number of enrollees. The ABD population currently comprises 22 percent of the enrollees, yet accounts for two-thirds of payments. (SSA 2011) This implies that Medicaid pays, on average, 6.4 times higher annual costs for ABD enrollees when compared to other categories. Thus, when designing cost containment initiatives for the Medicaid program, it behooves policymakers to directly address the growth rate of payments per capita for the ABD population and other high-cost categories of Medicaid enrollees. The program I evaluate in Chapter 3 provided DM interventions to the Medicaid ABD population in the state of Georgia.

A significant fraction of health expenditures have been linked to chronic conditions. For example, Anderson (2004) found that more than 80 percent of Medicaid expenditures for non-institutionalized beneficiaries are attributable to individuals with chronic conditions.[6] Members of the ABD population are far more likely to have one or more chronic conditions than the typical non-disabled adult or child. It is estimated that 35 percent of Medicaid recipients with disabilities and 39 percent of the aged had three or more chronic conditions, compared to 2 and 10 percent for non-disabled child and adult recipients, respectively. While Medicaid beneficiaries without chronic conditions rarely accumulate significant acute care expenditures, acute health expenditures increase with the presence of a chronic disease and nearly *all* of the highest-cost disabled beneficiaries have multiple conditions. Among disabled Medicaid enrollees, the 45 percent with three or more chronic conditions account for 75 percent of expenditures, and nearly all individuals in the highest percentile have one chronic condition (87 percent of the top percentile has three or more chronic conditions). (Kronick et al. 2007; Kronick, Bella, and Gilmer

[6] Estimate based on analysis of the 1998 and 2001 AHRQ Medical Expenditures Panel Surveys (MEPS) and the 2001 Medicare Standard Analytic File.

2009) The probability of hospital re-admission (within 30 days) is 15.2 percent among those with zero conditions, but is 36.1 percent among those with 10 or more conditions. (Gilmer 2010)

For common chronic conditions, medical practitioners have established guidelines that outline a recommended treatment plan or course of action for patients.[7] This often involves ongoing medical attention from the patient's medical providers, a "maintenance" regimen of medication or other treatments, regular monitoring to identify acute episodes at an early stage, and overall healthy lifestyle behaviors by the patient.

In practice, however, actual treatment of chronic conditions may fall short of these guidelines for a number of reasons. Not all patients manage their health as recommended, perhaps due to lack of education, addiction or lack of self control, and/or individual tastes. (Gertler and Simcoe 2009) In addition, patients' health care providers may contribute to the non-optimal treatment of health care. High-cost patients are very often treated by a "traditional" network of individual providers (doctors, specialists, nurses, laboratories, pharmacists, etc.) who each have a different expertise or responsibility, are motivated by a diverse set of financial or non-pecuniary incentives, and access different information. These providers are often uncoordinated and have a tendency (historically, at least) to focus on acute health events as opposed to the ongoing, routine needs of the chronically ill. (Beaulieu et al. 2006; Cebul et al. 2008; McDonald et al. 2007)

Furthermore, a growing body of research, including studies by the Agency for Healthcare Research and Quality (2007a), Institute of Medicine (2001), McGlynn et al. (2003), and National Committee for Quality Assurance (2007), compares the health care services that individuals receive to established clinical guidelines for the treatment of common chronic diseases and reports that a significant number of individuals receive "inadequate" health care. These shortcomings are linked to a number of expensive, undesirable outcomes: more bouts of acute

[7] These are also known as "clinical guidelines," "evidence-based guidelines," or "best practices." For example, the U.S. Preventive Services Task Force (USPSTF) provides recommendations for preventative clinical services and the NCQA publishes a set of indicators, the Healthcare Effectiveness Data and Information Set (HEDIS) that are used to track the quality of health care.

illness, avoidable hospitalizations, complications from co-morbidities, lengthy hospital inpatient stays, and more. Thus inefficiencies leading to under-utilization of health care in some aspects (noncompliance with a prescribed medication regime, smoking or not exercising, and the like), may in fact lead to over-utilization of health care (an emergency department visit that could have been avoided). Given these findings, it is believed that there remains significant potential for improvements in the management of chronic diseases to *prevent* health expenditures that are both avoidable and undesirable.

1.2 What is Disease Management?

DM programs have the broad objective of addressing shortcomings in the management of chronic diseases by aligning health care practice with the established clinical guidelines. DM is defined by the Disease Management Association of America as "a system of coordinated health care interventions and communications for populations with conditions in which patient self-care efforts are significant." (DMAA 2007) Most commonly, medical payers (insurers or, in the case of public programs, government agencies) implement these programs by (i) contracting with a Disease Management Organization (DMO) to provide identified services, (ii) building the necessary capabilities in-house or (iii) a "hybrid" combination of outsourced and in-house service provision.[8]

One observes a large diversity in the scope and methodologies of individual DM programs, although some general patterns emerge. Krumholz et al. (2006) developed a typology for DM programs, classifying programs according to patient population, intervention recipient (patient and/or health care providers), intervention content, intervention delivery personnel,

[8] Faulkner (2003) compares these approaches and discusses several Medicaid programs as case studies and Foote (2003) recommends the use of performance-based contracting with DMOs for the Medicare FFS population. Arora et al. (2008) and Roby, Kominski, and Pourat (2008) discuss a number of details related to Medicaid DM program design and contracting.

method of communication, intensity and complexity of interventions, environment (or context of interventions), and clinical outcome measures.

Not all individuals in a state's Medicaid program receive DM. In some states, only the highest risk patients are enrolled in a program, and therefore only a small fraction of the overall Medicaid population receive DM interventions (sometimes fewer than 1,000 individuals). In other states, large fractions of the population are eligible for services. For example, some states enroll the entire ABD population (about 25 percent of Medicaid enrollees) in their DM program.

Using the data that I collect on DM programs (discussed in detail in Chapter 2), I plot the number states with a program targeting selected chronic diseases in Figure 4. The most commonly targeted diseases – which I call the "top-5" chronic conditions – are diabetes, asthma, congestive heart failure (CHF), chronic obstructive pulmonary disease (COPD), and coronary artery disease (CAD).[9] The top-5 diseases are followed by hypertension and depression. Some DM programs target patients with other less common, but particularly high-cost diseases, such as sickle-cell anemia, schizophrenia, hemophilia, renal failure, and HIV/AIDS. Relatively fewer programs address other mental diseases, high-risk pregnancy, and cancers.

My understanding is that these "top-5" diseases are most commonly targeted because they sit at the intersection of several criteria: First, these diseases are relatively prevalent. (Kronick, Bella, and Gilmer 2009) Fixed costs of operation (for each disease) would cause states and DMOs to provide DM only for the most common diseases. Second, there is reason to believe that there are "gaps" in the management of these patient's diseases. The top-5 diseases are often discussed in relevant publications, including studies by the Agency for Healthcare Research and

[9] In the literature, these are consistently discussed as the five most commonly targeted diseases for Medicaid DM programs. See Arora et al. (2008, p. 3:2), Bella (2003, 2005), Kuo (2004), Owens (2006, p. 9), Rosenbaum et al. (2008, p. 13), and Williams (2004, p. 2). In a report on of private sector disease management programs, Matheson, Wilkins, and Psacharopoulos state the following: "Only a handful of chronic condition have been clearly identified and widely accepted as suited to the approach [disease management]: diabetes, asthma, CAD, congestive heart failure [CHF], and chronic obstructive pulmonary disease (COPD)—*conditions often referred to as the 'five core conditions.'"* (2006, p. 13) [emphasis mine]

Quality (2007a), Institute of Medicine (2001), McGlynn et al. (2003), and National Committee for Quality Assurance (2007). To the extent that these gaps are associated with undesirable health expenditures, states could generate financial savings by bringing health care into alignment with established guidelines. Arora et al. (2008, sec. 3) argue that these diseases have potential to yield outcomes within the required timeline of state administrators. Third, the recommended clinical guidelines are established for these diseases, allowing for DM interventions to be programmed into computer algorithms, call center phone "scripts," and other DM tools (unlike, for example, cancers which often require more idiosyncratic treatment plans for each patient).

Once administrators decide the set of diseases covered by a DM program, they typically use a risk stratification process to identify members who are expected to benefit from the DM interventions. This process usually includes an analysis of the individual's historic health insurance "claims" records and is sometimes supplemented with available clinical data, an in-house intake assessment, or other data acquired by the program's staff.[10] DM programs continue to monitor and evaluate the patients' health care utilization and symptoms (e.g., new medical claims or weekly phone calls regarding a diabetic's blood sugar) and use the new information to (i) identify opportunities for helpful interventions ("action items") and (ii) provide summary reports and recommendations to health care providers. In this capacity, the DM program's agents identify shortfalls from the recommended chronic care clinical guidelines and subsequently coordinate an appropriate response.

Selected patients receive targeted, proactive services from the DM program staff. Typically, services provide patients with education about their chronic disease, instruction and encouragement on how to manage their condition; they may also receive medical advice, such as information on smoking cessation or weight loss, non-specific to their medical condition. These

[10] DM programs implemented by health care providers (as opposed to insurers) may use Electronic Medical Records (EMRs) as the basis of their data analytics. Advances in information technology offer the potential for new methods, such as web-based education and remote monitoring, to be incorporated in DM program design. (Bigelow et al. 2008)

services are usually provided by nurses or other health workers from a telephonic call center, but may sometimes involve on-site case workers who visit a high-need patient's home or accompany the patient to a doctor visit. Some programs, including Georgia's GEC program (evaluated in Chapter 3), may coordinate an even larger scope of community services, such as transportation for the patient to attend health care visits. DM may be understood as one model for managing chronic illness(es) within broader frameworks of collaborative or managed health care, such as the so-called "chronic care model." (Von Korff et al. 1997; Wagner, Austin, and Korff 1996)

Thus, not all individuals enrolled in a DM program receive significant levels of interventions (except from the states that risk-stratify a large pool of patients, and *then* enroll the highest-risk patients into the DM program). Consider Georgia's program (evaluated in Chapter 3), which enrolled most ABD Medicaid recipients. The ABD population is about 22 percent of the population in a typical state. (SSA 2011) The Georgia program had one of the largest target populations, with 25 percent of enrollees classified into the "moderate" and "high" risk groups. Thus, even in this large program, less than 6 percent of Medicaid enrollees where likely receiving DM interventions in Georgia. Perhaps 75 to 80 percent of ABD patients have a chronic disease (Kronick, Bella, and Gilmer 2009), so in the case of Georgia, it is clear that a significant number of chronically ill individuals fall do not fall into these categories. For DM program enrollees without chronic diseases and enrollees with a chronic disease considered "low risk," the program may (only) affect them through DM services such as access to a "1-800" phone number to contact a nurse with questions. In some programs, administrators would routinely monitor insurance claims data and only "upgrade" these enrollees for more proactive DM interventions based on unexpected events.

In addition to the choice of DM interventions to provide, two policy dimensions are particularly important in the design of a DM program: administrators must carefully select enrollees into the program and then must choose the "intensity" of the interventions the enrollees receive. It is reasonable to believe that a significant fraction, perhaps the majority, of enrollees would be

poor candidates for DM interventions because the impact of DM services should be expected to vary within a heterogeneous population. If the program targets too large a population, it may provide costly services to members who are "too healthy" to show significant improvements; if services are limited to a very small population, the program may leave significant inefficiencies unaddressed.

The cost efficiency of a program will depend on the unit cost of providing DM to program participants, yet DM interventions are costly to provide. While some components of a program may benefit from economies of scale, many important components of DM depend on the time and involvement of skilled staff (e.g., nurses) and therefore are associated with nontrivial cost increases. If the intensity of DM interventions could be reduced to a single dimension, the relationship between intervention intensity and its effect on health expenditures is likely non-linear. At very low levels of intensity, the program might have no effect or a small effect and increased intensity is likely to experience diminishing returns at the highest intensity levels. Thus, a nontrivial selection process drives both (i) selection into the program and (ii) the intensity of interventions given to the enrollees. This implies that those who receive a given set of DM interventions are likely to differ from those who do not receive the interventions, making it difficult to find a reasonable comparison group.

According to the logic of DM program design, one may expect some Medicaid beneficiaries, including the ABD population, to respond especially well to DM programs.[11] There is a high prevalence of chronic conditions among the ABD population (see above), yet many of these patients receive "fragmented" health care from a number of providers in a fee-for-service payment model. Thus, DM programs may be able to take advantage of "low-lying fruit" of improvements in the care of their patients. As a simple example, Zillich et al. (2008) and Esposito

[11] For descriptive overviews Medicaid DM, see Arora et al. (2008), Faulkner (2003), Flowers (2007), Gillespie & Rossiter (2003), Health Strategies Consultancy (2004ab), Kuo (2004), RAND (2011), Wheatley (2001, 2002), and Williams (2004).

et al. (2009) found low rates of compliance to recommended prescription drug regimens among Medicaid patients with cardiovascular diseases, something that can presumably (i) be addressed by DM programs and (ii) could lead to substantial cost savings by preventing the need for hospitalizations and other high-cost medical events. In addition, there is less turnover in the group of individuals receiving SSI benefits, increasing the likelihood that investments in DM services would yield net cost savings before a typical individual exits the Medicaid program.[12]

On the other hand, there are several reasons why chronically ill individuals who receive their insurance from Medicaid may not respond positively to the introduction of a DM program (compared with those covered by other types of insurance). These patients have lower incomes than most other patient insurance pools and do not pay out-of-pocket for their health care. Thus, they do not have the same financial incentives to help avoid unnecessary health utilization as other patients. Compared to individuals with private health insurance, Medicaid enrollees may also have less education, less healthy living environments, or otherwise have more or less capability/willingness to manage their chronic condition and thereby comply with the objectives of the DM program.[13] Medicaid's payment rates and administration may also cause Medicaid patients' health care providers to behave differently than they do with other insurers. Finally, programs implemented by (or on behalf of) the public sector may also differ from the programs in the private sector. Thus, despite the fact that many observers point to the Medicaid ABD population as a "prime" population for DM interventions, it remains an empirical question whether Medicaid DM programs are, in fact, successful at meeting their objectives.

[12] The Medicare-Medicaid "Dual Eligible" population also has a high prevalence of chronic disease and low turnover. However, they are often excluded from DM programs because of a conflict of interest between the states and federal government: the state government would pay a large share of the DM program costs, yet the financial benefit of lower health expenditures (e.g., lower inpatient costs) would accrue primarily to Medicare, and hence the federal government. (Gruber 2000, pp. 10-11; U.S. Congress 2004, pp. 15:8-9)

[13] Rothman et al. (2004) found that a diabetes disease management program "benefited patients with low literacy to a greater degree than it did patients with higher literacy" in a small randomized trial.

1.3 Overview of findings

In Chapter 2, I use a national survey to study health care utilization patterns for chronically ill Medicaid patients between 1998 and 2007, a period in which the presence of DM programs expanded rapidly across the country, leading to substantial variation in the design and coverage of DM programs for Medicaid enrollees. I can identify the impact of these programs on health care utilization by exploiting (i) variation in the timing at which states implemented DM programs for Medicaid enrollees and (ii) within-state variation in eligibility criteria by disease, eligibility category, and/or county of residence. In some states, DM coverage varies across groups over time, as policies were changed to include or exclude particular classes of individuals from eligibility; empirical estimates are *not* identified with a simple pre/post statewide indicator. I link state/county DM program information to data on individuals surveyed in the nationwide Medical Panel Expenditure Survey (MEPS). Individuals linked to a DM program represent about 3.76 million individuals, or about 7.3 percent of the nation's Medicaid recipients.

Using difference-in-difference models, I test for evidence of a change in Medicaid expenditures after DM is implemented in a state or county. Although estimates have large standard errors, the point estimates are fairly consistent across model specifications, indicating that DM may cause a decrease in total medical expenditures, perhaps as much as $100 per person per quarter. Mean expenditures for chronically ill Medicaid enrollees are around $1,045 (excluding dual eligibles), thus $97 corresponds to almost a 10 percent decrease in total expenditures. The process of assigning individual to DM program may assign individuals to programs when they do not, in fact, receive DM interventions. Results, therefore, may be biased down.

This decrease in total expenditures occurs despite an increase in the probability that a chronically ill individual purchases medications or has an office visit with their medical provider. Expenditures decrease relatively more for patients with cardiovascular diseases or asthma than other common chronic diseases and the estimated effect is stable across a variety of specifications with different sets of relevant control variables.

The effect of DM is estimated imprecisely in Chapter 2 because individuals who are candidates for DM make up a relatively small share of survey respondents in the MEPS. This is one of the main limitations of this empirical method. Furthermore, the empirical approach with national data takes a fairly "high level" approach to studying the effect of DM, offering less insight into which types of DM programs work better than others, how well the programs target interventions to program participants, and the particular mechanisms through which DM has an effect.

Chapter 3 complements this research by focusing on a single Medicaid DM program, thereby gaining insight into these outstanding issues. In particular, I evaluate the Georgia Enhanced Care (GEC) Disease Management program. This program is of particular interest because idiosyncratic aspects of the program's implementation process postponed the introduction of the intended level of DM services for a significant number of high and moderate risk individuals. As such, some patients begin receiving high-intensity interventions when the program begins, while another group of observationally similar patients mistakenly receive only the low-intensity interventions at the beginning of the program. Moderate interventions included outbound phone calls tailored to their chronic conditions and high risk members were assigned to care managers who performed additional DM interventions. This feature of the program introduced a natural experiment that can be used to empirically identify the causal impact of the high-intensity and moderate-intensity interventions – administered to high and moderate risk patients – compared to the counterfactual of what would have occurred if these individuals had instead only receive the very minor low-intensity interventions. I am the first researcher to use this natural experiment to evaluate the effect of DM interventions. Detailed administrative data allows me to observe DM interventions and relevant health spending and utilization.

The point estimate indicates that the high and moderate intensity interventions lower the level of expenditures by an average of 4.4 percent for those patients with positive expenditures. The effect of the program is much larger for the high-intensity interventions. A subset of patients

sometimes has no medical expenditures in a given month; for these patients, the intensive DM interventions increase the probability of a zero-expenditure month by about 0.8 percentage points. Of the five diseases most commonly covered by DM programs, the largest decrease was for asthma, which was associated with a large decline in the number of asthma-related hospital and emergency department admissions. The source of empirical identification – a delay before some patients began receiving DM interventions – also allows me to identify the effect of the program over time. I find evidence that the decrease in medical expenditures from the high-intensity interventions appears relatively early (within the first 8 months of the program) and is sustained over time.

In addition, I examine the quantile treatment effect of these high and moderate intensity interventions over the distribution of medical expenditures using the methodology from Athey and Imbens (2006). In the baseline specification, the 90th percentile saw a decrease of about $200 per member per month, while the 10th percentile decreased only $80 per member per month. Because the baseline medical expenditures are highly skewed, this is actually a larger *percentage* decrease in expenditures for the lower end of the distribution.

Taken together, the results in these chapters indicate that appropriately targeted Medicaid DM programs can decrease medical expenditures for chronically-ill Medicaid enrollees. But the impact is relatively modest.

Chapter 2: The Effect of Medicaid Disease Management Programs on Medical Expenditures – Evidence from the Medical Expenditure Panel Survey

2.1 Introduction

In recent decades, states have experimented with various Medicaid policies to contain costs and improve health care outcomes for program participants. This chapter focuses on one strategy widely adopted by states across the country: the introduction of Disease Management (DM) programs for Medicaid enrollees with chronic diseases. By design, DM programs aim to increase the share of chronically ill patients who adhere to established clinical guidelines for their chronic disease(s). Program designs vary widely, but it is common for nurses or other trained staff to interact with enrollees (and/or their health care providers) by telephone, providing data-driven health interventions such as health education, health coaching, monitoring, or assistance coordinating health care services. These interventions are intended to reduce the likelihood of future complications with the patient's chronic disease, mitigating unnecessary health expenditures via better management of their condition(s).

Cost-containment has been a key justification for the creation of DM programs, despite mixed evidence that DM actually saves money for the Medicaid program or for society as a whole. The financial impact of Medicaid's disease management programs have been evaluated in only a few cases, while comprehensive literature reviews from the private insurance sector failed to find conclusive evidence that DM reduces net medical expenditures. (e.g., CBO 2004; Goetzel et al. 2005; Mattke, Seid, and Ma 2007) Despite limited evidence of cost savings, Medicaid DM programs for the chronically ill have become a popular component of state Medicaid programs throughout the United States over the last decade and a half. Figure 1 plots the number of states with a Medicaid Disease Management program, showing a significant increase in DM coverage

in the late 1990s and early 2000s. At the end of 2008, 36 states with at least one DM program accounted for 82 percent of the total Medicaid population in the United States.

This chapter empirically estimates the effect of Medicaid chronic disease management programs using nationally representative survey data. While the existing DM impact evaluation research literature measures the impact of individual Medicaid DM programs, this chapter explores the average "national" impact of multiple state Medicaid DM programs. In particular, I use a national survey to study health care utilization patterns for chronically ill Medicaid patients between 1998 and 2007, a period where the presence of DM programs expanded rapidly across the country, leading to substantial variation in the design and coverage of DM programs for Medicaid enrollees. I can identify the impact of these programs on health care utilization by exploiting (i) the timing at which states implemented DM programs for Medicaid enrollees and (ii) within-state variation in eligibility criteria by disease, eligibility category, and/or county of residence. Although this approach has been used to study other aspects of the Medicaid program and other public expenditure programs,[14] I believe this is the first study to use this methodology for estimating the effect of Medicaid DM on medical expenditures for common chronic diseases.

After constructing a detailed survey of state Medicaid programs,[15] I link state-month-disease eligibility criteria with individuals surveyed in the nationwide Medical Panel Expenditure Survey (MEPS). Using difference-in-difference models, I test for evidence of a change in Medicaid expenditures after DM is implemented in a state or county. The effect of DM is estimated imprecisely because individuals who are candidates for DM make up a relatively small share of survey respondents in the MEPS. I investigate the robustness of these results to a variety of control variables – including state-time and disease-time interaction term effects – and sub-

[14] Difference-in-difference models that exploit variation in program implementation are well known in public economics. To name a few examples on Medicaid policy, Yelowitz (1995), Currie and Gruber (1996, 2001), and Dafny and Gruber (2005) use difference-in-differences to evaluate the impact of Medicaid eligibility expansions on health outcomes. Currie and Far (2005) and Burns (2009) study the effect of Medicaid Managed Care.

[15] See Section 2.3.2 and Appendix A for information on data construction.

samples of the data. This approach allows me to estimate the effect of Medicaid DM on individuals with targeted diseases, under the assumption that medical expenditures for other, non-targeted Medicaid enrollees (or individuals with other insurance) (i) are not influenced by DM spillover effects and (ii) have secular trends that are not correlated with the timing of DM program introduction. To the extent that DM influences long-term health care expenditures – a category excluded from the MEPS – my estimates will understate the overall population.

Although estimates have large standard errors, the point estimates are fairly consistent across model specifications, indicating that DM may cause a decrease in total medical expenditures, perhaps as much as $100 per person per quarter. This decrease in total expenditures occurs despite an increase in the probability that a chronically ill individual purchases medications or has an office visit with their medical provider. Expenditures decrease relatively more for patients with cardiovascular diseases or asthma than other common chronic diseases. Due to the lack of comprehensive, standardized data on program costs, the net savings to state Medicaid programs cannot be established at this time.

In the remainder of this chapter, I first provide background on Medicaid DM programs. Sections 2.3 and 2.4 then introduce the data used in this study and my empirical approach. Results are discussed in sections 2.5 and 2.6 and a concluding discussion is provided in section 2.7.

2.2 Background

Medicaid DM programs are designed to improve the management of chronic conditions for enrollees by aligning patient behavior and health care providers' interventions with established clinical guidelines for the patient's chronic disease(s). The motivation behind DM can be understood from a cost-containment perspective, as growth in Medicaid expenditures has outpaced growth of state revenue, inflation, and GDP, increasing the pressure on program administrators to deliver benefits efficiently. The crux of cost-containment initiatives lies in the

fact that most Medicaid spending is concentrated on a small subset of very high-costs enrollees, many of whom have chronic diseases. The top five percent of enrollees accounted for 57 percent of Medicaid spending in 2004.[16] (KFF 2009) Over 80 percent of Medicaid expenditures for non-institutionalized beneficiaries are attributable to individuals with chronic conditions. (Anderson 2004) Given that a significant number of individuals with chronic conditions do not meet established clinical guidelines (AHRQ 2010; IOM 2001; McGlynn et al. 2003; NCQA 2007), there is reason to believe that costs could be reduced through cost-effective management of chronic conditions. In theory, Medicaid DM can increase the quality of living for program participants and lower Medicaid expenditures simultaneously.

DM interventions are intended to improved management of the chronic conditions (i.e., increased use of "preventive care"), which in turn will be followed by a reduction in total expenditures. In particular, increased adherence to established clinical guidelines should mitigate the risk of expensive complications in the short term (e.g., ED visits and hospitalizations) and the long term (e.g., the development of additional comorbid conditions). States explicitly (or implicitly) "target" patients with chronic illnesses such as asthma, chronic obstructive pulmonary disease (COPD), diabetes, coronary artery disease (CAD), congestive heart failure (CHF), and other diseases. A typical DM program collects and monitors data on the enrollees' health care, identifies intervention opportunities ("gaps" in care *vis-à-vis* the relevant guidelines), and then provides services intended to improve the education or health behaviors of the enrollee and/or his or her health care providers. These services are most often provided through a telephonic call center staffed by nurses or other trained staff, but may include mailings, in-person visits, electronic monitoring systems, and other forms of contact. Program administers try – with

[16] Estimate from the Kaiser Family Foundation (2009). Kennedy, Ruhter, and Selden (2009) show that 73 percent of spending for children in Medicaid/CHIP is for children in the highest decile and two-thirds of this spending is for children with a chronic disease; the top three deciles account for 90 percent of spending. A number of issues relevant to health care quality for high-cost Medicaid enrollees are discussed by Lipson et al. (2010).

varying degrees of sophistication – to balance the intensity of interventions with the potential benefit expected from intervening in the health care of a particular beneficiary. It is common for a small subset of high-cost (or "high-risk") patients to receive the majority of attention from DM program staff.

Naturally, Medicaid DM programs vary from state-to-state along a wide variety of dimensions. The term "disease management" does not refer to a homogeneous set of interventions or policies. Instead, it a loose term that can involve a variety of different target audiences, interventions, and institutional arrangements. Most commonly, states implement DM programs by contracting with a Disease Management Organization (DMO) to provide identified services. In some cases, they build the necessary capabilities in-house or use a "hybrid" combination of outsourced and in-house service provision. DMOs often have actuarial cost-savings targets they must meet in order to receive full payment for the services.

Programs have idiosyncratic rules about who exactly is eligible for program interventions. Inclusion or exclusion can be based on age (e.g., children, adults, aged) or categorical definitions (e.g., SSI, dual eligibles). Some programs have large eligible populations and then give their DMO vendors latitude to use risk stratification algorithms to select which individuals receive interventions. For example, the Indiana Chronic Disease Management Program enrolled participants from the Aged, Blind, and Disabled (ABD) population who had diabetes or CHF while the Georgia Enhanced Care Program included most ABD adults and children in the program, regardless of chronic disease status, and allowed the program vendor to target interventions by disease. More on these idiosyncrasies is discussed below.

Matheson, Wilkins, and Psacharopoulos (2006) and Shelton (2002) describe an evolution in DM program design in the private sector. In the early 1990s, DM was primarily sponsored by pharmaceutical companies and focused on the correct use of medications. In the mid-1990s DMOs began to provide a wider array of DM services for payers and hospitals, usually focusing on a single disease. Recent trends in the private sector point towards coverage for multiple

diseases (i.e., comorbidity pairs) and some programs are now targeting populations at risk of acquiring a chronic condition. My review of Medicaid DM programs indicates that this trend was mirrored in Medicaid. Early programs in Virginia (1997) and Mississippi (1998) focused on program enrollee's drug usage. The "Florida: A Healthy State" program (2001/02), was funded by pharmaceutical companies as a negotiated settlement to avoid paying rebates for their drugs inclusion on Florida's preferred drug list and to "to test the theory that better management of services and drugs would reduce costs and achieve better health outcomes through DM programs." (White et al. 2005) As Medicaid DM evolved, program designs tended to have more comprehensive intervention approaches and often explicitly targeted one disease or a few diseases. Recently implemented (or redesigned) programs in Maine, Nebraska, Oklahoma, Texas, and Washington state rely primarily on risk-stratification, not simply disease categories, to identify who receives DM services.

As discussed in the literature review below in Section 3.2, there are several studies that estimate the financial impact of Medicaid DM programs by evaluating a single DM program. Although some studies report that Medicaid DM may lower medical expenditures for (at least a subset of) treated patients, it is unclear how much external validity there is to these findings, particularly when one considers the widespread heterogeneity in DM program design. This chapter attempts to understand the wider impact of DM, beyond one-at-a-time evaluations of individual DM programs. Although this method abstracts from the rich details idiosyncratic to specific programs, it may help by providing an indication of the aggregate financial effect of Medicaid DM.

Katz et al. (2009) provide what is perhaps the most comparable study to this chapter. They use the case of the Indiana Chronic Disease Management Program to study the impact of Medicaid DM on Medicaid expenditures. The state was divided into three regions and DM began in the central region about one year earlier than elsewhere. Using administrative claims data, they report that DM interventions for CHF and Diabetes patients lowered the growth rate of total per-

member-per-month Medicaid expenditures. However, this interpretation is not straightforward, given that the growth rate in expenditures also falls in the non-treated regions at the same time the central region begins receiving treatment; this finding casts some doubt on the causal link between the observed break in the trend in expenditures and the DM intervention.

Also related, a study by Rossiter et al (2000) examines the Virginia Health Outcomes Health Partnership, a program for fee-for-service asthmatic enrollees. Here, particular communities received DM interventions and these communities were matched to comparable communities based on observable characteristics. The relative risk of an emergency department visit ranged from .74 to 1.09 (treatment vs. comparison communities) over the quarters in the post-intervention period and use of acute-asthma inhaler medications increased.

2.3 Data

This study combines data from several sources. The Medical Expenditure Panel Survey (MEPS) provides data for patient's health utilization and outcomes. Second, I merge the MEPS with state- and county-level panel data on DM programs available to Medicaid enrollees, which I compiled for the purposes of this study.

2.3.1 Data on health outcomes (MEPS)

The primary data source on health care utilization and outcomes is the Household Component of the Medicaid Expenditure Panel Survey (MEPS), a household survey that focuses on health care expenditures and health care insurance. The MEPS is collected by the United States Agency for Healthcare Research and Quality (AHRQ) and is regarded as one of the most complete sources of nationwide data regarding the cost and use of health care and health insurance coverage in the United States. Each year, a new panel of sample households is selected from the previous year's National Health Interview Survey (itself a nationally representative survey) and each panel responds to five "rounds" of interviews covering two calendar years. Panels are introduced yearly, so that two panels are interviewed in any given calendar year. When

adjusting for the complex design of the MEPS, the sample is considered nationally representative of the civilian, non-institutionalized population. The Household Component fields questionnaires to individual household members and their medical providers to collect complete, detailed health-related information on the individuals, including health insurance coverage, health conditions, health status, use of medical care services, charges and payments, access to care, and satisfaction with care.[17]

Unless otherwise noted, I limit the analysis sample to individuals in the MEPS dataset who receive health insurance through the Medicaid program at some point in the two calendar years of survey reporting.[18] The data is restricted in this manner due to the concern that trends in health expenditures may be different for individuals in the Medicaid program (compared to the rest of the population) due to national Medicaid policies and other factors. In alternative specifications, I check for robustness by experimenting with data samples that include chronically ill survey respondents who are not insured by Medicaid. In most specifications, I use all available observations (quarters) for the individual, even if they leave the Medicaid program. However, I also show results using alternative data samples; for example, I present results dropping observations (quarters) where an individual is not enrolled in Medicaid and limiting the sample to individuals who are continuously enrolled (see p. 47-49 and Table 6).

The data in this study pools observations for interviews conducted in 1998 through 2008 (i.e., the second year of panel 2, both years of data for panels 3 through 11, and the first year of panel 12). For each household, I compile quarterly time series data with the number of health-related events, by type of service, and total expenditures by payment source. I use the

[17] For more information on the MEPS's survey design, sample, and questionnaires and to download the public-use files, refer to the MEPS website (http://meps.ahrq.gov), Ezzati-Rice, Rohde, and Greenblatt (2008), and Cohen et al. (2003; 2009).

[18] I always drop individuals who appear in the raw MEPS files, but are excluded by AHRQ from the MEPS sample because they do not meet the eligibility criteria (i.e., individuals not in the civilian, non-institutionalized population).

methodology from Selden (2009) for assigning each event to the date of its occurrence.[19] The month and year of most health events is provided by MEPS, although individuals are not asked for the dates they filled pharmaceutical prescriptions. However, I could often assign pharmaceutical events to a quarter of the year based on (i) when the person first started taking the prescription or (ii) the date of an event linked to the prescription (e.g., a medicine taken in the hospital during an inpatient stay). For the pharmaceutical events that cannot be assigned to a specific date in this manner, I assigned a date at random in the year and interview round in which the particular event occurs.[20] As the MEPS conducts five rounds of interviews covering two years, this procedure leads to a reasonable chance of assigning an event to the quarter of the year in which it actually occurred, although measurement error is introduced. When building this dataset with event-level data (not AHRQ's "consolidated" summary files), I tabulate quarterly health expenditures with mutually exclusive categories by payer and type of service. Before calculating quarterly totals, all charges are adjusted to January 2005 dollars using the monthly series of the BLS's Consumer Price Index for all urban consumers.

A notable limitation of the MEPS is that the survey excludes institutionalized individuals – those living in nursing homes, other long-term care facilities, and prisons – and omits most Medicaid long-term medical care expenditures. This may not be a first-order concern because many Medicaid DM programs (e.g., Georgia's program evaluated in Chapter 3) exclude institutionalized individuals from program eligibility under the assumption that DM interventions would be redundant because nursing home personnel already perform education, care coordination, or other interventions. To the extent that DM influences long-term health care

[19] I thank Thomas Selden for generously sharing event-date assignment files from his study. For the years prior to or after his study, I follow his methodology as closely as possible.

[20] I also randomly assigned dates for a very small number of other events where the date was not reported. This was the case for 0.1 percent of inpatient, outpatient, and emergency department events, for example. Dates were most commonly missing for "other" (uncategorized) medical expenditure events, a category which accounts for just 1.7 percent of all Medicaid spending.

expenditures, my estimates will understate the overall impact of DM on the individual's total health care expenditures.[21]

The MEPS questionnaire asks respondents if they have one or more chronic conditions. In addition, diagnosis codes are recorded for any of the individual's medical events in the two calendar years of the MEPS. These ICD-9 diagnosis codes are reported at the 3-digit level for confidentiality. Following Lewis (2009), I identify inpatient, outpatient, and emergency department events with diagnosis for the following chronic illnesses: Asthma (493), COPD (491, 492, 494, 496, 506), CAD (410, 411, 413, 414), diabetes (250), and Heart Failure (404, 425, 428). These are the five most common chronic diseases targeted by Medicaid DM programs and are referred to as the "top-5" chronic conditions in this study. To identify an individual's chronic illnesses, I identify them as having the illness if they responded positively to the MEPS questionnaire *or* if the MEPS contains an event where they were diagnosed with the disease. This combined methodology gives higher diagnosis rates than either method alone. Other algorithms used in Chapter 3, such as the CDPS and the PQI procedure to identify "potentially preventable" hospitalizations, depend on 5-digit ICD-9 codes and cannot be used with the MEPS dataset.

The MEPS does not collect information on DM program enrollment, DM-related program (e.g., fees paid by Medicaid to a DMO or costs incurred by states for the administration of a program). Therefore, DM enrollment and the related costs cannot be observed directly in the MEPS.

2.3.2 Data on Medicaid Disease Management

In addition to the MEPS, this study requires a second dataset containing information on Medicaid DM programs in each individual's state of residence in each quarter. Comprehensive data on Medicaid DM programs has not been previously compiled; this study collects relevant

[21] This sentence assumes that DM affects long-term medical expenditures works in the same direction as other medical expenditures. This would not be the case if, for example, DM somehow delayed medical treatments until after the program participants enter nursing homes or if DM increases long-term medical use, beyond the amount saved on all other expenditures in the near term.

information from a variety of sources. This data represents a history of Medicaid DM programs for all 50 states plus the District of Columbia through the end of 2008. For each state and quarter, I identify if there is at least one DM program or pilot program, diseases the program(s) targeted, if dual eligibles were included in the program, and other details. This information was collected at the county-level when states implemented programs that were not statewide.

Additional details on the DM program data file construction, a list of programs, and a list of key data sources are included in **Appendix A**.

My definition of DM has remained broad enough to reflect the evolution in DM program design over the last few decades, although I generally attempted to follow the literature, including programs referred to as "disease management" by other researchers and excluding programs that were not. As the design of Medicaid DM program activities are heterogeneous in this sample, I also indicate whether the program belongs to a subset of programs meeting a more *strict* definition of "disease management." This definition is an attempt to restrict the sample of programs to "traditional" telephonic-oriented programs targeted at subgroups of the population who are high-risk. This *strict* definition rules out programs that feature relatively less-intensive interventions, pharmaceutical-management programs, programs that interact exclusively with health providers (not patients), and a few other programs. In my estimates below, I infer if an individual might be enrolled in a DM program by comparing characteristics of an individual in the MEPS to characteristics about the DM program(s) in his or her state/county of residence.

Data on DM program costs could not be collected uniformly. Table 1 presents estimated program costs for a few example programs. Costs per enrollee range from $1 to over $90 per member per month (PMPM). Very low program costs tend to occur when states select large demographic groups for program eligibility with their DMO contractor (with a PMPM fee for the entire eligible population), and then leave the DMO to target DM interventions to a subset of the individuals who they deem appropriate. (Arora et al. 2008, pp. 5-8) This was the case in Georgia's program, where about one-quarter of eligible members were classified as "moderate"

or "high" risk and therefore receiving the bulk of the DM interventions. (see Chapter 3) Thus monthly costs per high/moderate risk member was over $125, even though the *average* monthly cost per *enrollee* was $32. My estimates below make no attempt to estimate DM-related expenditures. Therefore, my results only represent one-half of a full cost-benefit analysis.

Most of the MEPS data used in this study is publicly available. However, the MEPS public access files do not identify each individual's state of residence (for confidentiality), preventing the combination of Medicaid DM information with the survey responses. Therefore, all research was conducted in the AHRQ's data center, where DM program information could be merged with each individual's state and county of residence with the assistance of AHRQ staff. Access to the MEPS data at the AHRQ data center is available by application.

2.4 Methodology

This chapter exploits cross-state and within-state variation in DM coverage to estimate difference-in-difference models for the impact of Medicaid DM programs on quarterly health care utilization. This section (i) discusses the variation in DM coverage and then (ii) introduces the econometric specification of the models.

2.4.1 Variation in DM coverage

2.4.1.1 Cross-state variation

Over the last decade and a half, Disease Management programs for the chronically ill became a popular component of state Medicaid Programs throughout the United States. I have identified 41 states (including the District of Columbia) that have implemented a DM program or pilot program for their Medicaid enrollees. The 36 states with an active DM program at the end of 2008 account for 82 percent of the total Medicaid population in the United States. As shown in

Figure 1 (Panel B), this represents a significant increase from 4 percent in the first quarter of 1998.[22,23]

DM activities remained relatively limited until 1998, when Florida and Mississippi implemented the first statewide programs for Medicaid FFS patients that targeted multiple diseases, making these states the primary "early adaptors" of Medicaid DM programs.[24] Over the next several years, DM began to gain more widespread attention and the Centers for Medicare and Medicaid Services (CMS) began to discuss DM as an important component of future operations. (CMS 2004, 2006) States have continued to experiment with DM programs over the last decade. By the end of 2004, over half of the states had established or were developing Medicaid DM programs for the FFS populations. To date, 41 out of the 51 Medicaid agencies have implemented some kind of program (Figure 5). This growth in Medicaid DM largely tracks, with a lag, the dramatic rise in the DM service industry in the private sector. (Matheson, Wilkins, and Psacharopoulos 2006; Williams 2004)

Two factors are associated with the ten states that did not implement a DM program and other "late adopters": overall Medicaid enrollment and Medicaid Managed Care (MMC) penetration. First, several states without Medicaid DM programs have small Medicaid populations (e.g., Alaska, D.C., Delaware, Hawaii, and South Dakota). At the other end of the spectrum, California and New York (the two largest large Medicaid populations) have not implemented statewide DM programs, but have implemented regional programs covering a subset of the states' counties. Several other states, such as Arizona, South Dakota, Tennessee, and (until

[22] This discussion on state-based coverage of Medicaid DM is based on tabulations from the author's survey of Medicaid disease management programs. Details about the data are discussed in section 2.3.2 and Appendix A. Maps in Figure 6 identify states that had implemented a DM program at two arbitrary points in time – December 31 of 1999 and 2008. This discussion applies to fee-for-service or similar insurance payment schemes (i.e., excluding DM in Medicaid Managed Care).

[23] For descriptive overviews Medicaid DM, see Arora et al. (2008) (2008), Faulkner (2003), Flowers (2007), Gillespie & Rossiter (2003), Health Strategies Consultancy (2004ab), Kuo (2004), RAND (2011), Wheatley (2001, 2002), and Williams (2004).

[24] Maryland and Virginia had introduced the first DM programs in 1991 and 1993, respectively, for a limited group of patients with specific diseases.

recently) Kentucky, rely primarily on MMC programs to provide care to their members and have very few, if any, fee for service enrollees. However, these factors are certainly not prescriptive: there are states that have DM programs, despite having small enrollments (e.g., North Dakota and Wyoming), high enrollment (e.g., Texas), or high MMC penetration (e.g., Oregon).

To implement DM programs, most states hire an outside disease management organization (DMO) to run their DM programs (about two-thirds of the programs) as opposed to running the program in-house or through a hybrid model. Most commonly, programs target patients with specific chronic diseases, but a few states target patients with previously observed high costs (e.g., Kansas and Oklahoma) or designate large groups of patients as eligible for the program and then expect the DMO to prioritize patients for more intensive DM interventions (e.g., Georgia and Texas).[25]

This cross-state variation in the timing of DM program rollout serves as the main source of variation for difference-in-difference estimates of the impact of a DM program on health care expenditures and other health care utilization metrics. The underlying assumption is that the exact timing of DM program implementation is not correlated with other determinates of health care utilization. One state may implement their DM program a few months or years earlier than other similar states, and my model relies on the assumption that this variation in timing is not systematically related to the outcome determinants, except through the impact of the DM interventions themselves. For example, a standard difference-in-differences analysis would be confounded if states are more likely to implement a DM program earlier if a particular patient pool had particularly high medical expenditures. Another potential concern is that a DM program's implementation occurs concurrently with other state-specific policy changes (or other factors) that affect medical expenditures. Fortunately, there are several sources of within-state variation in DM coverage that can be used to control for potential state-specific trends in the

[25] Arora, Boehm, Chimento, Moldawer, and Tsien (2008) provide an introduction to Medicaid DM program design and contracting.

outcomes of interest. My empirical estimates do *not* use a simple pre/post statewide indicator, but are instead determined for each individual based on geographic and categorical determinates, exploiting with-in state variation in DM coverage.

2.4.1.2 Within-state variation

Exclusion of dual eligibles: First, DM programs are often implemented with categorical eligibility rules that exclude subgroups of Medicaid enrollees who are fairly similar to the individuals included in the program. One example is the common practice where states provide DM services to the ABD Medicaid enrollees while excluding individuals eligible for both the Medicare and Medicaid (henceforth, "dual eligible" enrollees). This policy is the result of the fact that state Medicaid programs have little incentive to provide DM services to a dual eligible enrollee: the state government would fund the costs for the individual's DM services, but the financial savings would likely benefit the federal government via the patient's Medicare insurance coverage. While both of these groups are similar – at least to the extent that both groups have higher rates of chronic diseases and higher levels of health care utilization than other Medicaid enrollees – one group often receives DM while the other does not. To the extent that health care utilization in the state is affected by the same factors (for example, state Medicaid policy, health care cost inflation, or access to new health care technology), the dual eligible population can be used to identify state-year changes in utilization patterns that would have affected the ABD population, even if a DM program had not been introduced. My DM treatment variable in the analysis below always matches DM programs to an individual taking into account his or her status as a dual eligible enrollee.

Disease targeting: Second, Medicaid DM programs, with few exceptions, "target" patients with selected chronic diseases, providing relevant program interventions only for the selected disease(s). The most commonly covered diseases, which I call the "top-5" chronic conditions, are diabetes, coronary artery disease (CAD), congestive heart failure (CHF), asthma, and chronic obstructive pulmonary disease (COPD). As I discussed in section 1.2, these diseases

are most commonly covered because (i) they are relatively prevalent, (ii) their DM interventions can be more easily standardized, and (iii) shortfalls in health care quality are common. Some DM programs target patients with other less-common, but particularly high-cost diseases such as sickle-cell anemia, schizophrenia, hemophilia, renal failure, and HIV/AIDS.

There is substantial variation in the list of diseases targeted from state to state. For example, the state of Washington implemented a large DM program targeting asthma, diabetes, and chronic heart failure (CHF), which rolled out in 2002/2003. Shortly later, in 2004, the state of Texas contracted with the same company to provide a DM program that was similar to the program implemented in Washington. One of the key differences (aside from implementation timing) was that Texas's DM program targeted patients with COPD and CAD, in addition to the diseases targeted by Washington's program. Thus, in the time period after both programs are active, the COPD and CAD patients in Washington remain a comparison group for as similar group of treated patients in Texas. As different type of example, consider the state of Missouri. A statewide DM program was implemented in 2002 and covered asthma, COPD, diabetes, and CHF patients. In 2006, administrators modified the program and began targeting a number of additional diseases. Thus, Missouri provides one example where policy changes caused DM coverage to change for individuals with different diseases *within a state over time.*

The major exception to disease-based policies is a group of states that select patients based on historical claims data, targeting patients with high utilization.[26] In this chapter, I focus on the "top-5" chronic conditions, as the MEPS data is not well suited for identifying the impact of DM on populations that represent a very small share of the United States. There are simply not enough Medicaid enrollees in the MEPS survey to reliably estimate the effect of DM programs that target rare diseases or other limited populations.

[26] For example, the Enhanced Care Coordination Program in Nebraska targeted an estimated 850 patients with previously observed costs over $50,000 per year (who were *not* foster children, dual eligibles, individuals in MMC programs, in a nursing home, or belonged to other groups).

County-level variation: Third, some states either implemented limited pilot programs or introduced programs in limited geographical regions. (See Figure 1.) Thus, these states have periods of time where some patients receive DM services while there are patients in other counties who are otherwise similar, yet are not included in the DM program. To the extent that both groups are subject to policies (or other factors) that would affect the health outcomes for chronically ill patients statewide, the patients in un-treated counties can serve as proxies for trends in expenditures that would have occurred if these programs had not been implemented. For example, this is the case in the two states with the largest Medicaid populations, California and New York. Although neither state implemented a statewide program, both states have large programs in particular counties or regions. California implemented a "pilot" program in 2007 in Alameda and Los Angeles counties – two counties with a combined Medicaid population larger than most *states*. (KFF Kaiser Family Foundation (KFF) 2011a; Lundy et al. 2004, sec. 5.1c) The "Regional DM program or pilot program" lines in Figure 1 indicate this is fairly common, especially in larger states. In the empirical work below, I use a person's county of residence to identify individuals who are covered by a DM program, where appropriate.

DM coverage can also vary within states over time due to county-level variation. Several states implement "pilot" programs that cover a few counties and then are scaled up or replaced with statewide programs. For example, Texas transferred patients from a pilot program (six cities) into a new statewide program in November 2004. Indiana provides another example of within-state (county-level) variation in DM coverage over time; this case was evaluated by Katz et al. (2009) and is discussed below (section 2.2).

2.4.2 Econometric specification

I use difference-in-difference models to estimate the impact of Medicaid DM programs on quarterly health care expenditures (by payer and type of category of service) and other utilization measures. As discussed in more detail below, I use generalized linear models (GLM)

because the density of medical expenditures exhibits a mass at zero dollars and positive skewness. In particular, I estimate models of the following form:

$$\mathrm{g}(\,\mathrm{E}(y_{it})\,) = \alpha + \delta\, DM_{it} + \beta\, X_{it} + \lambda_s + \theta_t + \mu_p, \qquad y \sim F \tag{1}$$

y_{it} is a measure of health care expenditures (or other outcome) for person i in quarter t. All specifications include fixed effects for the individual's state of residence (λ_s), the quarter (θ_t), and MEPS panel (μ_p). I discuss the GLM link function ($\mathrm{g}(\,)$) and distributional family (F) below. In a series of additional regressions, I examine the sensitivity of the results to including state-specific linear time trends, state-by-year interaction fixed effects, and chronic disease-specific linear time trends, chronic disease-by-year fixed effects, chronic disease-by-quarter of year (seasonality) fixed effects, and other controls (not shown in equation 1). Section 2.6, below, discusses a special case where individual fixed effects can be included in the model.

The disease management treatment variable, DM_{it}, equals one if individual i lives in a state or county with a Medicaid DM program in quarter t. As the MEPS does not collect information on DM program enrollment or program fees paid by Medicaid to a DMO, I cannot observe DM enrollment directly. Instead, I *infer* if an individual might be enrolled in a DM program by comparing characteristics of the individual to characteristics about the DM program(s) in his or her state/county of residence. In the main specifications, I construct DM_{it} by matching an individual's insurance coverage and chronic diseases (asthma, COPD, diabetes, CAD, or CHF) to the program eligibility criteria and target disease(s) for any DM program(s) in their geographic region.[27] For example, $DM_{it} = 1$ for a dual-eligible asthmatic enrollee only in the case where the geographic region has a DM program that (i) targets asthma patients and (ii) accepts dual eligible enrollees. Conversely, $DM_{it} = 0$ if there was no DM program in the

[27] $DM_{it} = 0$ whenever the individual an individual is not enrolled in Medicaid. If someone with DM leaves a Medicaid program, the DM_{it} variable switches from 1 to 0.

state/county *or* the DM programs do not target asthma *or* the DM programs do not admit dual eligible patients.

As was discussed above, DM coverage varies across individuals due to (i) variation in the quarter at which their state of residence introduced DM programs, (ii) variation in DM eligibility between Medicaid enrollees that do not have Medicare coverage and those that do (dual eligibility), (iii) variation in the chronic diseases targeted by the DM program in their region, and (iv) the fact that some DM programs were not implemented statewide (introducing county-level variation). Thus DM_{it} is constructed for each observation for each individual through a matching process. The variable can equal one for some individuals in a state, but not others, and the inclusion/exclusion criteria *can change over time within a state* as states change the diseases, counties, and/or eligibility groups targeted by their program(s).

Any DM treatment variable with this data set will ultimately be imperfect, as the data in this study does not contain detailed enough information to link individuals to specific DM programs. Even if one could perform this data linkage using (unavailable) eligibility criteria or administrative enrollment data, one would not necessarily be able to identify the patients who received DM *interventions* (e.g., a phone call from a nurse) because many DM programs only provide substantive interventions to a subset of enrolled patients: those who are deemed "high risk" patients or otherwise meet idiosyncratic (or proprietary) criteria. Therefore, this treatment indicator may be more accurately considered an indicator for a policy intention to provide DM interventions within a *population* of Medicaid enrollees – specifically, the population of patients who share i's state/county of residence, chronic diseases, and insurance coverage in period t. One concern is that this treatment variable inappropriately classifies untreated individuals in the MEPS to the "treated" group. Furthermore, this construction will omit the DM programs that do not target the "top-5" five chronic diseases (discussed above), misclassifying observations into the "untreated" group when they are, in fact, receiving DM. To the extent that the treatment variable tends to misclassify individuals in this direction, I expect the models in this study to

underestimate the impact of DM programs.[28] I experiment with alternative measures of DM_{it} below.

The remaining variables, represented by the vector X_{it}, are an array of individual-level dummy variables: gender; 10-year age bins; race/ethnicity categories for white non-Hispanic, black, Hispanic, Asian or Pacific-Island, or Native American; highest education completed as no degree, GED, high school diploma, bachelor's degree, graduate degree, under-16, or other/missing; the presences of the five chronic illnesses focused on in this study (discussed above); and insurance coverage (in quarter t) by Medicaid, Medicare, both, or neither.

In the results below, I focus on the effects of DM on total health expenditures (all payers). In additional specifications, I also consider several sub-categories of health expenditures, grouping by payer (especially focusing on Medicaid expenditures) and type of service (e.g., inpatient, outpatient, office visits, pharmaceuticals, etc.). Disaggregation by payer allows, for example, one to determine the effect of the program on medical expenditures paid by Medicaid, which could be used to estimate the return on investment (ROI) for the Medicaid program if paired with detailed program cost information. Disaggregation of expenditures by service type allows some insight into the channels of DM's effect on total health expenditures. As mentioned above, the estimates excludes (most) long-term health care expenditures, as the MEPS excludes institutionalized individuals from the survey population.

This approach allows me to estimate the effect of Medicaid DM on individuals with targeted diseases in states (counties) with programs. Difference-in-difference models require that secular trends in the outcome variable health expenditures) for non-treated individuals are not systematically related to (correlated with) the timing of program implementation. Although this

[28] To estimate the effect of DM intervention on DM enrollees who *actually* receive significant levels of DM interventions, it would be necessary to scale up the results in Section 2.5. Under the assumption that DM has no effect on "untreated" individuals enrolled in the DM program, one could multiply the treatment effect by the ratio $(total\ enrollees)/(treated\ enrollees)$. This would require an estimate of this ratio for the "average" DM program, which is currently unavailable.

cannot be tested directly, I check the robustness of the model with alternative control variables and alternative data samples in the results below. One form of systematic correlation that could potentially influence the model's results would be the presence of "spillover" effects. I assume that DM does not have an indirect effect on non-program participants.[29] Although these effects cannot be ruled out, I expect they are, at best, second-order in magnitude compared to the main effect of DM on program participants themselves. These models implicitly assume DM does not affect Medicaid enrollment or disenrollment. This assumption would be violated if DM attracts patients to apply for Medicaid, improves satisfaction with the Medicaid program, assists enrollees in maintaining their benefits, or otherwise increases (or decreases) enrollment for the target population. Finally, the identification strategy requires that the date of DM introduction ($DM_{it} = 1$) is not systematically correlated with Medicaid policy changes. Some policies are potentially troublesome, such as the movement of enrollees in or out of Medicaid Managed Care (MMC) plans. However, DM_{it} is constructed by matching survey respondents to DM programs via their disease, dual eligibility, and county of residence. Thus, there are typically other individuals *in the same state* with $DM_{it} = 0$, yet who may be subject to the policies of concern, including MMC. In any case, a key test will be the robustness of the results to the inclusion of state-year and other interaction term effects.[30]

As is common in health economics, the outcomes variables in this study prove challenging because health expenditures (i) are strictly nonnegative, (ii) a large fraction of observations are zero, and (iii) the distribution of nonzero observations is highly skewed. Among continuously enrolled Medicaid recipients in the 2007 MEPS, the top decile accounted for 79 percent of

[29] Examples of potential spillover effects include the following: (1) General equilibrium effects on health care prices (which may be at work in the Florida example on p. 19) could alter health care utilization in the broader economy. (2) DM interventions that educate health providers may influence their decision-making with non-program participants. (3) Participants may pass along information to non-participants.

[30] In particular, see the discussion related to Table 4 (columns 2 and 3, p. 40), Table 5 (p. 41), and Table 8 (p. 45).

Medicaid expenditures (accounting for complex survey design of the MEPS). The summary statistics in Table 2 present the fraction of observations with zero expenditures, by category. Aggregating to quarterly data only partially smoothes the data inter-temporally and does not remove its kurtosis.

In light of these issues, my primary specification is a two-part model, with a generalized linear model (GLM) in each stage.[31] The first stage models the probability of any expenditure conditional on the independent variables, $\Pr(y_{it} > 0 | DM_{it}, z_{it})$, using a Bernoulli distribution and a probit link function ($g(x) = \Phi^{-1}(x)$ in equation 1).[32] Here, z_{it} refers to the remaining independent variables.

The second stage models the outcome conditional on positive expenditures, $E(y_{it} | y_{it} > 0, DM_{it}, z_{it})$. I choose to use a GLM model with a gamma distribution and a log link function $(g(x) = \log_e(x))$. This formulation with the log link function is essentially the nonlinear least squares estimator (NLLS) from Mullahy (1998, p. 260), and use of the gamma distribution was first used in this context by Blough et al. (1999). Alterative functional forms for the second stage of the model have been compared by Manning and Mullahy (2001) and Basu, Manning, and Mullahy (2004). The gamma regression model with the log link was shown to fit health expenditure data and, relative to the alternatives, was more robust to alternative data generating processes in Monte Carlo simulations. I used the model tests proposed by Manning

[31] The use of two-part models in health economics dates at least to the Rand Health Experiment (Duan 1983; Duan et al. 1983, 1984; Manning et al. 1987) and is widely used today. For example, Burns (2009) uses two-part models with the MEPS data set in her study on Medicaid Managed Care. Jones (2000) provides an excellent introduction to the topic. Two-part models are covered by econometric textbooks such as Cameron and Trivedi (2005) or Wooldridge (2002). Leung and Yu (1996) use Monte Carlo simulations to compare this model to the (Heckman) sample selection model. I did not consider the sample selection model for this research because there is no apparent exclusion restriction that could be used to fit the sample selection part of the model.

[32] The Bernoulli distribution is the canonical link for the logit function. Given the similarity of the probit and logit functions, this distribution should usually behave similarly with either link function.

and Mullahy (2001, pp. 471-472) and Deb, Manning, and Norton (2010); test results suggested that this link function and distribution would work well for the MEPS data.[33]

As discussed by Duan et al. (1983, pp. 118-119, 1984, p. 286), Jones (2000, pp. 285-292), and Cameron and Trivedi (2005, pp. 544-555) , this model allows the censoring mechanism and the outcome to be modeled using separate processes, which is appropriate if the researcher believes that one process explains the decision to seek medical care and another process determines the consequent expenses. As such, the estimated effects of DM in stage 1 and stage 2 of the model are interesting in their own right.

Furthermore, the estimates from the two stages of the model can be combined to calculate the effect of DM on the (observed) dependent variable as:

$$\mathrm{E}(y_{it}|DM_{it}, z_{it}) = \Pr(y_{it} > 0|DM_{it}, z_{it}) * \mathrm{E}\,(y_{it}|y_{it} > 0, DM_{it}, z_{it}) \qquad 2$$

Accounting for the panel data structure, I fit the GLM models in both stages with a generalized estimating equation (GEE) with an exchangeable within-individual correlation structure and provide robust standard error estimates.[34]

The DM treatment variable (DM_{it}) is dichotomous, thus marginal effects are calculated for the discrete change of the variable using the following equation, based on the estimated parameters from the two regressions:

$$\begin{aligned}\frac{\Delta\,\mathrm{E}(y_{it})}{\Delta DM_{it}} = \widehat{\Pr}(y_{it} > 0|DM_{it}, z_{it}) \\ * [(\hat{y}_{it}|y_{it} > 0, DM_{it} = 1, z_{it}) - (\hat{y}_{it}|y_{it} > 0, DM_{it} = 0, z_{it})] \\ + (\hat{y}_{it}|y_{it} > 0, DM_{it}, z_{it}) \\ * \left[\widehat{\Pr}(y_{it} > 0|DM_{it} = 1, z_{it}) - \widehat{\Pr}(y_{it} > 0|DM_{it} = 0, z_{it})\right]\end{aligned} \qquad 3$$

[33] For a Box-Cox test for total expenditures and the baseline set of treatment and control variables, the 95-percent confidence interval for the power parameter ranged from -.0267 to -.0224, which was close to zero, indicating a log link function is a close fit. The modified Park test fell between the ranges recommended for the Poisson distribution and the gamma distribution. A modified Hosmer-Lemeshow Test for linearity indicates a problem only for outliers at the far right tail of the distribution. Robust standard errors are used throughout the chapter to adjust for heteroscedasticity.

[34] For more on these models, see Liang and Zeger (1986), Zeger, Liang and Albert (1988), or some econometrics textbooks (e.g., Cameron and Trivedi 2005, sec. 16.4.1 and 23.2; Wooldridge 2002, sec. 15.8/19.6). Models were estimated using Stata.

Here, $\widehat{\Pr}(y_{it} > 0)$ is the estimated probability of positive expenditures and $\hat{y}_{it}$ is the estimated expenditures conditional on positive expenditures (other dependent variables suppressed). Note that the main DM_{it} variable is based on matching an individual's characteristics and the design of DM programs in their state (if any), so the DM variable is not an interaction term.[35]

Because these models are nonlinear, the marginal effect will vary across observations, depending on the realization of the other RHS variables (z_{it}). I calculate the effect of DM_{it} using two methods. First, I present the marginal effect of DM_{it} at a specific value of the other dependent variables: the mean, $\bar{z}_{it}$. Second, I calculate $\frac{\Delta \, \mathrm{E}(y_{it})}{\Delta DM_{it}}$ for each observation and then take the average across the treated observations to calculate the "average treated effect on the treated" (ATET). These estimates can be bootstrapped to obtain standard error estimates using a "block bootstrap" procedure to account for the panel data.[36]

In equation 1, $\hat{\delta}$ is the primary parameter of interest, as it identifies the effect of the presence of a DM program on the Medicaid enrollees who would have been eligible for the program in period t. Unbiased identification relies on exogenous timing in the introduction of state DM programs at the quarterly frequency. If one state implements a DM program, we might expect a decline (or smaller increase) in y_{it} among DM-eligible enrollees, relative to similar individuals in states without a DM program or other individuals in the state who are ineligible (e.g., a dual-

[35] In a few alternative specifications, the DM_{it} variable is an interaction term based on other dependent variables. In these cases, marginal effects are calculated for each individual as the double difference proposed by Ai and Norton (2003). Specifically, if $DM_{it} \equiv x_1 x_2$, then (subscripts and z_{it} suppressed):

$$\Delta \, \hat{y}/\Delta x_1 \Delta x_2 =$$
$$(\hat{y}|\mathrm{y} > 0, x_1, x_2) * \left[\left(\widehat{\Pr}(\mathrm{y} > 0|x_1 = 1, x_2 = 1) - \widehat{\Pr}(y > 0|x_1 = 1, x_2 = 0)\right) - \left(\widehat{\Pr}(\mathrm{y} > 0|x_1 = 0, x_2 = 1) - \widehat{\Pr}(y > 0|x_1 = 0, x_2 = 0)\right)\right]$$
$$+ \widehat{\Pr}(y > 0|x_1, x_2) * \left[\left((\hat{y}|\mathrm{y} > 0, x_1 = 1, x_2 = 1) - (\hat{y}|\mathrm{y} > 0, x_1 = 1, x_2 = 0)\right) - \left((\hat{y}|\mathrm{y} > 0, x_1 = 0, x_2 = 1) - (\hat{y}|\mathrm{y} > 0, x_1 = 0, x_2 = 0)\right)\right]$$

This marginal effect is averaged across treated individuals to calculate an ATET.

[36] Due to computational limitations – each estimate would require multiple days or weeks to run – bootstrapped standard errors are provided only for a limited number of specifications in this version of the chapter. The reported results and other preliminary inquiries found the statistical significance of the bootstrapped standard errors to be intuitive in light of the standard errors from the first and second stages of the model.

eligible enrollee). As such, for expenditure-reducing DM programs, the hypothesized sign of $\hat{\delta}$ is negative in a single-equation model. However, the predication is not so clear in the two-part model, as it is possible for $\mathrm{E}(y_{it})$ to decrease even though $\Pr(y_{it} > 0)$ increases, due to the effect of DM on $\mathrm{E}(y_{it}|y_{it} > 0)$. Indeed, this would be the case if DM lowers the likelihood of expensive hospitalizations via increased used of low-cost preventative health care such as office visits or prescription drugs among low-utilization Medicaid recipients.

2.5 Results

2.5.1 Summary Statistics

The main data sample contains 40,415 individuals who received Medicaid at any time during the MEPS, constituting 289,452 person-quarter observations. Most individuals have two calendar years (eight quarters) of data; the major exception is that the first and last MEPS panels have one year (four quarters) of coverage due to the overlap of the MEPS sampling design. Table 2 presents summary statistics for this sample, grouped by the presence of at least one of the five chronic diseases mentioned above, age less than 18, and dual-eligibility with Medicare.[37] In this table, means are presented unweighted, and thus the table reflects the MEPS's oversampling of minorities and low-income households (e.g., percentages of individuals by race and education do not match those of the entire Medicaid program).

In the sample, 9,503 individuals have one of the "top-5" chronic conditions mentioned above, about one-third of the adults and one in seven children. Asthma is the most common chronic disease (149 diagnosed per 1,000 people) and the only disease common among children. Among adults, asthma, diabetes and CHF are more common than either COPD or CAD. Over one quarter of chronically ill dual eligibles have diabetes. As discussed above, I focus my analysis on the "top-5" chronic diseases most commonly targeted by DM programs. There are other common

[37] The categories are mutually exclusive; I cut the sample by (1) having a chronic-illness, then by (2) dual-eligibility, then by (3) age group. (i.e., "Medicaid adults" does not include dual eligibles.)

chronic conditions – cancers, mental illnesses (especially development disabilities), and other diseases – that are present in this population but receive less focus from DM. See Kronick, et al (2009, pp. 11-15) for more information on the prevalence of diseases in Medicaid. [38]

In the MEPS, one observes some turnover in insurance status as some individuals enter or exit Medicaid enrollment during the sample period. On average, Medicaid insurance covers this sample 76 percent of the time they are in the MEPS and about half of the individuals are continuously enrolled in Medicaid. Individuals with a chronic disease are more likely to be continuously enrolled in Medicaid.

Panel B of Table 2 presents average quarterly Medical expenditures for these groups. Individuals with at least one of the five chronic diseases have, on average, significantly higher medical expenditures than their counterparts (for example, $1,736 versus $679 for Medicaid adults), and are more likely to have positive expenditures in a particular quarter (78 percent versus 51 percent). Medicaid pays for all medical expenditures for over half of Medicaid recipients and, on average, pays 80 percent of Medicaid-only enrollee's total expenses; on the other hand, Medicaid only pays 35 percent of total expenditures for dual eligible with chronic conditions while Medicare pays the majority of costs (not shown in table). Even though inpatient hospitalizations are relatively infrequent among Medicaid recipients with chronic conditions – only 6.7 percent, 1.8 percent, and 9.7 percent of chronically ill Medicaid adults, children, and dual eligibles, respectively, have any inpatient expenditures in a typical quarter – these rare events are associated with very high expenditures, such that the inpatient expenditure category accounts for 34 percent of total expenditures ($633 per person per quarter, on average). Prescribed medicines are quite common and are the second largest spending category (24 percent of total costs), followed by office-based provider visits (18 percent). Emergency Department

[38] Their report uses full ICD-9 diagnosis information from administrative datasets, making it more reliable than the MEPS. Differences with the MEPS may occur because of sampling and because the MEPS excludes institutionalized individuals, a group with high rates of chronic diseases. The authors also emphasize the prevalence of particular 2- and 3-disease co-morbidity pairs.

visits are more common than inpatient stays but account for a small fraction of total expenditures (2 to 6 percent, depending on the category). Among adults, individuals with diabetes and COPD are hospitalized for their disease at the highest rates, followed by CAD and Asthma. Diabetes is associated with the largest number of hospitalizations. Asthma is the second most commonly diagnosed disease in hospitals due to its status as the most common disease.

The final rows of Table 2 (Panel B) present information on Medicaid DM coverage. Please note that the columns of this table do not separate treatment or control groups, as treatment varies by locality over time *within* groups. I find reasonably large numbers of Medicaid adults and children are treated in the MEPS panel. On average, an individual lives in a state/county with at least one DM program for 33 percent of person-month observations. For adults and children with chronic diseases and Medicaid insurance (excluding duals), 21 percent of observations have a DM program in their state/county that targets their chronic disease(s). This is a weighted average of the DM coverage for the years 1998-2007.

Figure 7 plots a DM coverage over time for adults and children with chronic diseases and Medicaid insurance. As expected, DM program coverage at the individual level tracks the state-level trends identified in Figure 1. In early 1998, the beginning of the MEPS sample, less than four percent had any DM program in their state/county. By the end of 1997, this figure increases to 62 percent. Most, but not all, individuals (with a top-5 chronic disease) have a program *that targets their disease* when there is a program is in their state/county (e.g., asthma patients who live in a state/county with an asthma DM program). At the beginning of the sample, less than one percent of MEPS respondents had a DM program that matched their top-5 chronic disease(s), but this figure increases to 52 percent at the end of the sample. My research indicates that very few DM programs included eligibility for dual eligibles, and thus less than four percent of dual eligibles are matched to a DM program in their state/county at any point in the sample. This is expected, given the states' financial incentive structure (see above, p. 30).

My DM coverage indicators can be combined with the MEPS's complex survey weights to create national estimates of DM coverage. In Table 3, I present the *weighted* number of people who are matched to a DM program using my DM_{it} coverage variable (see above, pp. 33-35). The MEPS survey weights produce nationally representative estimates. Nearly all the growth in DM coverage occurs during the time frame covered by this study, as can be seen in Figure 8. The number of people matched to a DM program starts from a negligible level in 1998 and increases to 3.76 million individuals in the final quarter of 1997. In the final time period, 1.79 million (48 percent) of these observations are adults with Medicaid insurance (not dual eligible) and 1.89 million are children (50 percent), while less than 100,000 are dual-eligible adults. This represents 38 percent of adults in Medicaid with a top-5 disease and 48 percent of children with a top-5 disease (columns 5 to 7). These 3.76 million individuals are 7.3 percent of the nation's Medicaid recipients.

2.5.2 Main results – The effect of DM on medical expenditures

Table 4 provides the results of the difference-in-difference analysis on the effect of Medicaid DM programs on (i) total medical expenditures and (ii) expenditures paid by Medicaid. As discussed above, the models use pooled MEPS data from 1998 through 2007 and the two stage-specification outlined in section 2.4. For each outcome, Panel A presents the estimated marginal effects of DM on the probability of positive expenditures (the first stage of the two-part model) and Panel B presents the marginal effect of DM on the level of expenditures conditional on positive expenditures (the second stage). Combining these results, the marginal effect on total expenditures at the mean and the average treatment effect for the treated observations (calculated according to equation 3) are reported in Panel C. For each dependent variable, I present the effect of DM with four specifications of control variables. The following tables will present variations upon this base model, using alternate control variables, data samples, models (e.g., OLS instead of two-stage GLM), and outcome variables.

In Table 4, column 1 reports the results from the equation 1, which includes unrestricted state and time (year-by-quarter) fixed effects and the other control variables discussed in Section 2.4.2. This column reports an increase in the probability of having a medical expenditure of about 1.5 percentage points when an observation is matched to a DM program. This estimate is statistically significant at the 90 percent level. This result is consistent with the hypothesis that some chronically ill Medicaid patients were receiving no health care services and that DM induced these patients to receive recommend levels of "preventative" care (e.g., prescription drugs, regular doctor's office visits, and so on). DM may still lower expenditures to the extent that DM causes patients to switch from zero expenditures to having some expenditures on cost-efficient services.[39]

For patients with positive expenditures, the point estimate indicates that DM is associated with a decrease in expenditures of about $87 per quarter. However, the second stage regression lacks statistical power to estimate this effect precisely: the p-value for this estimate is 0.149 and the 95 percent confidence interval ranges from -$204 to +$31. Thus, the results from the two stages of the model work in opposite directions: the first stage indicates an increase in expenditures while the point estimate in the second stage indicates a decrease (although it may be positive). The results from the first and second stages of the model are combined to obtain a point estimate for the marginal effect of DM on total expenditures using equation 3. At the mean, the model estimates that DM reduces total expenditures by about $33, so there is some evidence that the (negative) second stage impact of DM may outweigh the (positive) impact in the first stage.[40] The control variables for treated observations are not centered around the sample mean, so the average treatment effect on the treated (ATET) differs from the marginal effect at the mean. The ATET calculations show that expenditures are reduced by $97. Bootstrapping indicates this result

[39] An alternative theory is that DMOs may benefit from a small increase in expenditure for individuals without medical spending. For example, increasing expenditures above $0 may reduce the probability of an exit from Medicaid and/or the DM program.

[40] The mean of the dependent variable is 53.9 percent in the first stage and $1,395 in the second stage.

is imprecisely estimated (standard error 72.4). In Table 2, the weighted average of total medical expenditures for chronically ill Medicaid enrollees (columns 1 and 2) is $1,045 ($1,709 including dual eligibles, columns 1 through 3). Thus, $97 corresponds to a 10 percent decrease in total expenditures.

In Appendix Table B1, I present the GLM regression coefficients and marginal effects for all the control variables of Table 4 (columns 1, 3, 5, and 7). The marginal effects for the controls are generally as expected: individuals tend to have higher expenditures if they are insured by Medicare, insured as a dual eligible, have one of the top-5 chronic diseases, are male, are aged less than ten, are elderly, and/or are not in a racial/ethnic minority. This level of detailed reporting is suppressed for other regressions for the sake of brevity.

Columns 2 and 3 add controls for potential within-state variation over time by adding state-specific time trends and unrestricted state-by-year interaction dummies, respectively.[41] The results are robust across specifications with the point estimates remaining similar in magnitude. Second stage estimates continue to lack statistical power. The robustness of the result between columns 1 and 3 is very encouraging, as the state-by-year fixed effects control for any potential policy changes implemented at the state level during the sample. Here, the effect of DM is identified by the within-state complexities of DM coverage by county, disease, and/or insurance status, not simply the state-level timing of the introduction of DM. For example, these specifications may better control for statewide changes in structure of MMC plans.

One potential reason I do not find large DM treatment effects is that the DM treatment variable may be imprecise, in the sense that the DM programs in the sample are heterogeneous and some programs may have a small effect while others have a larger effect. It is not unreasonable to expect low-intensity interventions to be less effective at lowering medical

[41] For columns 2 and 6, year fixed effects and quarter-of-year fixed effects are substituted in lieu of the full array of time fixed effects (θ_t) to ease the computational burden. Analogous substitution for year and quarter-of-year fixed effects in column 1 had a trivial effect on the estimates and standard errors.

expenditures or improving health. (Coleman et al. 2010) In column 4, I use an alternative, *strict* definition for DM, which drops some of the programs with less intensive interventions or coverage (see Appendix A). The results indicate that programs meeting these criteria tend to have a larger impact: the point estimate for the second stage doubles in magnitude (approximately) and the estimate becomes statistically significant. (Note that the point estimate for the first stage effect is smaller.) The ATET is a $216 reduction in quarterly expenditures after the introduction of a DM program. This indicates that at least some DM programs may be having an effect on total expenditures.

Columns 5 through 8 repeat this exercise for medical expenditures paid for by Medicaid, which is perhaps the outcome variable of more concern to program administrators. The results from the first stage of the model indicate that DM increases the probability of having a Medicaid insurance claim in a quarter by 3 to 5 percentage points. As before, I find a statistically insignificant treatment effect in the second stage with a negative sign. The result remains stable when state-year fixed effects are included in the model. This second-stage effect is smaller and is not large enough to offset the first-stage effect (except in column 8, which uses the strict DM definition). To conclude, these main results fail to find statistically significant evidence that DM lowers Medicaid expenditures.

Table 5 examines the robustness of the previous results to the inclusion of various arrays of control variables. In this table, I address the concern that DM targets individuals with particular chronic conditions, for whom health care expenditures may trend differently than for the Medicaid population as a whole. Therefore, I introduce disease specific time trends (column 1) and unrestricted disease-time fixed effects (columns 2, 3, and 4) into the model to control for observable differences in medical expenditures by disease over time. All four columns control for expenditure levels by including disease-insurance group interaction dummies and I include insurance group-year fixed effects in column 4.

The results indicate that DM may lower medical expenditures. Compared to Table 4, both the first stage and second stage results are responsible for this finding: Panel A shows a more modest increase in the probability of medical spending (to a trivially small level) than was found above. In Panel B, the second stage results indicate that DM may decrease expenditures (for those with spending) as much as $100 per person per quarter at the mean, or $120 to $180 for the treated population. The differences between this table and the results presented above is consistent with a scenario where secular increases in expenditures are growing faster-than-average for the individuals with diseases targeted by DM.

Table 6 examines the robustness of the main results (Table 4) by using alternative data samples. Remember that the results thus far have used data for all individuals who "touch" the Medicaid program while they are in the survey and I use all available observations (quarters) for the individual, even if they leave the Medicaid program. First, I repeat the analysis while dropping observations (quarters) from the dataset where the individual is not enrolled in Medicaid. Results would differ if the control variables in the baseline regression do not properly control for utilization differences before/after the patients enroll in Medicaid. The effect of DM, measured in the first and second stage regressions are similar to the results above and the estimated ATET is a net decrease of $93.

Columns 2 and 3 present results when the data is sampled as to only include the individuals continuously enrolled in Medicaid (49 percent of the main sample). Thus, I provide a specification that may (i) eliminate potential concerns of attrition [selection] bias and (ii) identify the effect of DM on the policy-relevant population of patients (in the sense that the effect of DM on people who leave Medicaid matters relatively less to Medicaid policymakers). However, this "stable" population of enrollees can differ from Medicaid patients with short spells of Medicaid coverage on observable characteristics (e.g., SSI disability patients often have stable insurance coverage) and also because insurance coverage "churning" provides less opportunity and incentive for preventative health care. (J Currie 2000, p. 1061; Fairbrother, Emerson, and

Partridge 2007) Column 4 uses an intermediate-sized sample, include those who are enrolled at least half the time they are in the MEPS (78 percent of the main sample). Results in these three columns are similar in magnitude to those found above, demonstrating some robustness. However, there is less statistical precision (presumably from the smaller sample size): only with the strict definition of DM are second stage results statistically significant (in column 2, a $141 decrease in total expenditures).

The empirical strategy in this chapter relies on fixed effects to control for secular trends in total medical expenditures. One concern is that underlying trends in expenditures for the potential DM-eligible population differ from the trends in expenditures for other sub-populations (e.g., relatively healthy Medicaid children). By dropping Medicaid patients without chronic conditions, for example, the estimated fixed effects may more appropriately control for trends in expenditures for the population of interest, although I lose power because of the smaller sample size. This is an alternative to using disease-time controls (as in Table 5). Column 5 in Table 6 presents results when I limit the data to the sub-sample of 9,503 individuals who have a "top-5" chronic condition and column 6 limits the sample to only the adults who receive Medicaid (i.e., those over 18 years of age). The standard errors are significantly larger than the main results but the point estimates are relatively similar in magnitude: the implied ATET from combining the first and second stage results are $101.90 and $92.94 reductions in medical expenditures, respectively, compared to a $97.29 reduction in Table 4 (column 1).

In the final columns of Table 6, I run models with additional data (compared to the primary data set), adding all MEPS observations for individuals who have at least one of the "top-5" chronic conditions. This could improve the estimation, as the secular trend in time fixed effects for treated individuals may be more similar to the trends for the chronically ill (than it is for an average Medicaid recipient). Furthermore, the much larger sample size increases power (as long as it does not introduce bias). To control for differences across insurers, I include insurance-by-disease interaction dummies in all regressions. One concern with this setup is that I do not have

any information to identify which chronically ill individuals do or do not receive DM outside of the Medicaid program, and thus $DM_{it} = 0$ for non-Medicaid observations. However, it is likely that a significant number of privately insured chronically ill individuals do, in fact, receive DM, due to large increases in DM coverage in the private sector over the period. (Matheson, Wilkins, and Psacharopoulos 2006) If DM coverage is widespread in the private sector and DM decreases costs, the trend in medical expenditures for non-Medicaid chronically ill individuals may increase more slowly than it does for comparable untreated Medicaid enrollees, understating the effect of Medicaid DM in my regressions.

Columns 7 and 8 of Table 6 present the results with this larger sample (over three times as large), indicating a larger reduction from DM in the second stage regression than was found previously. In column 9, I use the larger data set and include a large array of additional fixed effects to control for differences in the levels and trends of medical expenditures by disease (disease-by-year and disease-by-quarter-of-year fixed effects) and insurance (disease-insurance dummies and insurance-by-year fixed effects). The results are consistent with those found previously: DM increases the probability of a medical expenditure in a quarter by 1 percentage point and, for those with expenditures, reduces the level of expenditures by $138. Combining the results, DM appears to have reduced expenditures by $133.08 for the treated individuals ($82.54 at the mean).

Next, I check the decision to use a two-part model based on two GLM regressions. Table 7 presents alternative empirical models, using the control variables from the first regression presented in Table 4 (above). The first two columns reproduce the first and second stages of the model using simple ordinary least squares (OLS) regressions. Columns 3 and 4 re-run the model with the complex survey weights that are provided with the MEPS data for each individual. The results in columns 1 through 4 have similar signs and similar magnitudes to the results found above, which reassure that the results are not dependent on the functional form of the GLM models. The columns 6 and 7 present marginal effects for probit models on the probability an

individual has quarterly expenditures in the quarter over two arbitrary cutoff points. Point estimates indicate that DM has a positive impact on the probability of an individual spending $1,000 or more in a quarter, but reduces the probability of spending over $5,000. This result is potentially consistent with the findings above, which indicated DM was associated with an increase in expenditures at the far left tail of the distribution (stage 1) but decreasing expenditures for higher-cost individuals (stage 2). In Appendix C, I discuss an alternative model: the tobit model. Under the assumptions of a tobit model, individuals with zero expenditures are not viewed as having "actual" zeros, but are instead modeled as having a "latent" demand for negative expenditures. This "latent" demand for health care is interpreted as a censored dependent variable. The appendix presents results from the tobit model and explains why the results may differ from the two-part model.

Unfortunately, the model has difficulty converging with the full array of potential fixed effects that I have discussed in this section. That is, the data does not have the power to identify state-time fixed effects, time fixed effects, disease-time fixed effects, and/or insurance-time fixed effects in the same regression while at the same time using the (preferred) two-stage GLM models. However, it is possible to include large arrays of control variables with simple OLS models without the models becoming computationally burdensome. The results of this exercise are presented in Table 8. The first columns reproduced specifications from above, for comparison. OLS estimates are similar to the results found above. The point estimates continue to indicate that DM lowers medical expenditures, but results are estimated without precision (nothing is significant at the 95 percent level).

The remaining columns of Table 8 add various combinations of interaction effects from the variables for state of residence, time (year or year-quarter), disease, and insurance group (e.g., dual eligibles are an insurance group). Hence, the effect of DM is identified by the within-state variation in DM_{it} across diseases, insurance groups, and/or counties. The point estimate is robust to these control variables, with the estimated effect of DM reducing quarterly expenditures about

$95 per person in each of the specifications (expect for column 10, which attempts to include many interaction effects at once).

I also ran models with a simple triple-difference-in-difference model where the DM independent variable equaled one for individuals who simply had a chronic disease and had a DM program for their insurance group in their state/county (tables not shown). The point estimates indicated a large decrease in expenditures.

2.5.3 The effect of Disease Management by target disease and for sub-populations of interest

These difference-in-difference models can also identify the effect of DM for each of the target diseases by introducing additional treatment variables into the regression. The results of this exercise are presented in Table 9. As before, the main "DM" independent variable equals one if an observation is matched to a DM program in the individual's state/county based on their chronic disease(s) and insurance, and zero otherwise. The "Asthma DM" variable equals one if this match was between the individual's diagnosis for asthma and the program targeted asthma (and likewise for the other "top-5" diseases). These treatment variables are included for the "top-5" diseases. The first column includes disease-specific time trends and the following two columns use disease-time interaction effects, similar to previous tables. Column 3 uses the larger data sample, including people who never enter the Medicaid program (i.e., the same data as in the final columns of Table 6, above).

Because the "DM" variable equals one whenever one of the disease-specific DM variable equals one, the net effect of DM can be taken from adding the marginal effect of a disease-specific DM variable to the marginal effect of the main "DM" variable.[42] For example, the implied marginal effect for someone with only asthma would equal $-37.46 - 63.66 = -101.12$

[42] The six treatment variables are not colinear because some individuals have more than one top-5 chronic condition that matches a DM program in their state/county.

dollars (in column 1, panel C). In Figure 9, I plot the results of this simple arithmetic, for easy comparison across diseases and across specifications.

The first stage results in Table 9, indicate that DM increases the probability of medical expenditures for COPD, Diabetes, and CHF patients. This increase is particularly large for COPD patients. In the second stage (Panel B), the results indicated that DM has a negative effect on expenditures – consistent across specifications – for asthma, CHF, and CAD. In Panel C, the marginal effect of DM on expenditures in the two-part model is a reduction in costs for CAD, CHF, and asthma patients. For COPD, the effect is mixed (with the first and second stage working in opposite directions for two specifications) while diabetes is the only disease where DM increases medical expenditures in both stages of the model.

In Table 9, the negative sign on the main DM_{it} treatment variable indicates that DM causes expenditures to increase (or decrease less) for individuals with more than one chronic condition, relative to individuals with only one top-5 condition treated by a DM program.[43] This result is explored further in Table 10, where I interact the DM_{it} treatment variable with a dummy variable that equals one if the individual has more than one top-5 disease. Using this interaction term as the treatment variable (Column 1), I find that DM for this group of individuals is associated with a small, positive change in expenditures. In the second column, I include the DM_{it} and comorbidity variables, in addition to the interaction term, and calculate the average

[43] For expositional purposes, consider the treatment effect in Table 9, column 1 for patients that may have asthma and/or CHF. From panel C, we calculate the effect of DM is $(-\$37 - \$64) = -\$101$ for patients with asthma, $(-\$37 - \$33) = -\$68$ for patients with CHF, and $(-\$37 - \$64 - \$33) = -\$134$ for individuals with both asthma and CHF. This result could have been calculated with the following equation:

$$y = -101\ (asthma\ DM) - 68\ (CHF\ DM) + 37\ (asthma\ DM\ \boldsymbol{and}\ CHF\ DM)$$

Note that the coefficient on the third term is simply the marginal effect for DM_{it} (from Table 9) with the opposite sign.

incremental effect for the treated individuals.[44] The average incremental effect for the treated individuals is positive and large ($491), again indicating that there are not costs savings from DM among the population with comorbidities.[45] In the third column, I introduce a variable that equals one if the individual is matched to a DM program in their state/county using more than one disease. Results are similar with this alternative variable.

There is one confounding factor to be aware of when interpreting these results in Table 10. One concern with the construction of my main DM_{it} variable is that some individuals may be assigned $DM_{it} = 1$ when, in fact, they actually are not enrolled in a DM program. Or, if they are enrolled, they may not receive many DM interventions (e.g., the low-risk group in Chapter 3). There is reason to believe, however, that individuals with comorbidities would be *more* likely to get DM interventions than someone with only one disease (even though $DM_{it} = 1$ in both cases). That is, I would expect fewer "false-positives" in my DM_{it} variable construction for the comorbid population. (This cannot be tested in the data.) Thus, the estimated effect in Table 10 could potentially confound (1) differences in the effect of DM with (2) differences in the likelihood of receiving intensive DM interventions between the population with one disease and the population with more than disease.

One concern about these results is that there could be differential time trends: secular cost growth for comorbid individuals is growing faster than it is for those with zero or one top-5 condition, biasing the results. Thus, in columns 4 through 6, I replicate the results with an array of year fixed effects for the comorbid individuals, disease-year interaction term effects, and disease-

[44] Given the formula for calculating treatment effects for interaction term treatment variables (see footnote 35), it is only appropriate to present average incremental effects for the treated individuals in the results. The computational burden of these calculations prohibits the estimation of bootstrapped standard errors at this time.

[45] This cost increase for comorbid treated individuals can be consistent with an average cost decrease for all treated individuals given that there are relatively few individuals with more than one disease *and* a DM program. Of the 264,018 observations in the regression, 5.8 percent have more than one top-5 disease. Just 11 percent of these observations are matched to a DM program, or 0.66 percent of the entire sample.

quarter-of-year (seasonality) interaction term effects. The results are *very* similar to those found in the first two columns. I conclude that secular trends in expenditures for comorbid individuals is not the reason why I find DM does not decrease costs for comorbid DM program enrollees.

In Table 10, I also introduced a variable into the baseline regression to measure the effect of DM for dual eligible enrollees (column 7 and 8). This is an interaction term between the main treatment variable, DM_{it} and the dual eligible insurance indicator (one of the control variables included in all regressions). Because so few dual eligibles receive DM due to the reasons discussed above, the average incremental effect for dual-eligible DM should be interpreted as having little statistical power. The estimated effect for dual eligibles is of the opposite sign and much larger in magnitude to the main DM effect, indicating that it is unlikely DM decreases costs for this population.

2.5.4 The effect of Disease Management on disaggregated categories of medical expenditures

In this section, I further explore the impact of DM by disaggregating medical expenditures by type of service. The impact of DM should follow a logic model where program activities lead to short term outputs and outcomes, which only ultimately results in long-term outcomes. (Esposito, Taylor, and Gold 2009) The underlying logic of DM indicates that the effect of DM may work in opposite directions for difference categories of expenditures. For example, DM interventions might encourage the use of more prescribed medicines (e.g., asthma inhalers) or encourage patients to schedule their annual (screening) visit with their primary care provider. Increases in medical expenditures for these categories of care could lead to a net reduction in total expenditures if the medical care results in better-managed chronic diseases over the long term, ultimately causing decreases in spending for categories such as emergency department visits or inpatient hospitalizations. *A priori*, in a study design with limited statistical power, I expect that I will be more likely to detect an impact of DM for the categories most directly affected by DM in the logic model.

Table 11 and Table 12 disaggregate spending into mutually exclusive categories: (1) office-based medical provider visits, (2) prescribed medicines, (3) emergency department events, (4) inpatient hospital events, (5) outpatient hospital events, (6) home health, (7) dental and vision, and (8) other medical expenses. Table 11 and Table 12 provide results for total expenditures (all payers) and Medicaid expenditures, respectively, and the model specification corresponds to those in columns 1 and 5 of Table 4.

Columns 1 and 2 begin with what are likely the most proximate categories to DM, office visits and prescribed medicines. In the first stage, there is an increase in the probability that an individual has at least one office visit (paid by Medicaid) of 1.5 percentage points (standard error .0064). For prescribed medicines, there is a 5.4 percentage point increase in the probability of an expenditure (paid by Medicaid). Thus, I find fairly strong evidence that DM has an effect on getting patients who are not receiving routine care into their providers office and taking prescribed medicines. To get some understanding of the financial size of this first-stage effect, we can pair the results with the summary statistics in Table 2. For example, Medicaid Adults with a chronic disease who are on prescribed medicines spend, on average, $672 per quarter (i.e., $464 divided by 69 percent) on their prescriptions. The 95 percent confidence interval for the first stage effect is 3.93 to 6.87 percentage points, implying that quarterly expenditures for this group increase somewhere between $26 and $46 dollars per person on average. The second-stage effect of DM on pharmaceutical and office visit expenditures is trivial.

The next three columns continue this analysis with the hospital-related expenditure categories: emergency department visits, inpatient events, and outpatient events. The results are modest. The estimated effect in the first stage of the model has an absolute value less than 2 basis points across all three outcome variables, payer (all payers and Medicaid), and definition of DM (regular or strict) and is actually positive for ED and outpatient visits. The decrease in the probability of a hospital stay is small and does not appear to represent significant cost savings. For exposition, I can demonstrate this finding by establishing a lower bound estimate. The largest

estimated effect (Table 11) is an 11.8 basis point reduction in the likelihood of an inpatient event; the 95-percent confidence interval from -37.4 to 13.8 basis points. In Table 2, the dual eligibles have the highest inpatient expenditures at $12,721 per quarter, conditional on an inpatient event ($1,235.17 divided by 9.71 percent). Therefore, the lower-bound savings estimate might be savings of as high as $47.55 per quarter ($12,721 times 37.4 basis points), although more modest savings is likely (about $15 near the point estimate of 11.8 basis points). The second stage of the model is imprecise and less robust – perhaps because sample sizes are very small – I hesitate to draw further conclusions.

Finally, for completeness, columns 6 through 8 provide results for home health, dental and vision, and other medical expenses. Perhaps unsurprisingly, I fail to find significant cost savings from DM in these categories.

All results are replicated with the *strict* DM definition in Appendix Table B2 and Appendix Table B3 and the results are replicated with the larger data set that includes non-Medicaid, chronically ill individuals in Appendix Table B4.

In order to verify the robustness and further understand the previous findings, Table 13 presents results for models – similar in form to Table 11 and Table 12 – where the dependent variable is the *number* of medical events for selected services categories. Linden (2006) argues that hospitalization utilization rates may be a superior outcome variable to hospitalization costs, as utilization rates are not affected by changes in unit costs for hospitalization services. As the dependent variable is now count data, the second-stage GLM models use a negative binomial distribution and a log link function (the equivalent to a Poisson regression). Comparing Table 11 to Table 13, negligible differences in the first stage of the model arise because the table with count data includes a small number of events that were recorded as having zero dollars in related expenditures. In column 2, the results show an increase in the number of prescribed medicines filled in a quarter (among those on medication, in the second stage regression). Thus, even though the previous results did not find an increase in pharmaceutical expenditures for patients on

medications, this table indicates that DM may help patients manage their disease with additional medications. The second-stage results for all other outcome variables – office visits, emergency department visits, and inpatient hospital visits – were statistically insignificant.

It may be expected that hospitalization rates cannot drop completely from DM interventions, as DM is not expected to mitigate the risk of hospitalization for reasons unrelated to the person's chronic disease (e.g., broken legs). Thus, I also examine the effect of DM on hospitalizations (emergency department, inpatient, and outpatient) where the patient received a diagnosed for one of the "top-5" chronic conditions. In columns 5 and 6, we find that the effect of DM on the probability of one of these events is very small and maybe actually positive. Column 6 limits the sample to those who are chronically ill. In column 7, I increase the sample size using chronically ill individuals without. Results for the three samples consistently indicate a small increase in hospitalizations, if anything.

In columns 8 and 9, I focus exclusively on asthma hospitalizations for people diagnosed with asthma. With the strict definition of DM, there is a negative effect (marginally significant at the five percent level) on the number of hospitalizations in the second stage. Perhaps this indicates that DM has some effect on reducing hospitalization for asthmatics who repeatedly visit the hospital. Hospitalizations with a diagnosis for the other four diseases (COPD, diabetes, CHF, CAD) were too infrequent to conduct a similar analysis.

2.6 Within-panel treatment variation

All of the results thus far have relied on an array of covariates to control for determinates of medical expenditures, including individual-specific variables that are known to be correlated with medical services use (for example, age, race, education, chronic conditions, and so on). However, this study could suffer from omitted-variable bias if DM is correlated with variables that are excluded (or unavailable) from the regressions; with nonlinear models, this concern

extends to the omission of interaction terms.[46] The concern of omitted variables also applies to the possibility of unobservable heterogeneity, such as individual preferences for healthy (or unhealthy) behaviors. To the extent that these omitted diseases are correlated with DM_{it} in the MEPS panel, it could bias the results presented above (see equation 1).

One standard method to address this issue to is to introduce an individual-specific fixed-effect that will control for all idiosyncratic, time-invariant aspects particular to the individual, including age, gender, and the presence of chronic conditions and co-morbidities, but also unobservable characteristics. However, in the MEPS, my Medicaid DM variable (DM_{it}) is a constant (always zero or always one) for the majority of individuals, and thus the impact of DM would be "differenced out" in the estimation procedure. In the main results, presented above, I do not include an individual fixed effect in the regressions, which allows the effect of DM to be identified via the whole panel's cross-program variation in DM treatment.

However for a subset of individuals, a DM program is introduced in their country/region while they are in the survey. For these people, I observe one to seven quarters of data without DM treatment, followed by a post-DM phase where a DM program is available in their state/county for the remainder of the panel.[47] Thus, I can use within-person variation in treatment status for these individuals to identify the effect of DM, controlling for an individual fixed effect, using models of the following form:

$$y_{it} = \mathrm{g}(\alpha + \delta\, DM_{it} + \beta\, X_{it} + \theta_t + \sigma_{\mathrm{i}}) + \varepsilon_{it} \qquad 4$$

The variables are the same as equation 1, except for the introduction of the individual fixed effect (σ_i) which precludes the need for control variable that are constant for an individual (λ_s, μ_p, and

[46] For example, the limitation of the MEPS data to 3-digit ICD-9 codes prohibits the use of detailed disease indicator variables, which means that some chronic disease were inevitably omitted from X_{it}. Fleishman and Cohen (2010) were given access to the MEPS's 5-digit ICD-9 diagnosis codes and used this data to fit prospective risk stratification models. They predict year-2 expenditures with higher precision than my results in section 2.5.2.

[47] There are 10,348 person-quarter observations for people who experience variation in DM while they are in the MEPS, about half of these observations are pre-treatment. In Table 4, over half of the "treated" observations are for people where $DM_{it} = 1$ the entire time they are in the MEPS.

some variables in X_{it}). In addition to measuring the average effect of DM across the post-intervention period, it is also possible to estimate a profile for the effect of the cumulative DM treatment over time by estimating regressions of the following form:

$$y_{it} = \mathrm{g}\left(\alpha + \sum_{\tau=1}^{7} \eta_\tau \, h_\tau(DM_{it}) + \beta \, X_{it} + \theta_t + \sigma_\mathrm{i}\right) + \varepsilon_{it} \qquad 5$$

Here, $h_\tau(DM_{it})$ is a dummy variable that equals one τ quarters after a DM program is first matched to individual i, and zero otherwise. These dummy variables equal 0 for all t if individual i does not experience variation in DM_{it}.[48] The coefficients η_τ estimate the change in expenditures that result from receiving DM in period $(t - \tau)$ and subsequent quarters. This effect is measured relative to the periods where i is untreated. The coefficients will be estimated less precisely for longer periods of treatment (e.g., η_6 and η_7) because this variable will be estimated with fewer people (by construction).

The first stage of the regression is modeled with a conditional logit model; it excludes individuals for whom quarterly expenditures are always zero or always positive during the survey. The second stage is an OLS regression with the logarithm of expenditures (conditional on positive expenditures).

The entire panel of individuals who receive Medicaid while in the MEPS are included in the sample, which improves estimation of the secular time fixed effects. However, it is important to remember that the DM effect is identified only by the small subset of individuals with variation in DM_{it} in both equations 4 and 5. As such, a primary concern with this model is that the DM effect for these 10,348 is not representative of the DM effect for Medicaid as a whole.

[48] That is, $h_\tau(DM_{it}) \equiv \begin{cases} 1 & \text{if } DM_{i,t-\tau} = 1 \text{ and } 0 < 1/t \sum_t DM_{it} < 1 \\ 0 & \text{otherwise} \end{cases}$ for the t periods individual i is in the panel. This dummy variable does not depend on the individual receiving DM in period subsequent to $t - \tau$, although many of the individuals do receive treatment continuously after DM is introduced. This would not be the case if DM is stopped shortly after its introduction in the state/county, or if the individual's insurance status changes. However, there may be a treatment effect in these periods, even after the individual exits the DM program (e.g., from patient education).

The regression coefficients from equations 4 and 5 are presented in Table 14. As was the case above, the first stage point estimate indicates an increase in the probability of medical expenditures after a DM program begins in the individuals' state/county. This DM effect is statistically insignificant in equation 4 and for the majority of values for τ in equation 5.

Turning to the second stage of the model, the estimates in columns 4 and 5 of Table 14 are very imprecise. For example, the 95 percent confidence interval of the effect of DM on medical expenditures ranges from negative 1.1 percent to positive 14.8 percent (column 3). The point estimates are all positive and do not trend downward. In any case, the estimates are too imprecise to draw strong conclusions. That said, the models do *not* find evidence that DM significantly lowers medical expenditures: the second stage estimates reject the hypothesis, for example, that DM lowers medical expenditures by 5 percent, 10 percent, or more in the final periods of the model. For ease of interpretation, the η_τ coefficients from equation 5 are also graphed in Figure 10. There is no obvious trend to support the hypothesis that DM increases expenditures initially but this is "paid back" by large decreases in expenditures in later as the length of time with DM accumulates. However, the 95 percent confident intervals are wide enough that such a trend may exist undetected.

There are several plausible interpretations for the failure to find evidence that DM lowers medical expenditures for the second stage point estimates in Table 14. First, the results could be due to statistical variation; the 95 percent confidence intervals in columns 4 and 5 do include negative values. Second, one could accept the apparent increase in expenditures as the causal effect of DM. It is certainly plausible that DM may encourage the use of some medical services but fail to address the underlying issues that drive (undesired) health care costs for the chronically ill. Third, DM may actually decrease expenditures (relative to a counterfactual) and the cost increases observed in Table 14 are the result of state-specific or disease-specific time trends that are correlated with DM provision but excluded from the regressions. (The models do adjust for CPI inflation and unrestricted time fixed effects). One might consider adding disease-specific

time trends, state-specific time trends, and/or other controls to the model. However, the small sample sizes and imprecision of the models discourages any further investigation with the MEPS data.

2.7 Discussion

Supporters of disease management have stressed the potential for DM to lower the cost of health care for chronically ill Medicaid enrollees. Thus, if DM has the intended effects, it offers Medicaid policymakers an attractive alternative to the status quo. However, empirical research to validate these claims has been elusive to date. Very few Medicaid DM programs have variation in treatment that could be used to establish a valid comparison of what would have happened to medical expenditures in the absence of the intervention. Medicaid policy is often set at the state level, providing treatment to the entire population of individuals in the state in a particular group (e.g., Medicaid SSI recipients with asthma). Or, individuals remain untreated for reasons that may invalidate comparison, such as when individuals are allowed to voluntarily opt-in to the treatment group. In the few cases where valid study designs exist, it is unclear if the results from one program have the external validity necessary to understand the effect of DM in other states or for other populations, given the wide heterogeneity in program designs across states and over time.

This chapter avoids these limitations by utilizing national data on health care expenditures. While DM rapidly spread across the country, states created rich variation in timing at which DM coverage was introduced to various disease groups. My DM treatment variable is much more complex than a simple, statewide pre/post variable; it is constructed by matching program inclusion criteria in each month to the characteristics of individuals in the MEPS, accounting for the evolution of program design within-states over time.

Exploiting this treatment variation, this study provides suggestive evidence that a "typical" Disease Management program may lower health expenditures for individuals in the targeted population. For a given treated population, other individuals in the same state with different

diseases provide reasonable estimates for underlying secular trends in the state while comparable individuals with the same disease in other states provide plausible counter-factual trends for a particular disease-insurance group-time cell.

However, this chapter has data limitations that merit attention in future work. DM consists of relatively high-intensity interventions and are, by design, targeted toward a small subset of high-risk or high-cost individuals. Given that the MEPS is designed to provide national estimates, it simply does not have high sample sizes for the subset of chronically ill Medicaid enrollees. The empirical results in this chapter are identified with relatively small samples and therefore the impact of DM is estimated imprecisely. In addition, the MEPS does not ask respondents questions about their participation in DM programs directly. Therefore, this study can only infer DM participation based on broad categories. As such, the results herein are more akin to "intent to treat" estimates than they are actual treatment effects. To estimate the effect of DM intervention on DM enrollees who *actually* receive significant levels of DM interventions, it would be possible to "scale up" the results under the assumption that DM has no effect on "untreated" individuals enrolled in the DM program. Given that some programs provide DM interventions to only a minority of the enrolled members, it is possible that actual treatment effects are significantly higher than the results stated in this study. DM-related costs for individuals in DM programs are not collected in the MEPS and not included in this analysis. Therefore, my results represent only one-half of a cost-benefit analysis. The financial savings observed in this chapter would need to be compared to an estimate of average DM program costs to determine the cost-efficiency of the programs or the net social financial benefits.

Both of these problems could be addressed by combining multiple-state administrative Medicaid enrollment data matched to insurance claims and records of DM interventions. Given large samples of high-cost, chronically ill Medicaid patients, this empirical methodology could be used to produce high-quality estimates. A researcher would be able to more precisely estimate the effect of DM on the main outcome variable, total medical expenditures (and, perhaps, include all

fixed effects and control variables in a single regression model). Furthermore, he or she could explore the effect of DM on outcome variables that occur in the MEPS infrequently. For example, one could estimate the DM effect on inpatient expenditures, separately for each disease. Administrative data could also be used to explore longer-term impacts of DM for a particular treated individual, perhaps controlling for individual fixed effects, beyond the two-year length of a MEPS panel.

Measuring the effect of DM on long-term health care expenditures is left to future work. Given the types of health behavior changes encouraged by DM programs, coupled with the fact that program participants are expected to have their disease(s) for life, it is possible for DM to have significant, long-term effects, particularly due to patient education and learned behaviors. Long-term effects would not be captured by the research methodology in this paper, given that identification relies on differences in the timing of DM implementation and that the MEPS does not collect data that would allow one to track the effect of DM in periods long past the DM interventions. Furthermore, most states have less than 10 years experience with Medicaid DM programs, and thus many long-term effects are yet to be observed.

The potential for long-term effects has important policy implications. I already mentioned that states lack an incentive to provide Medicaid DM services that would primarily reduce costs for other insurers, namely Medicare, in reference to the common practice whereby states tend to exclude dual-eligibles from program eligibility (p. 30). However, if DM has significant long-term effects, there would also be an externality for *current* Medicaid program participants (not just dual eligibles): state Medicaid agencies may under-provide DM services when benefits accrue to the individuals, to private insurers, or to Medicare after an individual exits Medicaid. It is possible that DM has more broad-based effects and Medicaid DM is currently targeted only at patients likely to yield very-fast, large reductions in Medicaid expenditures.

Similarly, this paper also leaves the measurement of "spillover" effects of DM on non-program participants to future work. The identification methodology implicitly assumes that such

spillover effects are small or non-existent. However, it is possible that Medicaid DM could drive system-wide improvements in the health care sector, through its influence on individuals (e.g., a doctor becomes better at managing the population of all patients under his or her care) and better understanding of how to manage chronic diseases (e.g., from data gleaned from DM information technologies).

The results in this study indicate that the introduction of a “typical” DM program for common chronic diseases may be associated with reductions in medical expenditures, although the effect is modest. These findings merit future research to validate the findings with higher precision and better explore the causal pathways by which DM affects Medical expenditures. Given the heterogeneity of DM program designs, it will be important for future research to identify "best practices" in Medicaid DM.

Chapter 3: Evaluation of the Georgia Enhanced Care Disease Management Program

3.1 Introduction

This chapter contributes to the literature by providing evidence on the health care cost efficiency of one Medicaid DM program in the state of Georgia: the Georgia Enhanced Care (GEC) disease management program. In this program, relatively low risk patients received a very limited set of interventions, while moderate and high risk patients (constituting approximately one quarter of all patients) qualified for more intensive and comprehensive services. For example, the highest risk patients were assigned to local nurse care managers, who contacted them regularly to help them manage their chronic conditions. I use a proprietary administrative data set to measure the impact of the DM program interventions on health care expenditures and other health utilization measures.

I use a natural experiment to empirically estimate the causal impact of the high-intensity and moderate-intensity interventions – administered to high and moderate risk patients – compared to the counterfactual of what would have occurred if these individuals had instead only received the very minor low-intensity interventions. I use a natural experiment to identify the causal impact of these interventions by exploiting an administrative error that postponed the introduction of the intended level of DM services for a significant number of high and moderate risk individuals. As such, some patients begin receiving high-intensity interventions when the program begins, while another group of observationally similar patients only receive the low-intensity interventions at the beginning of the program. Moderate interventions included outbound phone calls tailored to their chronic conditions and high risk members were assigned to care managers who performed additional DM interventions. The effect of the program is identified by differences in the timing at which individuals began receiving DM interventions, as

the data only includes individuals that are (eventually) enrolled in the DM program. I am the first researcher to study the effect of DM using this idiosyncratic feature of the GEC program.

Under the assumption that low-intensity interventions do not increase medical expenditures or lead to worse health outcomes, my estimates form a *lower bound estimate* of the effect of high-intensity DM interventions relative to no DM interventions. The point estimate indicates that the high and moderate intensity interventions lower the level of expenditures by an average of 4.4 percent for those patients with positive expenditures. The estimated effect of the program is much larger for the high-intensity interventions. A subset of patients sometimes has no medical expenditures in a given month; for these patients, the intensive DM interventions increase the probability of a zero-expenditure month by about 0.8 percentage points. Of the five diseases most commonly covered by DM programs, the largest decrease was for asthma, for which there was a large decline in the number of hospital and emergency department admissions. The source of empirical identification – a delay before some patients began receiving DM interventions – allows me to identify the effect of the program over time. I find evidence that the decrease in medical expenditures from the high-intensity interventions appears relatively early (within the first year of the program) and is sustained over time.

In addition, I examine the quantile treatment effect of these high and moderate intensity interventions over the distribution of medical expenditures using the methodology from Athey and Imbens (2006). In the baseline specification, the 90th percentile saw a decrease of about $200 per member per month, while the 10th percentile decreased only $80 per member per month. Because the baseline medical expenditures are highly skewed, this is actually a larger *percentage* decrease in expenditures for the lower end of the distribution.

This chapter compliments Chapter 2, by using detailed administrative data, which allows me to observe DM interventions and relevant health outcomes for thousands of high-cost, chronically ill Medicaid patients. By exploring the details of the program and, in particular, its

use of risk stratification and intervention targeting, I gain new insights into the mechanisms of DM program effects, as well as the size of the effect on sub-populations of eligible enrollees.

The remainder of this chapter proceeds as follows: Section 3.2 discusses relevant DM program evaluations already conducted by other researchers. Section 3.3 provides relevant details about the GEC program and the source of empirical identification. Section 3.4 describes my empirical models and Section 3.6 reports the findings. Section 3.7 concludes.

3.2 Relevant literature

Despite the rapid expansion of DM programs for the Medicaid population (see Chapter 2), the net fiscal impact of these programs has remained unclear. Many existing studies simply compare outcomes for treated individuals to baseline measures, in some cases after adjusting for a preexisting trend. This methodology is potentially confounded by other changes that occur simultaneously with the introduction of the DM program that have an effect on outcomes of interest. Furthermore, the measurement of preexisting trends are sensitive to key variables (such as the timeframe length and claims runout periods) and changes in the risk profile of the population. (Juster et al. 2009; Duncan et al. 2008) Another common approach is to compare outcomes between individuals who do and do not opt into a program. However, these two groups likely differ along a variety of both observable and unobservable characteristics, potentially biasing estimates of the program's effect. DM program evaluation is also difficult because there are large numbers of important outcomes to choose from, many of which are difficult to measure precisely, and it may take many years to have an effect on some variables. Health spending is often very skewed and subject to random fluctuation, making it particularly difficult to obtain precise estimates from small randomized trials. (Farah et al. 2008) Finally, the financial effect of a DM program will depend on the details of the program's design, including what population is covered and the scope of DM interventions and services. It is possible that differences in the

design of DM programs contribute to variance in the results between existing studies, although the extent of this phenomenon is unclear.

In the only existing publication on the GEC program, Rust et al. (2011) discuss the first-year growth of average Medicaid expenditures for adult Medicaid enrollees eligible for Georgia's GEC program in the northern region (i.e., the counties *not* evaluated in this chapter, see below). They compare tabulations of realized Medicaid costs rate to four alternative actuarial cost estimates (by other authors) and conclude the program may have saved Georgia Medicaid over $200 per member per year, or 20 percent.[49] The program in the northern region cost the state $13.94 per person, leading the authors to conclude the program resulted in substantial net savings for the state of Georgia. The authors lack a control group and make no further attempt to empirically estimate the counterfactual of what would have occurred in the absence of the program.

Very few DM programs within the U.S. public health care system have been rigorously analyzed for their financial impact. A recent study by Holmes et al. (2008) made progress on this issue by presenting results from the Indiana Chronic Disease Management Program (ICDMP). This program was implemented for the state's Aged, Blind, and Disabled (ABD) population with diabetes or congestive heart failure.[50] In two urban provider group practices, the implementation of the program was staggered in a randomized manner across fifty individual practices, creating a treatment group with 387 individuals and a control group of 439. The authors report some

[49] The four actuarial cost estimates were (1) a cost-trend analysis commissioned by the state of Georgia with Mercer consulting, (2) the consumer price index (CPI), (3) US medical inflation rate from CMS, and (4) US Medicaid program cost trends from CMS. The Mercer report is unavailable to the public and the most recent CMS estimates were summarized by Martin et al. (2011) and are available online from CMS (2011).

[50] High risk patients received nurse care management while lower risk patients received a less intensive telephonic program. More information about the ICDMP program and the evaluation methodology is provided by Rosenman et al. (2006) and the technical appendix to the article. Two additional studies from the ICDMP program present generally negative results, failing to show a link between the DM interventions and the desired outcomes (lower medical expenditures and increased drug regimen compliance, respectively). (BP Katz et al. 2009; AJ Zillich et al. 2008)

evidence that the program induced cost savings for low risk CHF patients; the estimated effect for the other three groups – high risk CHF and high and low risk diabetes patients – was statistically insignificant. There are some concerns with the study, given the small sample sizes (especially for CHF patients) and that baseline comparisons between the treatment and comparison cohorts do not match well on observable characteristics (p. 858).

The state of Washington recently began a DM pilot program which included a randomized trial evaluation component. A study by Qualis Health (2008) examined the impact of the program over the first 9 (or 10) months of the program showed some indication of an *increase* in medical expenditures, although this result was statistically insignificant.[51]

Several additional studies review the impact of Medicaid DM, although they suffer because the treatment and comparison groups are formed by comparing individuals who opt-in (or do not opt-out) to the program to those who do not opt-in using propensity score matching algorithms. As such, the studies are subject to potential selection bias. Afifi et al. (2007) evaluate a large program in Florida, comparing 15,275 enrollees who opted-in to the program and 32,034 who did not. They report that the hospital utilization rates were lower for the treated group, with the strongest effects for the SSI patients with CHF and diabetes and asthmatic patients eligible for Temporary Aid for Needy Families (TANF). Results were generally consistent across racial groups. (Kominski et al. 2008) In this Florida study, pre/post comparisons for individuals in the treatment group showed increased probability of self-reported healthy behaviors (e.g., quit smoking, dieting) and related health outcomes (e.g. blood pressure decrease) after participating in the program. (Morisky et al. 2009) Linden, Berg, and Wadhwa (2007) evaluate an asthma DM program with Medicaid patients in Oregon and report some evidence of increased office visits and a decrease in asthma-related emergency department visits (no financial outcomes reported).

[51] A predecessor program in Washington state, the Washington Medicaid Integration Partnership (WMIP) was evaluated by Esposito et al. (2007, pp. 185-195). They show some positive results on outcome measures such as hospitalizations, although the impact on medical expenditures is not reported.

Thiebaud et al. (2008) find that DM was associated with higher adherence to recommended medication and testing regimens for diabetic patients in Florida (no financial outcomes reported). In another study, Berg and Wadhwa (2009) find that, in a Medicaid DM program for diabetes patients in Puerto Rico, 490 treated individuals spent significantly less in the post-intervention period than matched controls. Pharmacy costs and usage was little changed but other medical costs decreased to 50 percent of the matched controls. Apparently, much of this reduction in expenditures resulted from reduced incidence of inpatient admissions and readmissions (emergency department usage actually increased).[52] For a program in Pennsylvania, Johnson, Yin, and Berg (2003) compare 313 members who opted in to the program to non-participants and other members of the MMC plan not referred for DM. Using difference-in-difference methods, they found statistically significant reductions in hospital utilization rates for the treated group. This resulted in reduced costs of about $370 per person per year with an implied return on investment of 131 percent. Zhang et al. (2008) compare the patients of doctors and pharmacists who did and did not opt-in to the Virginia Disease State Management (DSM) program. They found the program reduced ED, hospital, and physician office visits and lowered the prevalence of adverse drug events. Survey responses indicated quality-of-life indicators decreased more slowly as treated individuals aged, relative to the control group. Estimated financial savings were just over $20 per patient per year.

A larger literature examines the effect of various DM programs outside of Medicaid, typically in the private sector. This includes some (typically small) randomized trials. Reviews of this literature generally conclude that DM programs have positive effects on disease management and some health outcomes, but do not find conclusive evidence that DM leads to a net reduction

[52] A few other studies exist, but these are not based on an empirical analysis with a moderate to large sample and/or do not use a control group. (Ricketts, III et al. 2004; McCarthy and Mueller 2009; T Wilson 2007; Florida Office of Program Policy Analysis and Government Accountability 2001, 2004; Lind, Kaplan, and Berg 2006)

in health care expenditure.[53] (CBO 2004; Goetzel et al. 2005; Mattke, Seid, and Ma 2007; Ofman et al. 2004; Shekelle et al. 2003; Weingarten et al. 2002; McDonald et al. 2007)

Gertler and Simcoe (2009) examine a DM program that was implemented by a private health plan for members with diabetes. The authors, using methods similar to mine, compare 848 members who opted into the program with members who were eligible for the program but did not enroll because they did not opt-in or could not be contacted due to poor quality phone records. They "find evidence that the program led to increased compliance with clinical practice guidelines, improvement in patient health, and significant reductions in the total cost of care." The DM effect is large: HbA1c test scores fall 20 percent and total medical expenditures fall 65 percent in the first few quarters after enrollment among patients who did not receive recommended diabetes screening tests in the baseline period. Lairson et al. (2008) use similar methods to study the effect of an enhancement to a DM program in the private sector, although there the treatment and control groups were based on insurance provider, with matched patients from other managed care plans. Cost savings estimates were small and statistically insignificant. Beaulieu et al. (2003, 2006) and Fireman, Bartlett, and Selby (2004; 2005) provide analysis on the cost effectiveness of two private sector DM programs, but their chapters also suffer because they do not use comparison groups that convincingly serve as a counterfactual. These papers provide insight into how a number of classic issues in health economics (adverse selection, turnover, contracting, externalities, etc.) are relevant to studying the impact of DM on program participants, insurers, and others.

CMS recently conducted large, randomized DM demonstration programs for Medicare beneficiaries. A review of the programs are provided by Bott et al. (2009). Overall, the programs demonstrated modest success, at best: the majority of the 35 programs struggled to increase

[53] Literature reviews are also available for DM programs for particular diseases, including diabetes (Knight et al. 2005; Sidorov et al. 2002), cardiovascular diseases (Clarke, Shah, and Sharma 2011; Gonseth et al. 2004), and asthma/COPD (Mattke, Martorell, et al. 2006; Sin et al. 2003; SJC Taylor et al. 2005).

health care quality and at the same time maintain budget neutrality (much less financial savings). In the Medicare Coordinated Care demonstration, 15 sites throughout the country, each with a unique vendor, implemented DM with various program interventions. The results of this demonstration were disappointing; analysis of data through the first three years of the program found that 13 of the 15 programs did not observe decreases in the number of hospitalizations and that none of the programs generated net savings. Only two programs that appeared to improve some aspects of care while remaining cost-neutral were allowed to continue; the remaining programs were discontinued. (R Brown et al. 2008; Peikes et al. 2008, 2009) Chen et al. (2008) evaluated three programs that targeted severely chronically ill patients with disease management interventions, coupled with a prescription drug benefit (before Medicare Part-D). All three programs were terminated early due to failure to generate enough savings to cover the program costs, failure to adhere to CMS protocols, or Hurricane Katrina. Several other demonstration programs also report disappointing results with respect to quality improvement and/or the lack of financial savings. (Esposito et al. 2008; McCall et al. 2008, 2010; Moreno et al. 2005) Some of the programs were discontinued or redesigned due to poor findings in their evaluations.

My research ultimately relates to the larger literature on cost savings and quality enhancement programs in the Medicaid program. It is not clear that contracting with private firms to provide health services will result in the dual objectives of reduced costs and increased quality of health care services for Medicaid enrollees. A large number of other studies have shown significant relationships between various Medicaid program designs, payment structures, costs, health care utilization, and health outcomes in other settings. (e.g., J Currie, Gruber, and Fischer 1995; Duggan 2004; Duggan and Scott Morton 2006; Gruber, Kim, and Mayzlin 1999; Mullen, Frank, and Rosenthal 2009; Quast, Sappington, and Shenkman 2008) For an introduction to issues related to "prevention" in health care, see Kenkel (2000).

3.3 The Georgia Enhanced Care Program

This chapter examines a specific DM program that was implemented in the state of Georgia in late 2005. I use a proprietary administrative data set to measure the impact of the most *intensive* DM interventions on the highest-risk members in the eligible population. I identify the *causal* impact of these interventions by exploiting an administrative error that postponed the introduction of the intended level of DM services for a significant number of individuals.

3.3.1 Overview

The state of Georgia has the ninth largest Medicaid population in the United States, with 1.46 million enrollees in June 2010. (KFF 2011b, p. 4) Beginning in January 2005, Georgia began a series of major reforms in the Medicaid program, including shifting the majority (roughly 85 percent) of enrollees into a Managed Care program. The rest of the population, including all ABD adults and children and Katie Beckett program participants, were enrolled in a DM Program that supplemented the pre-existing PCCM/FFS program.[54,55]

The state is divided regionally by county into two regions; each region received its own DM program by separate DMO vendors. This chapter exclusively studies the performance of the program in the 106 counties in the Southern Region (see Figure 11), which is known as the "Georgia Enhanced Care" (GEC) Disease Management program. The vender was paid a per-member-per-month fee (PMPM), about $30, for each member in the eligible population. All of the contract fees were at risk if the DMO did not meet its cost savings guarantee targets (80

[54] The GA PCCM program makes the primary care physician (PCP) responsible for locating, coordinating, and monitoring all primary care and other medical services on behalf of recipients involved in the program. As compensation, they are paid a small "primary care case management" (PCCM) fee in addition to the usual fee-for-service (FFS) reimbursements.

[55] Information on the program in this section taken from Georgia Department of Community Health RFP (2005); response to RFP by United HealthCare Services (2005); the related contract between the parties (2005) and other documents, personal communication with program staff (Abraham et al. 2007), and relevant websites (http://www.georgiaenhancedcare.com, http://dch.georgia.gov).

[56] To approximate, the payout by the vender for missing the target was calculated by comparing post-implementation PMPM costs to pre-implementation PMPM costs, where the later was adjusted by actuarial estimates of the pre-program growth rate.

percent of the fees) and health outcomes benchmarks (20 percent); the key restriction was that the DMO vendor guaranteed a five percent reduction in average PMPM costs, net of the fees the state paid to the DMO vender.[56] As program costs were about 5 percent of baseline PMPM costs, the DMO was required to generate savings of about $65 PMPM, or a 9.3 percent decrease in PMPM claims.[57] The program was implemented in October 2005 and took a few months to fully ramp-up. In a typical month, 48,000 to 50,000 individuals were deemed eligible for DM services. (Table 15) As is typical for Medicaid programs, there is some variation in the number of eligible members from month to month, both from individuals exiting and entering the program or temporarily becoming ineligible for short spells. Program membership increased to just over 60,000 individuals in January 2007 due to changes in the program eligibility criteria during contract renewal. (There was a decrease in the number of eligible children in December 2006 due to the creation of a separate program for children; children deemed to already be receiving substantial DM services remained in the GEC program and the rest were switched into the new program).

In Georgia, the introduction of DM services was part of a much larger set of health care reforms for the Medicaid population, which included moving a large number of individuals into a Medicaid Managed Care program. This could bias an analysis of the impact of DM using a simple pre/post break in the trend of medical expenditures; other concurrent policy changes could increase or decrease medical expenditures in addition to the actual effect of DM interventions themselves. This implies that pre-2005 trends in health expenditures could be unreliable for formulating a counterfactual of what would occurred in the absence of the program.

[56] To approximate, the payout by the vender for missing the target was calculated by comparing post-implementation PMPM costs to pre-implementation PMPM costs, where the later was adjusted by actuarial estimates of the pre-program growth rate.

[57] For this purpose, the FY2004 baseline was determined to be $624.26 PMPM with a baseline growth trend of 11.8 percent per year. (Georgia Department of Community Health 2005, sec. Addendum 4, Attachment A.2)

3.3.2 Variation in treatment and source of empirical identification

The main source of variation in my data set exists because the population was divided into *high*, *moderate*, and *low* risk groups, based on risk stratification algorithms for (i) previous diagnosis with specific chronic conditions or by reaching specific "cutoffs" in (ii) health care expenditure or (iii) health utilization measures (hospital emergency department visits and admissions).[58] DM interventions for these distinct groups were performed by separate staffs and varied significantly in intensity. The data set used in this study includes detailed information on staff-enrollee interactions, as discussed below. Although it is possible to construct continuous measures of DM interventions (e.g., "number of phone calls per month"), I focus on the differences in treatment associated to these discrete categorical groups. The intensity of DM interventions can be understood as increasing monotonically between the categories.

All enrollees, including those in the *low* risk group, received an initial outreach call to notify them of the program and ask questions from a short "initial intake assessment" (although moderate and high risk members were clearly prioritized, as seen below in Table 21). The program also provided a few basic services such as access to a 24-hour, "1-800" phone number staffed by a nurse. Mass mailings with educational information were sent to members. Insurance claims data and information collected by the program was periodically analyzed and members could be upgraded into a higher risk group. These interventions for the low risk group are

[58] According to the company that implemented the program, utilization-based stratification assigned enrollees to the high risk group according to the following formula in the initial period ($t = 0$):

$$HighRisk_{i,t} = \begin{cases} 1, \text{ if } Disease_{i\tau} \text{ for particular diseases in } \tau < t \\ \quad \text{or if } \sum_{\tau=t-13}^{t-1} expend_{it} \geq \$50{,}000 \\ \quad \text{or if } \sum_{\tau=t-7}^{t-1} ED_{it} \geq 6 \\ \quad \text{or if } \sum_{\tau=t-4}^{t-1} inpatient_{it} \geq 3 \\ 0, \text{ otherwise} \end{cases}$$

In later months, this algorithm was replaced with much more complex predictive modeling strategies. These cutoffs create sharp discontinuities in the risk group eligibility criteria, suggesting a regression discontinuity model. This approach was pursued by the author. Preliminary analysis suffered from weak power due to the small numbers of individuals immediately near the discontinuity and was eventually abandoned in favor of the analysis presented below.

assumed *a priori* to have relatively small effect on health expenditures, if any, although my identification strategy does not require the assumption of zero effect.

As described in Table 16, enrollees assigned to the moderate and high risk groups received significant, additional attention from the program staff beyond these basic services. The *moderate* risk group received significantly more attention from telephonic care managers (the "call center"), particularly in the form of outbound phone calls for health education, encouraging particular health services, and monitoring of their conditions. Education could focus on prevention, behavior modification, or compliance with recommended clinical guidelines. Some common issues were addressed with "call scripts," where lower-level staff (i.e. not nurses) would make outbound calls to, for example, tell enrollees about resources to assist with smoking cessation or remind the patient to have a HbA1c blood test or schedule a visit with their health care provider.

In addition to these types of services, members in the *high* risk group were individually assigned to local care managers who were personally responsible for providing a variety of education, coaching, monitoring, and consultations with the patients. In addition to phone calls, the care manager could make home visits or accompany patients to appointments with their health care providers. By matching patients to care managers (often nurses), the interventions for this group were naturally less prescribed by the DMO's computer algorithms and more tailored to the individual needs of these patients by the care manager (although high risk patients also received calls based on "call scripts" from other staff). They could receive medical equipment when deemed necessary. The DMO's proposal provides the following example on the difference between the moderate and high risk interventions:

> *… For example, the interventions for "Moderate Risk" asthmatics indicate that we will provide "Routine RN/Educator coaching", while the interventions for "High Risk" asthmatics indicate that they will receive the same intervention, along with additional approaches. In reality, the "Routine RN/Educator coaching" will differ for these risk strata based on the need of the individual. "Moderate Risk" enrollees may receive telephonic coaching on a monthly basis; the "High Risk" enrollees are more likely to receive in-person*

> *coaching from an RN, who may visit the individual as often as weekly. In addition, it is common practice for our nurses to accompany individuals to PCP appointments to coach them in asking the right questions and requesting appropriate support. (United Healthcare Services, Inc. 2005, pp. 4.4-8)*

Many of the interventions were designed to address common issues, particularly common diseases including asthma, diabetes, coronary artery disease (CAD), congestive heart failure (CHF), chronic obstructive pulmonary disease (COPD), and risk factors related to chronic illness (e.g., smoking and obesity). However, the high risk group, by nature of being paired with trained nurses, could receive idiosyncratic interventions for relatively rare diseases (e.g., hemophilia, schizophrenia) and the complexities that arise between co-morbid conditions.

Although the DM vendor intended to target intensive interventions to all of the most sickly patients, an administrative database programming "mistake" prevented some claims data from being transferred to the DMO that was contracted to provide the DM services. As a result, for some individuals, incomplete data was used in the risk-stratification algorithms during the first months of the program and thus these individuals were categorized into a lower risk group (lower than they would have been assigned to in the absence of the mistake) for the first eight months of the program. The mistake was corrected in the summer of 2006.[59]

The individuals affected by this mistake did not receive the "proper" level of DM interventions, as did their peers, and therefore form an unintended comparison group that can be used to measure the program's effectiveness using quasi-experimental methods. This unusual event provides the basis for an empirical evaluation of the program. Although data analysts and program administrators at the DMO were aware this issue existed, and the steps they took to rectify the situation, they did not attempt to use this mistake for evaluation purposes. (Abraham et

[59] I assume that the risk stratification algorithms were unchanged between October 2005 and July 2006. Although I do not believe there were any major changes unlit late 2006, the possibility of an adjustment to the risk stratification algorithm at an earlier date cannot be ruled out (for example, if the DMO gave a new directive to a subcontractor). The similarities between the "treatment" and "control" groups in Table 20 alleviate this concern.

al. 2007) I am the first researcher to study the effect of DM using this idiosyncratic feature of the GEC program.

3.4 Data and summary statistics

3.4.1 Description

I use a proprietary administrative data set acquired from the DMO that performed the DM program, which includes medical claims records, basic demographic information, program eligibility, and information on various program interventions. According to the plan, when a member became eligible for the program, the state was to provide the program DMO vendor with any Medicaid claims data for the past 24 months (if any claims occurred) and then continue to send the vendor monthly updates with new claims activity for as long as the member remained eligible for the program.

I use this claims data, which amounts to 13.2 million claims records for 69,831 individuals. Insurance claims include details such as ICD-9 diagnosis and procedure codes, information on prescription drugs, billed and paid claim costs, and other details. I tabulated claims records into monthly summary data. I also have access to de-identified demographic information such as age and gender plus additional data related to the DM program itself, such as the dates the individual was eligible for the DM program.

Finally, I have access to data created by the DMO vendor as they implemented the program. I observe what risk group (high, moderate, low) an individual was assigned to when they are enrolled in the program and other administrative details. I also have data on DM program interventions from the computer software used by program staff, including the dates of any completed assessments (27,600 health risk assessments, 6,400 comprehensive assessments, and 7,000 other assessments). I observe 240,000 tasks performed by case managers, which I classified into categories based on their method of contact (phone call, in-person, etc.) and objective (assessment, education/training, coordination/referral, etc.). Finally, I have records for 9,600

"goals" set by the Care Managers in the computer system and the date they were marked as completed.[60]

A timeline of program implementation and data coverage is provided in Figure 12. Claims data covers the period January 2005 through December 2007 and is limited to include only individuals eligible for the program at any time during this period. Thus, nine months of pre-program "baseline" claims data is available for those who entered the program at the beginning of the program in October 2005. By the program's design, most adults in this program are categorically members of the Medicaid aged, blind, and disabled (ABD) population.

3.4.2 Sample

Below I estimate the causal impact of the high and moderate risk interventions by exploiting the administrative error that postponed the introduction of the intended level of DM services for a significant number of individuals. Ideally, I could simply see in the data which enrollees (i) *actually were selected* or (ii) *should have been selected* into the high or moderate risk groups when the program was originally implemented. Unfortunately, the data does not identify who was affected by the administrative coding error and I cannot replicate their DM stratification algorithms given the data available.[61] However, I do see the risk group assignment for each individual for each month, which can be used to *infer* which individuals were affected by the mistake. After assigned people to risk groups in the initial months of the program, the enrollee population's risk group status is quite stable from month to month. Then, several thousand individuals are upgraded from a low risk group to a higher risk group in June/July of 2006, which corresponds to the date that program staff indicate the mistake was fixed. This is followed by

[60] Unfortunately, I do not have access to goals set by staff, but never completed. I do not have records of health information mailings.

[61] The data I have access to is more limited than the data used by the vendor (at some points in time). Most importantly, I do not have all the data that was available to the company for the risk-stratification in October 2005, as I have 9 months of data where the vendor would have had (up to) 24 months of data. In June 2006, the claims data not originally received by the DMO vendor was mixed with the existing data (from program start) and is indistinguishable from my perspective.

another period in which assignment to the high and moderate risk groups was relatively more stable. (Figure 13)

Table 17 shows the sample sizes based on being assigned to the high or moderate risk groups anytime in the phase-up period of the program (October 2005 – January 2006), plus individuals who I infer were affected by the administrative coding error and suddenly "switched" into the high or moderate risk group in June and July 2006 from a lower risk category.[62] The primary estimation sample is limited to individuals who were eligible for the program in the phase-up period and were therefore eligible for assignment into the high or moderate risk group at the discretion of the DMO. For example, some individuals were ineligible for the program for some months between October 2005 and June/July 2006 *and* assigned into the moderate or high-risk group in June/July 2006. These individuals are *not* included in the main estimation sample, in case there are systematic differences between the majority of individuals (who enter the program as expected) and the latecomers who gain DM eligibility at a later date.

These exclusions leave a primary sample size of 17,349 individuals. Table 18 identifies how these individuals were categorized for their original risk group during program startup (i.e., through January 2006) and their risk group after the mistake was fixed in the summer of 2006. About two-thirds of the individuals were correctly assigned into a higher-risk category during the phase up period (9,522 and 2,371 in the moderate and high-risk groups, respectively). The remaining one-third (5,456) were delayed from receiving the more intensive DM interventions until the mistake was corrected in the summer of 2006.

[62] In order to alleviate the concern that this group of individuals does not appropriately compare to those in the high or moderate risk groups, two additional exclusions were adopted. First, any individual who had a major health event just before June or July 2006 was removed from the sample to reduce the chance of mis-identification of the reason for being switched into a higher risk group (i.e., those switches not due to the administrative error). Major health events are defined to be an increase in the individual's Chronic Illness and Disability Payment System (CDPS) score of 2.0 or more in the previous month or an increase of 3.0 or more in the last two months. Second, the primary sample also excludes a few individuals who "switch" into the higher risk groups for one-month and then return to their initial classification (a "false switch").

3.4.3 Summary statistics

Table 19 presents summary statistics at baseline for the five groups presented in Table 18. Comparing the columns in Table 19, one finds support for the claim that observationally similar groups of individuals both were and were not classified into the moderate or high risk groups in October 2005. Although comparisons between these groups are nuanced, the general pattern is that the early-entry and delayed-entry high and moderate risk patients are generally similar based on demographic, chronic diagnoses, and baseline expenditure and utilization measures. As expected, the individuals that were classified into the high and moderate risk groups (initially or after the mistake was fixed) tended to be sicker than those individuals in the low risk groups, and their health utilization tended to be much higher. According to the Chronic Illness and Disability Payment System (CDPS) disease identification algorithm for all months in the data, the vast majority of high and moderate risk patients had five or more different disease-category diagnoses flags.[63] As suggested by Lewis (2009), I also calculate hospitalization rates and find that the high and moderate risk patients were more likely to visit the hospital and/or emergency department for events related to chronic illnesses commonly addressed by DM programs.

I test if these groups are observationally similar, and therefore their baseline characteristics should have led to similar risk categorization. First, for the data presented in Table 19 (Panels A and B) I calculate the normalized differences between selected columns for each of the variables. Results are presented in Table 20.[64] As a rule-of-thumb, the normalized difference

[63] This study uses the CDPS + Rx model, software version 5.1 (Gilmer et al. 2008; Kronick et al. 2000; Gilmer et al. 2001). For a comparison of the CDPS model to other tools, see Winkelman and Mehmud (2007) and Weir, Aweh, and Clark (2008). I do not use lagged indicators of claims in the models, even though this may increase the model fit (Li et al. 2005), to avoid endogeneity concerns and selection prone to suffer from reversion to the mean. Rein (2005) risk stratifies a sample of Georgia Medicaid enrollees.

[64] The normalized difference is given by the formula $\Delta_X = \left(\overline{X}_1 - \overline{X}_0\right)/\sqrt{S_0^2 + S_1^2}$, where S_w^2 is the sample variance of X_i and $\overline{X}_w$ is the sample mean for group w. This variable has the advantage over the t-statistic because it is not affected by the sample size.

should not exceed 0.25 for most variables in the baseline period. (Imbens and Wooldridge 2009, p. 24) Broadly speaking, the individuals who are classified into the moderate risk group (initially or after the mistake is fixed) are observationally similar, as are the individuals in the high risk group. Normalized differences between columns are less than 0.25 in most cases. (Less that 5 percent are over the cutoff; it is expected that a few variables would be over the cutoff, just by chance, given the number of comparisons).

Second, I run an ordered probit regression to predict the probability of selection into the moderate or high risk groups on a large array of baseline indicators: a quadratic polynomial in pre-period health expenditures; the number of admissions to hospitals, emergency departments, and residential facilities; a large array of chronic disease dummy variables; and dummy variable for 10-year age-by-gender bins. The propensity score for selection into these categories is graphed as a histogram in Figure 14. Those patients who were delayed entry into the high/moderate risk groups have a propensity score distribution that is more similar to those who were assigned early into these groups, compared to the individuals that never were assigned to the high/moderate risk groups. That is, this propensity scoring exercise indicates substantial *common support* between the groups of individuals who were assigned to the high/moderate risk groups originally, and those who were upgraded to the high/moderate risk groups when the mistake was fixed.

Because of the delay in the risk categorization into high/moderate risk groups due to the administrative error, the individuals affected by the mistake were subject to significantly different disease management intervention patterns. As seen in Table 21, those who were classified in the high or moderate groups initially were far more likely to receive significant DM interventions between program start-up in October 2005 and July 2006. The DMO vendor preformed assessments at much higher rates, and performed a larger number of DM intervention "tasks" with the members or for the members who entered these groups during program startup. To

conclude, this section provides supporting evidence that otherwise similar patients did in fact receive different DM interventions during this period.

3.5 Empirical approach

This chapter seeks to measure the causal impact of the *high* and *moderate intensity* interventions in this Medicaid DM program.[65] In response to the current policy debate, one relevant outcome in this study is "overall cost savings" because the program was justified by the argument that enhanced preventive care and patient adherence to evidence-based guidelines would lead to reduced costs for the Medicaid program. I also research the impact of DM on other relevant outcome variables, such as hospital and prescription drug utilization, to better understand the mechanisms and the non-pecuniary benefits of DM.

Although this data has the advantage of its completeness and transparency with respect to program interventions, this data set, by its nature, has limitations that affect the types of questions that can be researched, the analysis methods available, and ultimately the interpretation of the results. Note that this *data set only includes individuals who are enrolled in the DM program* at some point during the first 26 months of the program. Therefore, I do not have a comparison group always left out of the DM program. Instead, the effect of the program must be identified based on differences in the *timing* at which individuals began receiving DM interventions, as the data only includes individuals that are (eventually) enrolled in the DM program.

In particular, I exploit this administrative error that postponed the introduction of the intended level of DM services for a significant number of high and moderate risk individuals (discussed above). As such, some patients begin receiving high-intensity interventions when the

[65] Obviously, there are many other potential research agendas. For example, there are principle-agent conflicts between the state government administrators and the vendor that administered the program or the interaction of DM programs with enrollees' (or their providers') decisions regarding their own health care (e.g., the role of information, addiction/self-control, the demand for acute services with low marginal costs).

program begins, while another group of observationally similar patients only receive the low-intensity interventions at the beginning of the program. Conceptually, the potential response to this delay in treatment may be similar to what I have drawn in Figure 15. In October 2005, an individual would begin to receive DM interventions and therefore the outcome variable, Y_{it}, begins to fall as a result of the program. However, if this individual was affected by the administrative "mistake," they would not have received the high- and moderate-intensity interventions due to assignment to a lower risk group. As such, Y_{it} would continue at a higher level, until the mistake is fixed in June 2006, whereupon Y_{it} begins to respond to the DM interventions. The crucial element in this case is the *length of time* a patient has been eligible for the program and assigned to receive the more-intensive DM interventions.

Although the interventions provided to the low risk group are expected to be small, I do not explicitly assume they have zero effect. This framework identifies the effect of the GEC program's more expensive "high-touch" interventions that were provided to the moderate and high risk groups. That is, I identify the effect of being "upgraded" to receive moderate or high risk group interventions from the basic low risk group. I cannot identify the effect of low-intensity interventions (relative to no DM), because there is no comparison group in the data who receives no DM interventions. I do not have access to data for other potential comparison groups.[66] (In Figure 15, I abstracted away from the impact of low-intensity interventions, which may cause Y_{it} to fall in October 2005 for both cases). Under the assumption that low-intensity interventions do not increase medical expenditures or lead to worse health outcomes, my estimates form a *lower bound estimate* of the effect of high-intensity DM interventions relative to no DM interventions.

As with most empirical program evaluations, this chapter approaches the question at hand via a potential outcomes framework. Following the literature, I define the treatment effect to

[66] I unsuccessfully tried to obtain data on the Medicaid-Medicare "dual eligible" population in Georgia, which was similar to the ABD population but not eligible for the DM program.

equal the difference between an outcome, $Y_{it}(D_{it})$, for an individual that does ($D_{it} = 1$) or does not ($D_{it} = 1$) receive treatment from a DM program:

$$\Delta Y_{it} = Y_{it}(1) - Y_{it}(0) \quad 6$$

As one never observes Y_{it} under both regimes, one of the terms in this equation is a counterfactual that must be estimated. [67]

3.5.1 Baseline model

To begin, consider a standard difference-in-difference model of the form,

$$y_{it} = \delta\, DM_{it} + \eta_i + \lambda_t + \varepsilon_{it} \quad 7$$

where y_{it} is an outcome variable of interest (e.g., total health expenditure claims), for individual i in month t. The key variable of interest, DM_{it}, is a dummy variable that equals one if the individual has ever entered the high or moderate risk group during month t or earlier, and equals zero otherwise. DM_{it} remains equal to one for a patient even if he or she was "graduated" into a lower risk group (due to proper maintenance of their disease).

The variable η_i is an individual-specific fixed-effect that will control for all idiosyncratic, time-invariant aspects particular to the individual, including age, gender, and the presence of chronic conditions and co-morbidities. The variable λ_t represents a month-specific fixed-effect that will control for any changes that influence y_{it} for all individuals in the model. The month fixed-effect controls for seasonal variation in utilization, inflation in the price of health care, new technologies, statewide Medicaid policies, and so on. Furthermore, it controls for any low-intensity interventions, such as the fact that eligible members were provided access to a 24-hour nurse line beginning in October 2004, regardless of their risk group status. Finally, ε_{it} is an idiosyncratic error term with the usual properties.

[67] For an introduction to evaluation methodologies in the context of DM research, see Arnold, Folsom, and Bosk (2007), Fitzner et al. (2004), Linden, Adams, and Roberts (2003), Linden (2006), MacDowell and Wilson (2002), Mattke et al. (2006), and Wilson (2003). For more on the potential outcomes framework, see the exchange between Angrist, Imbens, & Rubin (1996) and Heckman (1996), or the overview by Imbens & Wooldridge (2009).

The coefficient, δ, therefore measures an average response to the introduction of more intensive DM services. The key source of identification for the estimate on the effect of the DM intervention, $\hat{\delta}$, comes from the *timing* at which DM interventions are introduced. In this baseline specification, this parameter is primarily identified by the initial treatment period, when some individuals are properly assigned to the high-risk group, while some of their peers received fewer interventions (due to the "mistake" discussed in Section 3.3.2).

This chapter is the first research on public DM programs I am aware of that controls for unobservable characteristics of an individual with individual fixed effects. I argue that observed and unobserved characteristics about the individual can have important impacts on the outcomes of interest, and that this methodology is therefore superior to propensity-score matching in this setting. Propensity-score based methods, which have been used in other studies (e.g., Gertler and Simcoe 2007; Linden, Adams, and Roberts 2005; Linden, Berg, and Wadhwa 2007), would rely on the assumption that – once adjusting for the estimated propensity of being selected into a high/moderate risk groups – remaining covariates are independent of the treatment indicator and therefore would not bias outcome estimates. However, that argument is fairly difficult to justify here.[68] Controlling for an individual fixed effect allows me to control for important differences in the underlying (observed and unobserved) characteristics of the treated and untreated populations.

One of the main concerns in this setting is that the high and moderate risk patients will exhibit *reversion to the mean*. Consider the case where an individual has abnormally high expenditures in the months before the program began. High expenditures would increase the

[68] Take for example a patient with cancer that also has the chronic condition asthma, which she manages appropriately. As the DM program only based risk group assignment on health care utilization and chronic illnesses, she could likely be "matched" to another patient with asthma that is poorly controlled and therefore generating a large number of hospitalizations, yet these patients would be expected to have very different future medical expenditures. In other words, there are a large number of individual-specific factors that are not independent of the outcomes of interest variable, yet do not necessarily influence the propensity of selection (i.e., unobserved variables plus variables that are observed but imperfectly modeled). In practice, propensity score matching yields to estimates of similar magnitudes to those presented below.

probability of assignment to the high/moderate risk groups, by design. However, if these high-expenditures were simply a random event, the level of expenditures are likely to return to normal (lower) levels, even in the absence of the DM interventions. If the analysis below is not careful, estimates could be biased because the treatment group might be more likely to experience a fall in expenditures than the untreated group, *ceteris paribus*.

The primary way to control for reversion to the mean is to form a counterfactual with untreated individuals likely to experience similar levels of mean reversion to the treatment group. Thus, in order to exploit this natural experiment, I limit my sample to individuals that either (i) *actually were selected* or (ii) *should have been selected* into the high or moderate risk groups when the program was originally implemented in the empirical specifications. That is, I limit my data set to the 17,349 individuals I defined above in Section 3.4.2: individuals originally assigned to the high and moderate risk groups and individuals who I infer to have been affected by the administrative mistake, but were otherwise eligible for higher-intensity interventions. I drop the remainder of individuals – three quarters of the original data set – from the baseline regressions because the low-risk individuals are less likely to experience said mean revision and/or outcomes for these individuals may have underlying trends that are systematically different than the trends for the moderate and high risk individuals.

As can been seen in Table 18, there were individuals who were kept out of both the high and moderate risk groups due to the administrative coding error. Thus, this data can also be used to identify the differential effect of the high risk group interventions (on-site providers) above and beyond the moderate risk group interventions (telephonic). Thus, I also present results of the following form:

$$y_{it} = \delta_{mod}\, DMMod_{it} + \delta_{high}\, DMHigh_{it} + \eta_i + \lambda_t + \varepsilon_{it} \qquad 8$$

Similar to above, $DMMod_{it}$ and $DMHigh_{it}$, are dummy variables that equal one if the individual has ever entered the moderate or high risk groups, respectively, during or prior to month t. $DMMod_{it}$ is defined to equal zero when $DMHigh_{it}$, equals one, to prevent both terms

from equaling one simultaneously. With this contrction, δ_{high} identifies the impact on y_{it} due to all DM interventions received in the high risk group and δ_{mod} measures the impact of the interventions to individuals in the moderate risk group. The marginal impact of moving from the moderate to high risk group is the difference, $(\delta_{high} - \delta_{mod})$.

It is common that many health outcome variables of interest (potential y_{it}'s), such as health care expenditures or the number of hospitalizations, do not fit linear models particularly well. Typically, three issues require additional attention from the econometrician: (i) y_{it} is strictly defined such that it must be non-negative, (ii) a significant number of observations equal zero, and (iii) the distribution of the nonzero observations are highly skewed (long right tail). In the literature in health economics, the issues are commonly addressed by using a two-part model. Compared to a tobit-type models on a latent censored variable, the two-part model has the advantage that it provides parameter estimates that are interesting in their own right: they estimate the response of the dependent variable to treatment along the intensive and extensive margins.[69] The first part of the model may be a logit model that estimates the probability that y_{it} is positive:

$$\Pr(y_{it} > 0 | Z_{it}) = \frac{\exp(\beta_1 Z_{it})}{1 + \exp(\beta_1 Z_{it})} \quad 9$$

where Z_{it} is the vector of all independent variables, similar in form to equations 7 and 8. The second part of the model estimates the outcome, y_{it}, conditional on a positive observation. It is common to remove undesired skewness of health expenditure data by estimating a natural logarithm transformation of the dependent variable:[70]

$$\log(y_{it}) = \beta_2 Z_{it} + \varepsilon_{it} \text{ where } y_{it} > 0 \quad 10$$

[69] Results using a random effects tobit model are presented in Appendix C. Results are qualitatively similar to the two-part model.

[70] Manning and coauthors compare the OLS regression on $\log(y)$ to generalized linear models and propose an algorithm for choosing among the various possible estimators. In cases where y is a counting variable, researchers have alternatively used a zero-inflated Poisson model. (Manning 1998; Manning and Mullahy 2001; Basu, Manning, and Mullahy 2004)

The estimates from equations 9 and 10 are interesting in their own right and are helpful in understanding how individuals are effected by DM.

In addition, the expectation of y_{it} can be obtained by the equation

$$\mathrm{E}(y_{it}|Z_{it}) = \frac{\exp(\beta_1 Z_{it})}{1+\exp(\beta_1 Z_{it})} * \phi * \exp(\beta_2 Z_{it}) \quad 11$$

where ϕ is a "smearing" estimate used to transform the estimate from the log scale to the levels of y_{it} from Manning (1998).[71] For more on the two-part model, see the discussion in section 2.4.2, above.

3.5.2 Effect of DM interventions over time

In addition to measuring this average effect of the DM interventions across the post-intervention period, the data allows for more specific time-based analysis of the effect of the interventions. Therefore, I also run regressions where I provide dummies for the number of months before/after entry into the program for the individuals in the main sample:

In addition to measuring the average effect of DM across the post-intervention period, it is also possible to estimate a profile for the effect of the cumulative DM treatment over time by estimating regressions of the following form:

$$y_{it} = \sum_{\tau=-12,\tau\neq 0}^{25} \left(\delta_{\tau,high} h_\tau(DMhigh_{it}) + \delta_{\tau,mod}\, h_\tau(DMmod_{it})\right) + \eta_i + \lambda_t + \varepsilon_{it} \quad 12$$

Here, $h_\tau(DM_{it})$ is a dummy variable that equals one τ months before/after entering the DM risk group. Thus, each $\delta_{\tau,high}$ ($\delta_{\tau,mod}$) parameter estimates the effect of a patient having entered the high (moderate) risk group τ months prior to or after entering the program, while still controlling for both individual and time fixed effects. The parameters corresponding to $\delta_{0,high}$ and $\delta_{0,mod}$ are normalized to zero. This allows me to observe the time-profile of the program's effects on

[71] The smearing estimate was originally developed by Duan (1983) and updated to account for heteroscedasticity by Manning (1998). Manning's method is less precise than $\phi \equiv \sigma_\varepsilon^2/2$ if, in fact, the error is homoscedastic.

patient outcomes. As was the case above, these parameters are identified by differences in the timing at which (otherwise similar) individuals entered the high or moderate risk groups.

3.5.3 Effect of DM interventions across the expenditure distribution

Based on the discussion above, one should expect the impact of DM on health care expenditures and other variables to vary across the distribution of baseline health care expenditures. Understanding these distributional effects has important implications for program design and welfare considerations. For example, a social planner would choose to give high intensity interventions only to patients where the financial cost reduction and social benefit of better health are larger than the marginal cost of providing the interventions.

To identify the distributional impact of the program, I implement the Changes-in-Changes (CIC) model of Athey and Imbens (2006). As discussed by these authors, this model is in many ways a generalization of the models used above that allows one to calculate a "difference-in-difference" at each quantile of the distribution. This model relies on the assumption that the differences between the treated and untreated groups is stable, after eliminating a common time trend, and that differences between the two distributions can be attributed to the program. This assumption is not unreasonable to the extent that the delay of entry into the high or moderate risk groups was not systematically associated with differences in the time trends in the outcome variables.

In this model, we assume that in the absence of the intervention, the outcome variable is a function of the unobservable characteristics of individual i and the time period:

$$Y_i(0) = h_0(U_i, T_i) \qquad 13$$

The distribution of U varies across the treatment and control groups, but not across time ($U_i \perp T_i|G_i$). Therefore, the average effect of the program for the treated is given by the expression

$$\tau_{CIC} = \mathrm{E}[Y_i(1) - Y_i(0)|G_i = 1, T_i = 1] \qquad 14$$

Assuming that $h_0(U_i, T_i)$ is monotone in u and conditionally independent of T_i and U_i, given one's group, G_i, Athey and Imbens show that one can identify the full distribution of $Y(0)$, given that $T_i = G_i = 1$ (the second period for the treated group):

$$F_{Y_{11}}(y) = F_{Y_{10}}\left(F_{Y_{11}}^{-1}\left(F_{Y_{01}}(y)\right)\right) \quad 15$$

With this data set, the CIC model is implemented by collapsing each individual's monthly observations into one pre-intervention summary observation and one post-intervention observation. So that the pre- and post- observations cover a comparable length of time (and have the same expected levels and distributions as if the groups of individuals were identical and there was no program), I restrict the data set to the maximum period of time that would allow for there to be an equal number of months before and after the program was implemented: January 2005 through June 2006 (9 months of pre/post data). The data includes the average monthly expenditure in these two periods, rounded to the nearest dollar.

First, the standard CIC model was estimated on several outcome variables of interest. Second, I re-estimate the model while controlling for an array of standard covariates such as gender, age, and diseases observed in the pre-period. This can be done by first estimating an OLS regression of outcomes with group-by-time dummy variables and the control variables. Third, I calculate the residuals from this regression (including the four time-by-group effects) and then estimate the CIC model with these new "augmented residuals." (Athey and Imbens 2006)

3.6 Results

3.6.1 The effect of intensive DM interventions on medical expenditures and other outcomes

In this section, I present results for the primary variable of interest, net medical expenditures, based on the baseline model outlined in Section 3.5.1. I also use this model to evaluate the impact of DM on other measures of interest, such as hospital utilization.

Before turning to the formal analysis, I first present graphs with the mean monthly medical expenditures and mean number of emergency department visits for the high risk group. In Figure 16, one sees a pattern that generally follows the conceptual plot discussed above. Although the monthly sample estimates display a fair amount of month-to-month variation, both variables indicate high and somewhat stable levels of expenditure/emergency department utilization before the program is implemented in October 2005. At that point, the group of patients who is treated with the high-intensity interventions falls below the other two groups (those who mistakenly receive the low or moderate intensity interventions). However, by the time the data ends in December 2007, all of these patients have been receiving the DM interventions for at least 18 months and the medical expenditures/emergency utilization indicators appear below their initial levels. To formalize this intuition, I turn to regressions for the remainder of this section of this chapter.

Because nearly all patients in the moderate and high risk groups have some medical expenditures in a given month, I begin my formal analysis by focusing on the intensive margin, regressing the natural log of health expenditures (excluding the DM program fees, conditional that expenditures are positive) on the DM explanatory variables (discussed in section 3.5.1), individual fixed effects, and time fixed effects. Results from the baseline model (equation 1) are provided in Table 22, columns 1. This indicates a 4.43 log point decrease (SE 1.0) in net medical expenditures from receiving the moderate or high DM interventions. In column 2, I separate the effect of high and moderate intensity interventions, and find that nearly all the savings can be attributed to the effect of the high-intensity interventions: the high-risk group experienced a decrease of 17 log points (SE 1.4) in Medicaid expenditures while the decrease for the moderate risk group was small and statistically insignificant. Thus, my baseline model finds that the high-intensity interventions did generate significant cost savings while the effect of moderate-intensity interventions was indistinguishable from the effect of the services for the low-risk group.

The major concept behind DM programs rests on the prediction that DM interventions should have large effects among high-cost enrollees with chronic diseases. Thus, theory implies that I should expect different treatment effects for those without chronic illnesses. In columns 3 and 4, I interact the treatment variable(s) with a dummy variable that equals one for anyone with a chronic illness. This estimate produces a negative coefficient on DM_{it} of -9.9 (SE 2.81) log points, indicating significant costs savings among the high and moderate risk patients who do not have one of these diseases identified by the CDPS algorithm (but potentially another high-cost chronic conditions such as hemophilia, sickle cell anemia, schizophrenia and others that are not identified with the CDPS algorithm). Those with a chronic disease experienced a more modest 4.1 log point decrease in total claims ($-9.9 + 5.8$ log points). In columns 5 and 6, I find that, for individuals with a "top-5" disease, the high/moderate interventions were associated with a 3.4 log point decrease in medial expenditures ($-10.0 - 1.5 + 8.1$). As was the case before, the effect of high intensity interventions were much larger than the effect of moderate intensity interventions.

It is possible to identify the impact of DM for individuals with particular diseases. By interacting the DM variable with dummies for each of the "top-5" illnesses, in column 7, I find that DM had the largest costs savings among asthmatic patients, with log expenditures decreasing 14.1 log points ($-9.8 - 3.3 - 1.0$). Asthma is followed by COPD and CAD patients with a 10.9 and 7.9 log point decrease, respectively. Medicaid expenditures decreased the least for individuals with CHF (5.0) and, lastly, diabetes (3.5). Given that the effect of DM is identified with the first eight months of the program, the intuition provided by Arora et al. (2008, sec. 3) may apply to this case. The authors compare the expected timing of effects between asthma and diabetes,

> *For example, in managing asthma, programs can expect to see outcomes and savings in a relatively short period of time compared with diabetes, which requires behavior change on the member's part and, thus, likely will fail to see substantial savings in the short term. … Asthma is relatively easy to manage. With monitoring, proper use of medications, control of the environment, and avoidance of triggers, such as pet dandruff or second-hand smoke, most children and families can be relieved of the burden of asthma… Diabetes is a difficult*

> *disease to manage, because it requires behavior change by the member. Furthermore, because many of the outcomes of diabetes care management are seen much later, when complications (e.g., kidney failure) are avoided, diabetes management is unlikely to generate cost savings in the short term. (2008, pp. 3:3-4)*

Interestingly, the results in this chapter differ somewhat from my findings in Chapter 2, where CHF had larger cost savings and COPD had smaller cost savings, relative to the three other diseases. Diabetes was found to have the worst financial outcomes in both studies.

As a robustness check, column 8 includes low risk patients in addition to the main estimation sample. This approach has the advantage of greatly increasing the size of the data set, but the disadvantage that the individuals added to the sample are less comparable to those deemed moderate- or high-risk (see Table 19). The average DM effect on health care costs of -3.4 log points is only slightly smaller than the specification in column 1. DM for high-risk patients continues to drive these results (column 9). Differences between this estimate and those reported earlier are rooted in differences in the time trends that exist between the low, moderate, and high risk groups.

Other outcomes: I also estimate the effect of DM two subcategories of health expenditures (emergency department and prescription drugs) and other health utilization measures using the same specification as above (Table 22, columns 1 and 2). One should remember that, in the second stage of the two-part model, observations are dropped if the outcome is not positive and that individuals are dropped if they do not have at least two months with positive outcomes. In Table 23, columns 1 and 2, I present the effect of the high and moderate DM interventions on the log of emergency department (ED) expenditures. One would expect a decrease in ED expenditures (conditional on visiting an ED in the month) if, for example, DM reduced the prevalence of costly complications (e.g., from co-morbidities interacting with another illness) and/or caused patients who repeatedly visit their ED to visit the ED fewer times per month. Although the point estimate indicates a 3.7 log point decrease in ED expenditures, conditional on

an admission, the results are statistically insignificant. The standard errors are large because there are relatively few admissions in the sample.

In column 3, DM appears to have no significant change, on the intensive margin, of expenditures for prescription drugs. However, closer inspection in column 4 reveals that there is actually a statistically significant *increase* for moderate risk patients and a statistically significant *decrease* for high risk patients.

In Table 24, I run OLS regressions for a number of additional dependent variables. As seen in Table 24, these DM interventions resulted in a decrease of about 4.4 admission days per 100 patients (SE 1.5), mostly for the high risk patients. When I identify which types of procedures were performed in the hospital, it appears that there was a larger decrease in admissions related to asthma than for other diseases. Confirming the results on pharmaceuticals in Table 23, there is an increase in the number of prescriptions taken by moderate risk patients and a decrease in the number of high risk patients. The last row of Table 24 shows a large decline in the number of unique health care providers that a high risk patient saw in a typical month. This could be due to more "centralized" care for these patients, or the fact that they are in the hospital less frequently.

The extensive margin: It is also possible to consider the effect of the DM program on an individual's choice or need to obtain certain types of health care services. Thus, I turn to estimating the "first stage" of the two-part model: the extensive margin. It is well known that most nonlinear models with fixed effects are confounded by what is known as the 'incidental parameters problem' and can lead to biased estimation. (Chamberlain 1984, p. 1256; Lancaster 2000; Arellano and Honoré 2001, p. 3270; Wooldridge 2002, p. 484) However, I take advantage of a peculiar seperability in the log-likelihood function that exists in the specific case of the logit model. This allows one to estimate the effect of an independent variable separately from the problem of consistently estimating the fixed effect. (Chamberlain 1984; Arellano and Honoré

2001; Jones 2000, 6.3.2)[72] One must use a *conditional* logit model, where individuals are dropped if they do not have at least one month without an event and one month with an event.

In Table 25, I estimate conditional logit models with individual fixed effects for an array of independent binary variables. In columns 1, I find a 86 basis point decrease in the probability that an average individual will have a medical insurance claim in a given month. Thus, in addition to the second stage effect found above (where DM caused users of medical care to decrease the level of their spending), it appears DM actually causes a small number of individuals to switch to receiving *no health care*. This effect larger for the moderate risk group (column 2), which is the group that has, on average, lower health expenditures. If 70 percent of individuals have an expenditure in a quarter, and mean expenditures are about $1,500 per person per month for the high and moderate risk groups, this indicates that the first-stage effect of the program is nontrivial but modest, in the neighborhood of a $\left({}^{\$1,500}/_{70\%} * 0.86\% \approx\right)$ $20 per person per month decrease in expenditures.

The remaining columns in the table repeat the conditional logit analysis, estimating the effect of DM on the probability of an ED admissions, an inpatient admission, a prescription drug claim, or a physician office visit.

Columns 3 through 6 show large, statistically significant decreases in the probability of emergency department and inpatient admissions after receiving moderate and (especially) high intensity interventions. ED admission rates also fell dramatically, with the largest effect found among the high-risk group. Furthermore, assignment to receive high or moderate intensity DM tends to lower the probability of inpatient admissions by 3.6 and 13.8 percentage points, respectively, or 6.1 percentage points on average. This is a very significant reduction in the hospitalization rate, given that the mean hospitalization rate (for the individuals in the regression)

[72] In Appendix C, I discuss an alternative model: a random effects tobit model, which treats health expenditures as a censored dependent variable. As seen in the appendix, results with a Tobit model yield conclusions similar to the two-part model.

is 8.54 percent. These differences between the high and moderate risk groups in the estimated effect of DM on ED and inpatient admissions explain much of the difference we observed (between the two groups) for total expenditures. Some patients are less likely to fill a prescription or visit an outpatient provider.

Table 26 presents yet another piece of evidence that the DM program is working as intended, by measure the effect of DM on the probability of a "potentially avoidable" hospitalization. This variable identifies if the individual had at least one hospital claim during the month that is identified, according to the Prevention Quality Indicators (PQI) algorithm from AHRQ (2007b), as a hospitalization that could have potentially been avoided if the individual had received proper ambulatory health care.[73] The results indicate that the probability of a potentially avoidable admission was reduced by over 6 percentage points and is significant at the 1 percent level. Interestingly, the size of the effect is similar between the high- and moderate risks groups. The decrease in potentially avoidable hospitalizations explains much, but not all, of the decreases in total hospitalizations we observed above in Table 25.

Quarterly data: In Table 27, I made two changes to the data. First, I aggregate the data to the sum of expenditures for each individual in each quarter. This aggregation to person-quarter observations can address the concern that the results shown above are subject to random fluctuation ("noise") from month to month. Second, instead of running the data as a two-stage model, I add one dollar to the quarterly sum for each individual in the data before performing the log transformation. This allows the individuals with zero expenditure to remain in the regression. With quarterly data, there are very few individuals with zero total expenditures in a given quarter. These two steps make the results in Table 27 comparable to the primary specification in the study by Gertler and Simcoe (2009). I run this specification for the total expenditures, as well as the four subcategories of expenditures.

[73] The methodology is similar to that of Dafney and Gruber (2005), but is applicable to adults as well as children.

In column 1, I find a 40 percent (SE 2.9) decline in total expenditures from being promoted to the high and moderate risk groups and – consistent with the results above – the decline is larger for the high risk groups (49.8 percent) than for the moderate risk groups (33.6 percent). This effect is large, but is smaller in magnitude than the decline Gertler and Simcoe (2009) found for the low self-control diabetics in their study (over 50 log-point decrease in the first quarter of the program). In the remaining columns with ED, inpatient, outpatient, and prescription drug expenditures, the results show a decrease expenditures from the high and moderate intensity interventions. Of note, there is a 23.3 percent (SE 2.5) decline in emergency department expenditures for high risk patients.

3.6.2 The effect of intensive DM interventions over time

The results that I have discussed thus far estimate an impact of DM that is primarily identified by the (approximately) first eight months of the program. However, the specification in Section 3.5.2 allows us to estimate the time-related path of DM interventions on medical expenditures. In Figure 17, Panel A, I plot the coefficient estimates and 95 percent confidence intervals from a regression of log(total claims) on an array of dummies indicating months after entry into high and moderate groups, with month and individual fixed effects. One can see the high-risk interventions lead to a steady decrease in medical expenditures over time, decreasing to more than 10 percent below their baseline level of expenditures as the data reaches the maximum of 27 post-program months. As expected, the moderate risk group does not experience such a large decrease.

One problem with this graph is that there is some variation in the baseline (per-program) months of the data. To verify the magnitude of these declines, in Figure 17, Panel B, I drop the dummy variables for all months prior to entry into the high and moderate risk groups. The coefficients are therefore normalized such that 0 equals the average pre-program medical expenditure for the individual. The post-implementation drop in expenditures appears even larger for the high-risk patients and the decrease becomes statistically significant for moderate risk

patients. These graphs indicate that the program effect grows over time and is sustained. The magnitudes of the effect, as expected, is similar to those in Table 22. I conclude that the effect of DM appears in the first six to twelve months after the individual begins receiving DM interventions and these savings appear to be sustained over time (i.e., patients do not "revert" back to a high-expenditure regime at some later date).

3.6.3 The distributional effect of intensive DM interventions

Finally, I turn to quantile treatment effects of the DM program for those who were treated in the high and moderate risk groups. As discussed in Section 3.5.3, I use the Changes-in-Changes models from of Athey and Imbens (2006) on total medical expenditures (without and with control variables) and total inpatient expenditures. The results of this exercise are provided in both tabular (Table 28) and graphical format (Figure 18). The table provides bootstrapped standard errors. As discussed above, the data was restricted to the months January through September 2005 in the pre-period and October 2005 through June 2006 in the post period (9 months of pre and post data) and converted to two summary pre/post observations. Outcomes in these periods are presented as monthly averages to make it comparable to the data presented above.

Decreases in the levels of medical care costs are significantly larger at the right tail of the distribution, as seen Figure 18. In the baseline specification without controls, the 90th percentile saw a decrease of about $200 per member per month, while the 10th percentile decreased only $80 per member per month. (These are the percentile of the high and moderate risk patients, not the entire pool of eligible members).[74] The specifications with the logarithm of medical expenditures

[74] For the 95th percentile, there is actually an increase in expenditures, which contrasts with the findings from the rest of the distribution. This may be from the fact that the very "sickest" patients are "too sick" to recover or be influenced by DM. This may also be from the fact that there is a "stop-loss" provision in the contract between the state of Georgia and the DMO. Claims above and beyond $150,000 for an individual in a single year are removed from the financial calculations, leaving the DM company with little incentive to move patients below the stop-loss. As similar situation is discussed by McInerney (2010) for the case of a worker's compensation program.

show an opposite slope, with the largest proportional decrease of 18 percentage points at the 5th percentile. These two results are perfectly consistent with each other, once you take into account the highly skewed distribution of medical expenditures. While a decrease of $70 dollars per patient is smaller than the $200 decrease at the top of the distribution, this figure represents a larger share of the patient's expenditures due to the extremely large per-member monthly costs at the far right tail of the distribution. Similar results are obtained in the model that controls for various indicators, such as age, gender, and diseases.

For inpatient and emergency department expenditures, the high number of zeros precludes running this model without the log transformation, and even with the log transformation there is less statistical precision. However, we do observe a decrease in inpatient expenditures. The decrease in emergency department expenditures is mostly concentrated among the most expensive patients, as might have been expected. Outpatient expenditures decline more modestly and there is less of a trend (in dollar terms) between the right and left tails of the distribution.

3.7 Discussion

The primary limitation of the Georgia data set is that this approach does not allow estimation of the DM program's effect compared to no treatment because all individuals in the data receive treatment, at least at the low-risk group level. Therefore, I cannot claim to identify the total program effect, as the data does not include individuals who were never eligible to receive at least basic DM services; that is, I can only identify the effect of being "upgraded" to receive moderate or high risk group interventions. My results serve as a lower bound estimate of the full effect of the program as long as the "low" intensity interventions do not have an adverse effect and *increase* health care costs (direct program costs aside). Under the assumption that interventions for the low risk group had no effect, my estimates would approximate the total financial impact of the program.

There are no estimates of the marginal cost of increasing the intensity of DM interventions, for a given individual, from the low-risk group interventions to the level of interventions given to moderate or high risk group. Therefore, it is not possible to pair the results in this study with individual-level, heterogeneous program costs and conduct a return-on-investment analysis at the individual level. However, the magnitude of the results can be compared to overall program costs with a simple, back-of-the-envelope calculation.

Note that there are 3.2 low risk patients for every high/moderate risk patient in the program. Georgia's Medicaid program paid the DMO vendor approximately $30 for DM services, regardless of the individual's risk group. This study does not measure the financial savings for low risk patients that result from the DM interventions they receive (access to a "1-800" phone number and a program website, a few educational mailings, data monitoring). However, we can speculate that total savings for this group is likely to be limited because (i) they receive fewer DM interventions, (ii) this group has much lower costs to begin with, and (iii) the distributional analysis indicated that savings from intensive interventions was limited for low-cost patients at the left side of the distribution. If one assumes that there was zero financial savings to the Medicaid program for the low-risk group, then the program would need at least ($30)(1+3.2) = $126 in average savings (per month) from the individuals in the high and moderate risk groups to cover total program costs. Given that mean expenditures for the main estimation sample were about $1,250 PMPM in the baseline period, this translates to about 10 percent of monthly expenditures for the 17,000 that "should" have been assigned to the high and moderate risk groups. The main results in this chapter indicate savings at this order of magnitude, at least for the high risk group when receiving the high risk interventions. From the changes-in-changes model, we see that a decrease of $126 may have occurred for many of the most expensive patients in the high/moderate risk groups, but not all of them. (Although, the results with quarterly data in Table 27 imply substantial savings.) Of course, the fact that some individuals were mistakenly undertreated has a negative effect on the realized economic return of the program.

Of course, the financial savings to the state Medicaid program does not represent all the benefits to the program. Better management of a chronic disease is likely to increase the private utility of the individual and may have indirect benefits such as the labor productivity gains from better health (e.g., fewer sick days).

Theory suggests that there are diminishing returns to providing these high-cost services to lower risk (less sick) individuals. However, it should be noted that this is a relatively large DM program. This program in Georgia assigned about 25 percent of individuals to the high and moderate risk groups, even though it is not uncommon in the industry to limit the intensive DM services to less than 10 percent of the eligible population. (Abraham 2008) Thus, it is interesting that I find robust cost savings across the entire distribution of high and moderate cost patients in this setting. The question, "What was the effect of low-intensity DM services given to low risk patients?" is left to future work.

This chapter does make several improvements upon the existing literature that are worth highlighting. First, the main estimation sample consists of over 17,000 individuals who received services from this Medicaid DM program, much larger than the 826 individuals in the study by Holmes et al. (2008). The GEC program, with 45,000-50,000 eligible members each month is one of the largest programs evaluated in the DM literature. My main identification comes from a natural experiment where a large sub-sample of approximately 5,500 individuals were delayed introduction into the high and moderate risk groups for 9 to 10 months, while about 11,900 individuals were treated with these interventions. I am the first researcher to study the effect of DM using this idiosyncratic feature of the GEC program. Additional research, addressing issues far beyond the scope of this chapter, could be conducted in the future. Second, existing work almost exclusively focuses on the average effect of DM programs and rarely explores heterogeneous treatment effects. This chapter provides interesting evidence showing variation in the program effect across diseases and the expenditure distribution. Third, by exploiting an administrative error, I am able to use experimental methods to avoid sources of bias that

potentially plague the DM program evaluation literature: unobserved variables for patients who opt-in or opt-out of a program.

Tables

Table 1: Examples of Medicaid DM program costs, selected programs

Program	Notes	(1) Annual Budget	(2) Number of people	(3) Implied PMPM cost
Georgia Enhanced Care (10/2005)	Contracts awarded for $7.05 and $18.7 million for the north and south service regions, respectively. About 25 percent of individuals in the south region were assigned to the "high" and "moderate" risk groups.	$7,052,000 $18,726,000	49,483 48,400 12,100	*$12* *$32* *$129**
Missouri - Chronic Care Improvement Program (7/2006)	Budgeted $3.9 million for FY 2009 with 280,000 overall, 118,000 "Disease Management," 9,000 "Case Management" enrollees.	$3,900,000	280,000 118,000 9,000	*$1* *$3** *$36**
Montana - Nurse First program (1/2004)	Estimated annual costs for DM across all diseases was $1.9 million. 64,000 enrolled; 6,800 with more intensive DM.	$1,900,000	64,000 6,800	*$2* *$23**
Nebraska - Enhanced Care Coordination Program (9-2008)	$90.79 PMPM for 850 patients.	*$77,000*	850	$91
Rhode Island - Connect Care Choice (8/2007)	Cost estimate $30 PMPM for employing nurse case managers in provider offices.	-	-	$30
Virginia - Healthy Returns Care Management Program (1/2006)	One report's ROI estimate used estimate of $13.69 PMPM totaling $1.96 million.	$1,962,000	*11,948*	$14
Wyoming - Healthy Together Total Population Management Program (7/2004)	About $5.50 PMPM. 54,000 overall, 3,500 in "disease management", 520 in "care management"	*$297,000*	54,000 3,500 520	$6 $85* $571*

Numbers in *italics* calculated as column (1) = (2) × (3). *Total program costs divided by number of individuals in the sub-category.

Table 2: Summary Statistics for Medicaid enrollees, MEPS 1998-2007

	Has Top-5 Chronic Disease			No Top-5 Chronic Disease			
	Medicaid Adults	Medicaid Children	Dual Eligible	Medicaid Adults	Medicaid Children	Dual Eligible	All
Panel A - Individual-level data							
Individuals (N)	3,086	3,531	2,886	8,008	20,184	2,720	40,415
Male (percent)	25	57	35	29	49	40	42
Age at survey exit	41.84	9.29	64.73	34.06	8.60	52.24	23.19
	[0.18]	[0.07]	[0.23]	[0.18]	[0.02]	[0.34]	[0.12]
SSI income (percent)	33.00	7.00	43.00	14.00	3.00	35.00	13.00
Education (percent)							
No degree	42.13	9.01	58.45	41.62	7.57	47.32	23.39
GED	10.73	0.11	5.02	7.38	0.12	3.49	2.94
High school diploma	39.70	0.34	25.81	42.45	0.36	25.29	15.19
Bachelor's degree	2.98	0.00	3.47	3.23	0.00	2.57	1.29
Graduate degree	0.91	0.00	1.59	0.76	0.00	1.36	0.43
Under-16/other/missing	3.56	90.54	5.65	4.56	91.96	19.96	56.76
Race/Ethnicity (percent)							
White (not Hispanic)	39.34	27.36	42.00	32.16	24.99	36.32	29.69
Black	28.09	33.22	27.44	25.07	24.69	25.63	26.03
Hispanic	27.09	34.07	24.81	37.08	45.37	30.81	38.90
Asian or Pacific Island	2.30	1.67	3.92	3.77	2.37	5.70	2.91
Native American	1.30	0.85	0.66	0.89	0.91	0.48	0.88
Other/multiple races	1.88	2.83	1.18	1.04	1.67	1.07	1.59
Chronic diseases (per 1000)							
Asthma	545.69	972.81	311.50				148.91
COPD	94.94	5.10	147.61				18.24
Diabetes	370.06	19.82	509.70				66.39
CHF	290.02	11.05	429.66				53.79
CAD	130.27	3.96	292.79				31.20
Insurance							
Medicaid							
Percent of months insured	74.42	83.25	78.61	66.88	79.00	74.48	76.29
	[0.31]	[0.25]	[0.91]	[0.48]	[0.28]	[0.6]	[0.16]
		(18.405)	(5.865)	(14.124)	(9.281)	(0.122)	
Continuously insured (percent)	48.70	59.59	53.08	37.86	51.63	46.91	49.16
		(9.622)	(6.477)	(8.281)	(2.625)	(2.212)	
Medicaid and Medicare							
Percent of months insured			77.21			73.35	10.45
			[0.79]			[0.62]	[0.16]
Continuously insured (percent)			50.83			45.37	6.68

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Table 2: Summary Statistics for Medicaid enrollees, MEPS 1998-2007 (continued)

	Has Top-5 Chronic Disease			No Top-5 Chronic Disease			
	Medicaid Adults	Medicaid Children	Dual Eligible	Medicaid Adults	Medicaid Children	Dual Eligible	All
Panel B - Person-quarter data							
Person-quarters (N)	23,260	25,376	21,048	59,732	141,124	18,912	289,452
Medical expenditures (2005 dollars)							
Total medical expenditures (mean)	1,736.17	411.27	3,242.59	678.91	220.76	1,414.14	751.49
	[36.78]	[28.24]	[36.91]	[17.65]	[5.99]	[56.91]	[14.94]
		(26.322)	(55.825)	(40.473)	(47.663)	(6.401)	
$0 (percent)	21.50	37.78	8.86	48.65	58.42	29.61	46.14
$.01 to $99 (percent)	9.90	20.20	5.11	12.49	18.23	11.14	15.13
$100 to $499(percent)	23.51	27.16	17.81	19.96	17.48	23.73	19.76
$500 to $999 (percent)	14.03	7.38	16.45	7.04	2.67	12.12	6.52
$1,000 to $4,999 (percent)	23.81	6.32	35.76	8.95	2.59	17.06	9.29
$5,000 or above (percent)	7.24	1.15	16.01	2.92	0.61	6.34	3.16
Expenditures paid by Medicaid	1,227.43	301.25	1,004.62	443.80	158.95	485.15	398.87
	[28.15]	[28.97]	[18.19]	[4.42]	[5.16]	[18.74]	[6.42]
		(20.482)	(6.112)	(30.082)	(44.467)	(20.955)	
Percent > $0	63.77	52.65	71.91	37.61	33.94	51.23	42.62
Expenditures paid by Medicare			1,795.68			704.36	
			[37.15]			[35.03]	
Percent > $0			73.74			48.22	
Emergency department expenditures	69.78	25.04	64.40	30.34	12.73	28.69	26.83
	[1.98]	[1.02]	[1.03]	[0.84]	[0.37]	[1.18]	[0.53]
Percent > $0	11.11	6.81	10.00	5.59	3.66	5.35	5.50
Hospital inpatient expenditures	660.85	109.40	1,235.17	293.87	79.19	476.83	282.92
	[17.02]	[14.36]	[26.18]	[18.03]	[4.45]	[27.86]	[6.43]
Percent > $0	6.68	1.81	9.71	4.03	0.94	4.20	2.97
Prescribed medicines expenditures	463.68	93.48	742.72	106.90	24.22	280.24	151.64
	[15.21]	[2.81]	[28.75]	[1.57]	[0.64]	[7.19]	[4.49]
Percent > $0	69.05	44.62	84.81	35.54	19.56	56.81	36.21
Outpatient expenditures	111.11	23.53	164.04	45.19	15.00	74.87	44.45
	[3.92]	[0.65]	[20.67]	[3.38]	[0.32]	[4.61]	[0.79]
Percent > $0	9.79	2.94	11.97	4.46	1.61	6.98	4.08
Office-based provider expenditures	303.81	85.37	441.15	140.52	50.08	217.90	131.62
	[11.71]	[5.03]	[23.16]	[6.06]	[1.38]	[10.24]	[2.86]
Percent > $0	55.10	38.85	70.92	34.05	27.78	50.21	36.84
Home health expenditures	81.57	42.97	519.34	33.90	13.49	289.85	80.60
	[10]	[7.5]	[36.6]	[3.07]	[0.61]	[19.86]	[3.2]
Percent > $0	2.85	0.86	15.94	1.12	0.42	8.11	2.43
Dental expenditures	22.92	24.70	21.85	19.15	21.30	21.93	21.37
	[1.36]	[1.09]	[1.24]	[0.51]	[0.31]	[1.3]	[0.23]
Percent > $0	8.44	12.86	7.16	7.78	10.69	8.03	9.67
Other medical expenses	22.45	6.77	53.93	9.04	4.75	23.85	12.06
	[2.36]	[0.16]	[1.71]	[0.46]	[0.21]	[0.9]	[0.33]
Percent > $0	7.33	3.72	10.48	3.93	2.26	5.80	3.97

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Table 2: Summary Statistics for Medicaid enrollees, MEPS 1998-2007 (continued)

	Has Top-5 Chronic Disease			No Top-5 Chronic Disease			
	Medicaid Adults	Medicaid Children	Dual Eligible	Medicaid Adults	Medicaid Children	Dual Eligible	All
Panel B - Person-quarter data (continued)							
E.D., inpatient, and outpatient events for selected diseases							
(number of events with diagnosis code per quarter per 1,000 patients with the disease)*							
Asthma	48.44	34.80	50.93				41.22
	[7.19]	[4.52]	[7.35]				[4.61]
COPD	82.68	66.67	82.79				82.38
	[13.67]	[15.86]	[7.91]				[1.79]
Diabetes	98.07	142.56	97.75				99.00
	[3.34]	[23.41]	[15.01]				[9.93]
Chronic Heart Failure (CHF)	25.40	13.39	40.93				34.07
	[1.47]	[10.4]	[3.58]				[2.21]
Coronary Artery Disease (CAD)	78.86	41.67	53.85				61.77
	[15.4]	[24.2]	[6.88]				[9.3]
Medicaid Disease Management Coverage (percent of person-months)**							
DM program or pilot in state/county	31.93	36.70	35.85	27.77	33.63	30.90	32.54
State/county has DM for Medicaid	23.90	31.59		18.46	28.11		22.26
State/county has DM for Medicaid***	13.30	18.04		10.20	16.16		12.66
State/county has DM for Dual			2.50			1.20	0.26
DM matching disease/insurance	17.47	23.58	1.85				3.61
DM matching disease/insurance***	11.65	16.41	1.71				2.50

Source: Author's tabulations of MEPS data. Table presents unweighted means (or percentages, where noted) for MEPS respondents who receive Medicaid in at least one quarter (1998-2007). Standard errors (in brackets) are clustered by MEPS PSU. T-statistics (in parentheses) shown for test that the mean is equal to the mean of Medicaid adults with a chronic illness. Columns separate groups by Medicare coverage, aged less than 18 on December 31, and presence of a top-5 chronic disease (asthma, COPD, diabetes, CAD, and/or CHF). The categories are mutually exclusive; I cut the sample by (1) having a chronic-illness, then by (2) dual-eligibility, then by (3) age group. (i.e., "Medicaid adults" does not include dual eligibles.) *Hospital event rates are calculated as mean number of events per 1000 patients. The denominator is patients who are ever diagnosed with the disease or provide an affirmative answer to MEPS survey. **Disease Management data, from author's survey of Medicaid disease management programs, was merged with MEPS as described in text. Rows marked with three asterisks (***) refer to the subset of DM programs that meet the strict definition of DM (see text for details).

Table 3: Weighted estimates of DM coverage, MEPS 1998-2007

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Number of individuals matched to a DM program for their top-5 disease				Percent of individuals with a top-5 disease			Percent of all Medicaid enrollees
	Medicaid Adults	Medicaid Children	Dual Eligible	Total	Medicaid Adults	Medicaid Children	Dual Eligible	
1998q1	22,518	0	0	22,518	1.2%	0.0%	0.0%	0.1%
1998q3	31,663	133,770	0	165,433	1.7%	7.2%	0.0%	0.4%
1998q4	46,645	157,023	0	203,668	2.4%	8.5%	0.0%	0.5%
1999q1	125,425	193,471	4,591	323,487	4.5%	7.7%	0.2%	0.8%
1999q2	125,425	225,181	4,591	355,197	4.5%	9.0%	0.2%	0.9%
1999q3	148,909	225,181	4,591	378,681	5.4%	9.0%	0.2%	1.0%
1999q4	145,970	211,642	0	357,612	5.3%	8.5%	0.0%	0.9%
2000q1	200,723	279,030	0	479,753	5.1%	8.4%	0.0%	1.2%
2000q2	299,256	267,497	0	566,753	7.6%	8.0%	0.0%	1.4%
2000q3	299,256	267,497	0	566,753	7.6%	8.0%	0.0%	1.4%
2000q4	315,328	305,026	0	620,354	8.0%	9.1%	0.0%	1.5%
2001q1	283,172	351,616	4,879	639,667	6.6%	9.2%	0.1%	1.5%
2001q2	310,483	342,635	4,879	657,997	7.3%	9.0%	0.1%	1.5%
2001q3	320,387	341,404	4,879	666,670	7.5%	9.0%	0.1%	1.5%
2001q4	261,739	316,503	4,879	583,121	6.1%	8.3%	0.1%	1.3%
2002q1	377,086	440,000	5,256	822,342	7.7%	11.2%	0.1%	1.8%
2002q2	389,163	549,516	1,753	940,432	8.0%	14.0%	0.0%	2.0%
2002q3	415,513	665,190	1,753	1,082,456	8.5%	17.0%	0.0%	2.3%
2002q4	522,927	659,509	79,834	1,262,270	10.7%	16.8%	2.1%	2.7%
2003q1	616,526	715,519	84,856	1,416,901	12.4%	18.1%	2.1%	2.9%
2003q2	619,718	758,543	107,951	1,486,212	12.5%	19.1%	2.7%	3.0%
2003q3	696,109	776,749	101,406	1,574,264	14.0%	19.6%	2.5%	3.2%
2003q4	697,312	763,388	96,221	1,556,921	14.0%	19.3%	2.4%	3.2%
2004q1	774,456	698,862	108,841	1,582,159	15.1%	17.2%	2.6%	3.1%
2004q2	742,326	727,153	121,642	1,591,121	14.5%	17.9%	3.0%	3.1%
2004q3	867,296	773,024	131,546	1,771,866	16.9%	19.0%	3.2%	3.5%
2004q4	971,680	957,376	92,222	2,021,278	19.0%	23.5%	2.2%	4.0%
2005q1	1,458,587	1,295,515	170,768	2,924,870	28.4%	30.5%	3.9%	5.6%
2005q2	1,502,030	1,323,694	163,429	2,989,153	29.2%	31.1%	3.7%	5.7%
2005q3	1,589,040	1,379,086	166,346	3,134,472	30.9%	32.4%	3.8%	6.0%
2005q4	1,498,182	1,503,211	164,568	3,165,961	29.1%	35.4%	3.7%	6.0%
2006q1	1,312,255	1,748,468	101,905	3,162,628	28.0%	38.9%	2.4%	6.1%
2006q2	1,444,931	1,804,262	109,399	3,358,592	30.9%	40.1%	2.5%	6.4%
2006q3	1,399,786	1,772,637	105,830	3,278,253	29.9%	39.4%	2.4%	6.3%
2006q4	1,271,745	1,745,100	105,830	3,122,675	27.2%	38.8%	2.4%	6.0%
2007q1	1,606,518	1,755,551	113,200	3,475,269	34.2%	44.9%	2.7%	6.8%
2007q2	1,615,744	1,743,875	78,739	3,438,358	34.4%	44.6%	1.8%	6.7%
2007q3	1,943,250	1,913,895	86,606	3,943,751	41.4%	49.0%	2.0%	7.7%
2007q4	1,787,473	1,890,496	78,619	3,756,588	38.0%	48.4%	1.8%	7.3%

Source: Author's tabulations of MEPS data. Disease Management data, from author's survey of Medicaid disease management programs, was merged with MEPS as described in text. Columns 1 to 4 present estimates of the *weighted* number of people who are matched to a DM program using my DM_{it} coverage variable (see text for details). In the columns 5 to 7, I divide this by the number of individuals (in the respective) category who have a top-5 chronic condition. In column 8, I divide column 4 by the estimated number of people with Medicaid in the quarter. All columns weight observations using the MEPS survey weights. Columns 1, 2, and 3 are plotted in **Figure 8**.

Table 4: Effect of Medicaid DM on quarterly health expenditures

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variable	Total medical expenditures				Medical expenditures paid by Medicaid			
Panel A: Marginal effect from stage 1 of two-part model								
Marginal effect	0.0148*	0.0146*	0.0119	0.00459	0.0503***	0.0488***	0.0457***	0.0396***
	[0.00875]	[0.00882]	[0.00890]	[0.00997]	[0.00867]	[0.00875]	[0.00887]	[0.00968]
ATET, stage 1	0.0111	0.0110	0.0089	0.0035	0.0429	0.0416	0.0388	0.0341
Panel B: Marginal effect from stage 2 of two-part model								
Marginal effect	-86.69	-99.79	-79.80	-163.1***	-8.520	-30.33	-4.296	-58.39
	[60.02]	[60.98]	[52.16]	[57.68]	[54.46]	[55.55]	[44.68]	[53.61]
ATET, stage 2	-132.75	-157.00	-130.6	-266.90	-12.72	-47.16	-6.86	-90.76
Panel C: Implied marginal effect in two-part model								
Marginal effect	-33.46	-40.98	-32.95	-86.63	34.77	24.81	31.65	6.91
ATET	-97.29	-117.21	-98.08	-215.71	31.82	5.66	33.16	-34.38
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year * quarter FE	Yes	Yes		Yes	Yes	Yes		Yes
Panel FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State time trends		Yes				Yes		
State * year FE			Yes				Yes	
Year FE			Yes				Yes	
Quarter of year FE			Yes				Yes	
DM Definition				Strict				Strict
N of obs., stage 1	289,452	289,452	289,452	289,452	289,452	289,452	289,452	289,452
N of individuals, stage 1	40,419	40,419	40,419	40,419	40,419	40,419	40,419	40,419
N of obs., stage 2	155,901	155,901	155,901	155,901	123,371	123,371	123,371	123,371
N of individuals, stage 2	36,618	36,618	36,618	36,618	33,730	33,730	33,730	33,730

Notes: Panels A and B present marginal effects of DM on expenditures from two separate GLM regressions calculated as (i) the marginal effect (at the mean of the other independent variables) and (ii) the average treatment effect for the treated observations (ATET). The DM independent variable equals one if an observation is matched to a DM program in the individual's state/county based on their chronic disease(s) and insurance, and zero otherwise. In panel A, the regression estimates the effect of DM on $\Pr(y_{it} > 0|DM_{it}, z_{it})$ using a Bernoulli distribution and a probit link function. In panel B, the regression estimates the effect of DM on $\mathrm{E}(y_{it}|y_{it} > 0, DM_{it}, z_{it})$ using a gamma distribution and a log link function. Robust standard errors (in brackets) are based on exchangeable within-individual correlation structure. In addition to fixed effects, these models include dummies for gender; age; highest education completed; race/ethnicity; diagnosis/reporting of asthma, COPD, diabetes, chronic heart failure, and/or coronary arty disease; and insurance coverage by Medicaid and/or Medicare in the quarter. The data consists of all available quarterly observations for individuals in the MEPS, 1998-2007, who receive Medicaid in at least one quarter. ***p<0.01, **p<0.05, *p<0.10.

Bootstrapped standard errors, after 125 iterations, for column 1 are **[32.39]** for the stage-1 ATET, **[0.00703]** for the stage-2 ATET, **[87.59]** for the combined marginal effect, and **[72.41]** for the combined ATET.

Regression coefficients and marginal effects for all control variables are provided in **Appendix Table B1** for columns 1, 3, 5, and 7.

Table 5: Effect of Medicaid DM on quarterly health expenditures, with disease-time controls

	(1)	(2)†	(3)†	(4)
Dependent variable		**Total medical expenditures**		
Panel A: Marginal effect from stage 1 of two-part model				
Marginal effect	0.000627	0.00260	0.00198	0.00395
	[0.00945]	[0.00953]	[0.00950]	[0.00953]
ATET, stage 1	0.000464	0.00192	0.00147	0.00293
Panel B: Marginal effect from stage 2 of two-part model				
Marginal effect	-133.60**	-91.32*	-110.09*	-105.29*
	[65.17]	[52.33]	[56.43]	[56.87]
ATET, stage 2	-218.25	-146.50	-178.20	-171.20
Panel C: Implied marginal effect in two-part model				
Marginal effect	-74.44	-48.74	-59.88	-55.20
ATET	-182.1	-120.78	-147.61	-140.15
State FE	Yes	Yes	Yes	Yes
Year * quarter FE	Yes		Yes	Yes
Panel FE	Yes	Yes	Yes	Yes
Disease * insurance FE	Yes	Yes	Yes	Yes
Disease time trends	Yes			
Year FE		Yes		
Qtr of Year FE		Yes		
Disease * year * quarter FE		Yes		
Disease * year FE			Yes	Yes
Disease * qtr of year FE			Yes	Yes
Insurance * year FE				Yes
N of obs., stage 1	289,452	289,452	289,452	289,452
N of individuals, stage 1	40,419	40,419	40,419	40,419
N of obs., stage 2	155,901	155,901	155,901	155,901
N of individuals, stage 2	36,618	36,618	36,618	36,618

Notes: Panels A and B present marginal effects of DM on expenditures from two separate GLM regressions calculated as (i) the marginal effect (at the mean of the other independent variables) and (ii) the average treatment effect for the treated observations (ATET). The DM independent variable equals one if an observation is matched to a DM program in the individual's state/county based on their chronic disease(s) and insurance, and zero otherwise. In panel A, the regression estimates the effect of DM on $\Pr(y_{it} > 0 | DM_{it}, z_{it})$ using a Bernoulli distribution and a probit link function. In panel B, the regression estimates the effect of DM on $\mathrm{E}(y_{it} | y_{it} > 0, DM_{it}, z_{it})$ using a gamma distribution and a log link function. Robust standard errors (in brackets) are based on exchangeable within-individual correlation structure. In addition to fixed effects, these models include dummies for gender; age; highest education completed; race/ethnicity; diagnosis/reporting of asthma, COPD, diabetes, chronic heart failure, and/or coronary arty disease; and insurance coverage by Medicaid and/or Medicare in the quarter. The data consists of all available quarterly observations for individuals in the MEPS, 1998-2007, who receive Medicaid in at least one quarter. ***p<0.01, **p<0.05, *p<0.10. †Convergence in stage-2 with lower precision.

Table 6: Effect of Medicaid DM on quarterly health expenditures, with alternative data samples

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Dependent variable	**Total medical expenditures**								
Panel A: Marginal effect from stage 1 of two-part model									
Marginal effect	0.0216*	0.00499	-0.0105	0.0139	-0.000912	0.0216*	.00585	-.00320	.0106
	[0.0128]	[0.0121]	[0.0137]	[0.00942]	[0.00731]	[0.0128]	[.00836]	[.00944]	[.00852]
ATET, stage 1	0.0131	0.00369	-0.00776	0.0105	-0.000993	0.0131	.00477	-.00263	.00865
Panel B: Marginal effect from stage 2 of two-part model									
Marginal effect	-94.79	-51.61	-140.81***	-20.82	-135.85	-94.79	-173.84**	-234.11***	-138.33*
	[106.1]	[51.73]	[48.72]	[49.73]	[111.38]	[106.14]	[80.10]	[72.87]	[76.53]
ATET, stage 2	-133.4	-85.39	-246.29	-32.15	-122.87	-133.42	-216.90	-301.43	-169.59
Panel C: Implied marginal effect in two-part model									
Marginal effect	-28.96	-27.25	-96.11	1.18	-111.89	-28.96	-113.20	-166.79	-82.54
ATET	-92.94	-70.18	-218.75	-16.25	-101.93	-92.94	-176.54	-253.58	-133.08
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year * quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Panel FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Disease * insurance FE							Yes	Yes	Yes
Disease * year FE									Yes
Disease * qtr of year FE									Yes
Insurance * year FE									Yes
Data Sample	Quarters with Medicaid	Continuously enrolled in Medicaid	Continuously enrolled in Medicaid	Individuals enrolled in Medicaid ≥50% of time	Medicaid, with Top-5 disease	Medicaid, Adults (≥18 years)	Top-5 disease and/or Medicaid	Top-5 disease and/or Medicaid	Top-5 disease and/or Medicaid
DM Definition			Strict					Strict	
N of obs., stage 1	122,972	140,480	140,480	226,132	69,684	122,972	479,376	479,376	479,376
N of individuals, stage 1	17,353	19,867	19,867	31,599	9,503	17,353	66,423	66,423	66,423
N of obs., stage 2	81,634	81,451	81,451	126,242	53,230	81,634	308,812	308,812	308,812
N of individuals, stage 2	15,873	18,178	18,178	28,922	9,286	15,873	61,884	61,884	61,884

Notes: Panels A and B present marginal effects of DM on expenditures from two separate GLM regressions calculated as (i) the marginal effect (at the mean of the other independent variables) and (ii) the average treatment effect for the treated observations (ATET). The DM independent variable equals one if an observation is matched to a DM program in the individual's state/county based on their chronic disease(s) and insurance, and zero otherwise. $DM_{it} = 0$ for non-Medicaid observations. In panel A, the regression estimates the effect of DM on $\Pr(y_{it} > 0 | DM_{it}, z_{it})$ using a Bernoulli distribution and a probit link function. In panel B, the regression estimates the effect of DM on $\mathrm{E}(y_{it} | y_{it} > 0, DM_{it}, z_{it})$ using a gamma distribution and a log link function. Robust standard errors (in brackets) are based on exchangeable within-individual correlation structure. In addition to fixed effects, these models include dummies for gender; age; highest education completed; race/ethnicity; diagnosis/reporting of asthma, COPD, diabetes, chronic heart failure, and/or coronary arty disease; and insurance coverage by Medicaid and/or Medicare in the quarter. The data consists of quarterly observations from the MEPS, 1998-2007, where individuals meet the criteria in the table's "Data Sample" row. ***p<0.01, **p<0.05, *p<0.10.

Table 7: Effect of Medicaid DM on quarterly health expenditures, additional robustness checks

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Model	**OLS** (regression coefficients)			**GLM** (marginal effect at mean)			
Dependent Variable	y_{it}	$\mathbf{1}(y_{it} > 0)$	$(y_{it}\|y_{it} > 0)$	$\mathbf{1}(y_{it} > 0)$	$(y_{it}\|y_{it} > 0)$	$\mathbf{1}(y_{it} > 5{,}000)$	$\mathbf{1}(y_{it} > 1{,}000)$
DM	-56.48	0.0348***	-98.49	.01275	-82.63	-.0002658	.0066172*
	[68.77]	[0.00728]	[92.21]	[.01114]	[67.34]	[.00136]	[.00381]
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year * quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Panel FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
MEPS survey weights				Yes	Yes		
GLM link function				Probit	Log	Probit	Probit
GLM error distribution				Bernoulli	Gamma	Bernoulli	Bernoulli
N of obs.	289,452	289,452	289,452	289,452	289,452	289,452	289,452
N of individuals	40,419	40,419	40,419	40,419	40,419	40,419	40,419

Notes: This table presents the effects of DM on total medical expenditures (y_{it}). The DM independent variable equals one if an observation is matched to a DM program in the individual's state/county based on their chronic disease(s) and insurance, and zero otherwise. Columns (1) and (2) present regression coefficients (and roust standard errors, clustered by individual, in brackets) from a simple OLS regression. Columns (4) thru (7) present the marginal effect (at the mean of the other independent variables) from a GLM model. Robust standard errors (in brackets) are based on exchangeable within-individual correlation structure and the link functions and error distributions are indicated in the table. In addition to fixed effects, all regressions include dummies for gender; age; highest education completed; race/ethnicity; diagnosis/reporting of asthma, COPD, diabetes, chronic heart failure, and/or coronary arty disease; and insurance coverage by Medicaid and/or Medicare in the quarter. The data consists of all available quarterly observations for individuals in the MEPS, 1998-2007, who receive Medicaid in at least one quarter. ***p<0.01, **p<0.05, *p<0.10.

Table 8: Effect of Medicaid DM on quarterly health expenditures, OLS models with additional arrays of fixed effects as control variables

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
DM	-56.48	-127.2	-76.77	-80.74	-65.38	-94.94	-93.99	-90.47	-109.2	69.22
	[68.77]	[85.41]	[70.00]	[70.37]	[73.34]	[75.65]	[74.57]	[73.21]	[72.52]	[122.4]
State FE	Yes	Yes			Yes					
State-year FE			Yes					Yes		
State-year-quarter FE				Yes		Yes	Yes			
Disease-insurance FE					Yes	Yes	Yes	Yes	Yes	
Disease-year-quarter FE					Yes	Yes	Yes			
Disease-year FE									Yes	
Disease-quarter of year FE									Yes	
Insurance-year-quarter FE							Yes			
Disease-insurance-year FE								Yes		Yes
State-insurance-year FE									Yes	Yes
State-disease-year FE										Yes
Year-quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Panel FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sample		Contin. enrolled								
N of obs.	289,452	140,480	289,452	289,452	289,452	289,452	289,452	289,452	289,452	289,452
N of individuals	40,419	19867	40,419	40,419	40,419	40,419	40,419	40,419	40,419	40,419

Notes: This table presents OLS regression coefficients for the effect of DM on total medical expenditures. The DM independent variable equals one if an observation is matched to a DM program in the individual's state/county based on their chronic disease(s) and insurance, and zero otherwise. Robust standard errors (in brackets) are clustered at the individual level. In addition to fixed effects (shown), all regressions include dummies for gender; age; highest education completed; race/ethnicity; diagnosis/reporting of asthma, COPD, diabetes, chronic heart failure, and/or coronary arty disease; and insurance coverage by Medicaid and/or Medicare in the quarter. The data consists of all available quarterly observations for individuals in the MEPS, 1998-2007, who receive Medicaid in at least one quarter. ***p<0.01, **p<0.05, *p<0.10.

Table 9: Effect of Medicaid DM on quarterly health expenditures, by DM target disease

	(1)	(2)†	(3)	(4)
Dependent Variable		**Total medical expenditures**		
Panel A: Marginal effect from stage 1 of two-part model				
DM	0.104***	0.102***	0.103***	0.0924**
	[0.0393]	[0.0383]	[0.0380]	[.0371]
Asthma DM	-0.116***	-0.111***	-0.113***	-.102
	[0.0426]	[0.0416]	[0.0413]	[.0489]
COPD DM	0.0342	0.0266	0.0281	.0387
	[0.0806]	[0.0770]	[0.0765]	[.0735]
Diabetes DM	-0.0593	-0.0593	-0.0614	-.0576
	[0.0464]	[0.0460]	[0.0456]	[.0516]
CHF DM	-0.0397	-0.0370	-0.0365	-.0508
	[0.0423]	[0.0413]	[0.0409]	[.0463]
CAD DM	-0.0966	-0.0860	-0.0881	-.0993
	[0.0617]	[0.0635]	[0.0621]	[.0681]
Panel B: Marginal effect from stage 2 of two-part model				
DM	-255.99**	-260.28	-277.6**	-329.53**
	[108.75]	[95.73]	[109.4]	[142.85]
Asthma DM	97.75	170.76	176.1	160.33
	[176.3]	[152.16]	[181.0]	[236.78]
COPD DM	222.35	320.54	146.3	205.70
	[301.35]	[307.96]	[293.3]	[414.06]
Diabetes DM	330.09	352.56	367.0*	387.31
	[206.17]	[175.29]	[217.4]	[272.85]
CHF DM	13.90	31.91	28.91	48.97
	[114.9]	[111.13]	[115.6]	[147.91]
CAD DM	56.64	45.51	78.68	123.22
	[202.15]	[182.31]	[201.4]	[257.84]
Panel C: Implied marginal effect in two-part model				
DM	-37.46	-43.05	-51.78	-109.84
Asthma DM	-63.66	-16.79	-15.56	-20.46
COPD DM	159.79	207.22	110.7	192.83
Diabetes DM	125.02	138.30	144.0	194.89
CHF DM	-32.69	-19.47	-20.77	-31.57
CAD DM	-66.68	-61.50	-45.17	-42.44
State FE	Yes	Yes	Yes	Yes
Year * quarter FE	Yes		Yes	Yes
Panel FE	Yes	Yes	Yes	Yes
Disease * insurance FE	Yes	Yes	Yes	Yes
Disease time trends	Yes			
Year FE		Yes		
Qtr of Year FE		Yes		
Disease * year * quarter FE		Yes		
Disease * year FE			Yes	Yes
Disease * qtr of year FE			Yes	Yes
Data Sample	Medicaid	Medicaid	Medicaid	Top-5 disease and/or Medicaid
N of obs., stage 1	289,452	289,452	289,452	479,376
N of individuals, stage 1	40,419	40,419	40,419	66,423
N of obs., stage 2	155,901	155,901	155,901	308,812
N of individuals, stage 2	36,618	36,618	36,618	61,884

Notes: Panels A and B present marginal effects of DM (at the mean) on expenditures from two separate GLM regressions. The DM independent variable equals one if an observation is matched to a DM program in the individual's state/county based on their chronic disease(s) and insurance, and zero otherwise. The "X DM" variable equals one if this match between the individual's disease and a program's target disease was for disease "X" ("DM" equals one whenever one of the disease-specific variables equal one). Aside from fixed effects (shown above) and data sample, the models and control variables are the same as in Table 4. ***p<0.01, **p<0.05, *p<0.10. †Convergence in stage-2 with lower precision.

Net treatment effects for each of the diseases are plotted in **Figure 9**.

Table 10: Effect of Medicaid DM on quarterly health expenditures on dual eligibles and comorbid enrollees

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent Variable				**Total medical expenditures**				
Panel A: Marginal effect from stage 1 of two-part model								
DM * Comorbidity	0.0135	†		-0.0268	†			
	[0.0319]			[.0327]				
DM		0.00937	0.01533*		0.00287	[0.00220]	0.0149*	0.00231
		[0.00910]	[0.00886]		[0.00979]	[.00967]	[0.00880]	[0.0096]
Comorbidity		-0.0894***						
		[0.0171]						
DM ≥2 diseases			-0.00920			-0.0348		
			[0.0366]			[0.0364]		
DM * Dual-eligible							†	†
Panel B: Marginal effect from stage 2 of two-part model								
DM * Comorbidity	19.71	†		0.71	†			
	[113.2]			[97.60]				
DM		-118.8*	-109.95*		-125.67**	-127.15**	-90.31	-120.58**
		[67.87]	[60.18]		[63.94]	[58.99]	[61.99]	[58.42]
Comorbidity		-58.50						
		[85.02]						
DM ≥2 diseases			181.68			197.97		
			[169.20]			[151.41]		
DM * Dual-eligible							†	†
Panel C: Implied marginal effect in two-part model								
DM * Comorbidity	24.79	†		-26.80	†			
DM		-56.89	-45.89		-67.71	-69.22	-35.31	-65.44
Comorbidity		-124.1						
DM ≥2 diseases			92.32			75.92		
DM * Dual-eligible							†	†
ATET, stage 1	0.00414	0.0101†	-0.00269‡	-0.00817	-0.00867†	-0.0103‡	-0.00428†	-0.00486
ATET, stage 2	69.63	487.4†	603.7‡	2.70	432.18†	678.26‡	313.04†	763.66
ATET	78.03	491.0†	565.20‡	-21.90	382.41†	611.76‡	285.26†	717.09
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year * quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Panel FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE for comorbid				Yes	Yes	Yes		
Disease * insurance FE				Yes	Yes	Yes		Yes
Disease-Year FE				Yes	Yes	Yes		Yes
Disease-Qtr of Year FE				Yes	Yes	Yes		Yes
N of obs., stage 1	289,452	289,452	289,452	289,452	289,452	289,452	289,452	289,452
N of ind., stage 1	40,419	40,419	40,419	40,419	40,419	40,419	40,419	40,419
N of obs., stage 2	155,901	155,901	155,901	155,901	155,901	155,901	155,901	155,901
N of ind., stage 2	36,618	36,618	36,618	36,618	36,618	36,618	36,618	36,618

Notes: Panels A and B present marginal effects of DM (at the mean) on expenditures from two separate GLM regressions. The DM independent variable equals one if an observation is matched to a DM program in the individual's state/county based on their chronic disease(s) and insurance, and zero otherwise. Aside from fixed effects (shown above) and data sample, the models and control variables are the same as in Table 4. ***p<0.01, **p<0.05, *p<0.10.

†For interaction- term treatment variable, average incremental effects on the treated calculated according to the formula in footnote 35. ‡ATET for the "DM ≥2 diseases" variable.

Table 11: Effect of Medicaid DM on quarterly health expenditures (all payers), by type of service

Dependent variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Total medical expenditures (by service type)							
	Office-Based Medical Provider Visits	**Prescribed Medicines**	**Emergency Department Visits**	**Inpatient Hospital Events**	**Outpatient Hospital Events**	**Home Health**	**Dental and Vision**	**Other Medical Expenses**
Panel A: Marginal effect from stage 1 of two-part model								
Marginal effect	0.00505	0.0270***	0.00184	-0.00118	0.00190	0.00288**	0.00180	0.00369**
	[0.00732]	[0.00849]	[0.00239]	[0.00130]	[0.00203]	[0.00137]	[0.00388]	[0.00188]
ATET, stage 1	0.00498	0.0239	0.00304	-0.00246	0.00318	0.00667	0.00206	0.00595
Panel B: Marginal effect from stage 2 of two-part model								
Marginal effect	-19.95	7.01	18.75	-241.6	33.25	1,040**	3.38	6.76
	[15.44]	[9.167]	[31.15]	[769.7]	[125.2]	[494.8]	[12.94]	[24.61]
ATET, stage 2	-26.35	13.17	18.72	-210.2	35.29	834.9	3.47	6.44
Panel C: Implied marginal effect in two-part model								
Marginal effect	-5.75	8.96	1.74	-14.64	2.69	15.51	0.64	1.07
ATET	-13.36	17.91	3.45	-30.80	5.69	40.89	0.82	2.14
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year * quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Panel FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N of obs., stage 1	289,452	289,452	289,452	289,452	289,452	289,452	289,452	289,452
N of individuals, stage 1	40,419	40,419	40,419	40,419	40,419	40,419	40,419	40,419
N of obs., stage 2	106,647	104,811	15,929	8,592	11,809	7,032	27,989	11,482
N of individuals, stage 2	32,474	28,195	11,121	6,482	6,935	2,079	15,436	8,339

Notes: Panels A and B present marginal effects of DM on expenditures from two separate GLM regressions calculated as (i) the marginal effect (at the mean of the other independent variables) and (ii) the average treatment effect for the treated observations (ATET). The DM independent variable equals one if an observation is matched to a DM program in the individual's state/county based on their chronic disease(s) and insurance, and zero otherwise. In panel A, the regression estimates the effect of DM on $\Pr(y_{it} > 0 | DM_{it}, z_{it})$ using a Bernoulli distribution and a probit link function. In panel B, the regression estimates the effect of DM on $\mathrm{E}(y_{it} | y_{it} > 0, DM_{it}, z_{it})$ using a gamma distribution and a log link function. Robust standard errors (in brackets) are based on exchangeable within-individual correlation structure. In addition to fixed effects, these models include dummies for gender; age; highest education completed; race/ethnicity; diagnosis/reporting of asthma, COPD, diabetes, chronic heart failure, and/or coronary arty disease; and insurance coverage by Medicaid and/or Medicare in the quarter. The data consists of all available quarterly observations for individuals in the MEPS, 1998-2007, who receive Medicaid in at least one quarter. ***p<0.01, **p<0.05, *p<0.10.

This analysis is replicated with the *strict* DM definition in Appendix Table B2.

Table 12: Effect of Medicaid DM on quarterly health expenditures paid by Medicaid, by type of service

Dependent variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Medical expenditures paid by Medicaid (by service type)							
	Office-Based Medical Provider Visits	**Prescribed Medicines**	**Emergency Department Visits**	**Inpatient Hospital Events**	**Outpatient Hospital Events**	**Home Health**	**Dental and Vision**	**Other Medical Expenses**
Panel A: Marginal effect from stage 1 of two-part model								
Marginal effect	0.0145**	0.0540***	0.00142	-0.000215	0.00164	0.00256**	0.00109	0.00170
	[0.00640]	[0.00748]	[0.00182]	[0.00104]	[0.00144]	[0.00114]	[0.00280]	[0.00114]
ATET, stage 1	0.0172	0.0648	0.00303	-0.000552	0.00370	0.00780	0.00163	0.00404
Panel B: Marginal effect from stage 2 of two-part model								
Marginal effect	8.92	8.82	27.15	528.0	6.37	1,317***	3.90	-2.68
	[11.79]	[9.584]	[22.66]	[512.6]	[65.66]	[481.0]	[9.882]	[24.06]
ATET, stage 2	12.63	16.15	33.54	700.5	7.65	1,111	4.40	-2.89
Panel C: Implied marginal effect in two-part model								
Marginal effect	5.17	12.72	1.30	6.05	1.014	12.37	0.37	0.32
ATET	11.37	32.45	4.32	27.91	2.90	47.68	0.72	0.97
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year * quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Panel FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N of obs., stage 1	289,452	289,452	289,452	289,452	289,452	289,452	289,452	289,452
N of individuals, stage 1	40,419	40,419	40,419	40,419	40,419	40,419	40,419	40,419
N of obs., stage 2	79,491	73,674	12,193	7,036	8,613	4,911	20,748	6,929
N of individuals, stage 2	27,727	23,340	8,843	5,499	5,354	1,566	12,252	5,400

Notes: Panels A and B present marginal effects of DM on expenditures from two separate GLM regressions calculated as (i) the marginal effect (at the mean of the other independent variables) and (ii) the average treatment effect for the treated observations (ATET). The DM independent variable equals one if an observation is matched to a DM program in the individual's state/county based on their chronic disease(s) and insurance, and zero otherwise. In panel A, the regression estimates the effect of DM on $\Pr(y_{it} > 0 | DM_{it}, z_{it})$ using a Bernoulli distribution and a probit link function. In panel B, the regression estimates the effect of DM on $\mathrm{E}(y_{it} | y_{it} > 0, DM_{it}, z_{it})$ using a gamma distribution and a log link function. Robust standard errors (in brackets) are based on exchangeable within-individual correlation structure. In addition to fixed effects, these models include dummies for gender; age; highest education completed; race/ethnicity; diagnosis/reporting of asthma, COPD, diabetes, chronic heart failure, and/or coronary arty disease; and insurance coverage by Medicaid and/or Medicare in the quarter. The data consists of all available quarterly observations for individuals in the MEPS, 1998-2007, who receive Medicaid in at least one quarter. ***p<0.01, **p<0.05, *p<0.10.

This analysis is replicated with the *strict* DM definition in Appendix Table B3.

Table 13: Effect of Medicaid DM on the number of medical events

Dependent variable	(1) Office-Based Medical Provider Visits	(2) Prescribed Medicines (Filled)	(3) Emergency Department Visits	(4) Inpatient Hospital Events	(5) Hospital Events with Top-5 Diagnosis	(6) Hospital Events with Top-5 Diagnosis	(7) Hospital Events with Top-5 Diagnosis	(8) Events in Hospital with Asthma Diagnosis	(9) Events in Hospital with Asthma Diagnosis
Panel A: Marginal effect from stage 1 of two-part model									
Marginal effect	0.00568	0.0270***	0.00182	-0.00114	0.00162***	0.00150	-.00143	0.000350	-0.000767
	[0.00736]	[0.00849]	[0.00247]	[0.00133]	[0.000530]	[0.00276]	[.00176]	[0.00254]	[0.00275]
ATET, stage 1	0.00550	0.0239	0.00296	-0.00237	0.00955	0.00154	-.00208	0.000368	-0.000794
Panel B: Marginal effect from stage 2 of two-part model									
Marginal effect	-0.0323	0.186**	0.0314	0.00240	0.0916	0.0916	.152	-0.116	-0.199**
	[0.0958]	[0.0786]	[0.0283]	[0.0245]	[0.117]	[0.117]	[.124]	[0.0884]	[0.0933]
ATET, stage 2	-0.0373	0.262	0.0317	0.00236	0.0781	0.0781	.122	-0.112	-0.195
Panel C: Implied marginal effect in two-part model									
Marginal effect	0.00130	0.171	0.00381	-0.00126	0.00289	0.00532	.00101	-0.00172	-0.00490
ATET	-0.00690	0.293	0.00701	-0.00261	0.0178	0.00552	.00195	-0.00195	-0.00522
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year * quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Panel FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Disease * insurance FE							Yes		
Data Sample						Medicaid & top-5 disease	Top-5 disease (any ins.)	Medicaid and Asthma	Medicaid and Asthma
DM Definition									Strict
N of obs., stage 1	289,452	289,452	289,452	289,452	289,452	69,684	259,608	44,424	44,424
N of individuals, stage 1	40,419	40,419	40,419	40,419	40,419	9,503	35,507	6,018	6,018
N of obs., stage 2	111,288	104,811	17,044	8,720	2,884	2,884	8,140	1,184	1,184
N of individuals, stage 2	33,067	28,195	11,683	6,555	1,794	1,794	5,229	777	777

Notes: Panels A and B present marginal effects of DM on the number of events from two separate GLM regressions calculated as (i) the marginal effect (at the mean of the other independent variables) and (ii) the average treatment effect for the treated observations (ATET). The DM independent variable equals one if an observation is matched to a DM program in the individual's state/county based on their chronic disease(s) and insurance, and zero otherwise. In panel A, the regression estimates the effect of DM on $\Pr(y_{it} > 0 | DM_{it}, z_{it})$ using a Bernoulli distribution and a probit link function. In panel B, the regression estimates the effect of DM on $\mathrm{E}(y_{it} | y_{it} > 0, DM_{it}, z_{it})$ using a negative binomial distribution and a log link function. Robust standard errors (in brackets) are based on exchangeable within-individual correlation structure. In addition to fixed effects, these models include dummies for gender; age; highest education completed; race/ethnicity; diagnosis/reporting of asthma, COPD, diabetes, chronic heart failure, and/or coronary arty disease (the "top-5" diseases); and insurance coverage by Medicaid and/or Medicare in the quarter. The data consists of quarterly observations for individuals in the MEPS, 1998-2007. In columns (1) through (8), the data is limited to those who receive Medicaid in at least one quarter (and meet the criteria in the table's "Data Sample" row, if applicable). Column (9) adds to this data sample any individual with a top-5 chronic disease, regardless of insurance. ***p<0.01, **p<0.05, *p<0.10.

Table 14: Effect of Medicaid DM on quarterly health expenditures for individuals with variation in DM coverage during MEPS survey

	(1)	(2)	(3)	(4)
	Conditional logit		OLS	
Model	$\mathbf{Pr}(y_{it} > 0)$		$\mathbf{log}(y_{it} \mid y_{it} > 0)$	
Dependent Variable (y_{it})	Total medical expenditures			
DM_{it}	0.0464		0.0683*	
	[0.0649]		[0.0409]	
month DM begins, $h_1(DM_{it})$		-0.0499		0.132**
		[0.105]		[0.0588]
1 month after DM begins, $h_2(DM_{it})$		0.297**		0.0382
		[0.115]		[0.0638]
2 months after DM begins, $h_3(DM_{it})$		0.192		0.0828
		[0.127]		[0.0707]
3 months after DM begins, $h_4(DM_{it})$		0.296**		0.0918
		[0.141]		[0.0806]
4 months after DM begins, $h_5(DM_{it})$		-0.0324		0.0672
		[0.175]		[0.0984]
5 months after DM begins, $h_6(DM_{it})$		0.517**		0.0981
		[0.219]		[0.124]
6 months after DM begins, $h_7(DM_{it})$		-0.156		0.306*
		[0.282]		[0.177]
Year * quarter FE	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes
N of obs.	209,020	209,020	155,901	155,901
N of individuals	28,789	28,789	36,618	36,618

Notes: Table presents regression coefficients for the effect of DM on quarterly total medical expenditures, controlling for individual fixed effects. In columns (1) and (3), the DM independent variable equals one if an observation is matched to a DM program in the individual's state/county based on their chronic disease(s) and insurance, and zero otherwise. In columns (2) and (4), the dummy variables identify the number of months since DM was introduced to the individual; the coefficients reflect the change in expenditures relative to the period before the introduction of DM. Robust standard errors (in brackets). The data consists of all available quarterly observations for individuals in the MEPS, 1998-2007, who receive Medicaid in at least one quarter. This base sample is composed of 289,452 individuals. There are 10,348 observations for people who experience variation in DM while they are in the MEPS. The conditional logit model excludes individuals for whom quarterly expenditures are always zero or always positive during the survey and the OLS model excludes observations with zero expenditures. ***p<0.01, **p<0.05, *p<0.10.

The coefficients from columns (2) and (4) are plotted in **Figure 10**.

Table 15: GEC program eligibility October 2005 through December 2006

	Program eligibility (number of members)					Total	
Month	Enter GEC	Re-Enter	In GEC	Exit GEC	Not in GEC	In GEC	Not In GEC
Before Oct-05	-	-	-	-	69,831	-	69,831
Oct-05	47,655	-	-	-	22,176	47,655	22,176
Nov-05	2	0	47,594	61	22,174	47,596	22,235
Dec-05	1	0	43,142	4,454	22,234	43,143	26,688
Jan-06	2,682	2,690	42,253	890	21,316	47,625	22,206
Feb-06	636	366	46,760	865	21,204	47,762	22,069
Mar-06	944	179	46,702	1,060	20,946	47,825	22,006
Apr-06	744	237	46,946	879	21,025	47,927	21,904
May-06	130	37	46,362	1,565	21,737	46,529	23,302
Jun-06	1,993	975	45,453	1,076	20,334	48,421	21,410
Jul-06	2,924	341	47,067	1,354	18,145	50,332	19,499
Aug-06	791	316	48,408	1,924	18,392	49,515	20,316
Sep-06	915	906	47,491	2,024	18,495	49,312	20,519
Oct-06	830	974	48,248	1,064	18,715	50,052	19,779
Nov-06	759	312	48,958	1,094	18,708	50,029	19,802
Dec-06	128	88	43,551	6,478	19,586	43,767	26,064
Jan-07	6,602	11,465	42,714	1,053	7,997	60,781	9,050
Feb-07	470	390	60,071	710	8,190	60,931	8,900
Mar-07	491	324	60,500	431	8,085	61,315	8,516
Apr-07	416	297	60,845	470	7,803	61,558	8,273
May-07	247	210	60,968	590	7,816	61,425	8,406
Jun-07	250	268	60,757	668	7,888	61,275	8,556
Jul-07	43	307	61,062	213	8,206	61,412	8,419
Aug-07	14	204	60,538	874	8,201	60,756	9,075
Sep-07	15	457	60,349	407	8,603	60,821	9,010
Oct-07	19	319	60,504	317	8,672	60,842	8,989
Nov-07	12	200	60,310	532	8,777	60,522	9,309
Dec-07	10	274	60,400	122	9,025	60,684	9,147

Notes: Author's calculations for 69,831 individuals listed in the GEC program's eligibility files, October 2005 through December 2007.

Table 16: GEC program DM interventions, by risk group

Risk Level	Intervention
Low	• Ongoing claims monitoring and risk stratification • Non-clinical staff "Welcome Call," health history and periodic health review calls to monitor status • Initial Intake Assessment • Access to 24-hour "1-800" nurse phone line, program web site; some health information materials by mail
Moderate	All low-intensity interventions as well as: • Interventions by telephonic nurse care managers • 1-2 phone calls each month to physician's office or the patient with visits to the home or hospital as needed for education, personal coaching and monitoring • Reporting of vital signs and symptoms to the case/disease manager and appropriate nurse follow-up • Educational mailing
High	All low and moderate-intensity interventions as well as: • Individually assigned locally based nurse care managers for interventions, monitoring, and ongoing care • One or more home visits each quarter or more than 2 phone calls to the physician's office or the member each month, for education, personal coaching and monitoring • Personalized training in managing chronic disease

Source: Georgia Department of Community Health RFP (2005); response to RFP by United HealthCare Services (2005); the related contract between the parties (2005), and personal communication with program staff (Abraham et al. 2007).

Table 17: Selection of primary estimation sample: number of individuals in data and estimation sample

Sample selection process for baseline specifications	**N**
Initial Sample	69,884
- Low risk category (October 2005 to January 2006 -*and*- June to July 2006)	- 39,468
- Ineligible (October 2005 to January 2006 -*or*- June to July 2006)	- 12,619
- Major health event April-June 2006 (or "false switch")	-448
Final Sample	17,349

Table 18: Selection of primary estimation sample: number of individuals by risk group assignment in pre/post period

Initial Risk Group (by Jan. 2006)	**Risk group after switch** (June/July 2006)			
	No Switch	**Moderate**	**High**	**Total**
Low/Other	x	2,507	1,663	4,170
Moderate	9,522	x	1,286	10,808
High	2,371	x	x	2,371
Ineligible	x	x	x	x
Total	11,893	2,507	2,949	17,349

x = group excluded from sample

Table 19: GEC program summary statistics

Initial risk group (Jan. 2006)	(1) High	(2) Low	(3) Mod.	(4) Mod.	(5) Low	(6) Low	(7) Eligible after Jan.	(8)
Risk group switch June/July 2006		Low to High	Mod. to High		Low to Mod.	Risk / Other	2006	All
Panel A: Individual-level characteristics								
Number of individuals	2,371	1,388	1,286	9,522	1,713	34,060	19,457	69,797
Percent female	65.8%	65.7%	69.0%	62.2%	59.3%	50.7%	50.7%	53.6%
Percent under 18 years	18.9%	17.6%	6.5%	7.5%	11.9%	34.0%	28.9%	27.1%
Mean age	36.8	35.0	40.7	45.8	42.7	31.9	33.9	35.0
	[0.38]	[0.46]	[0.39]	[0.16]	[0.41]	[0.11]	[0.15]	[0.07]
Percent with chronic condition	100%	100%	100%	100%	100%	98.7%	74.5%	92.3%
Mean number of chronic	14.9	15.2	16.8	13.1	12.9	9.1	7.1	9.6
conditions	[0.10]	[0.11]	[0.12]	[0.05]	[0.11]	[0.03]	[0.04]	[0.02]
Percent with:								
1 chronic condition	0.0%	0.0%	0.0%	0.1%	0.1%	0.7%	4.6%	1.6%
2 to 4 chronic conditions	1.6%	0.1%	0.2%	3.4%	2.9%	16.6%	10.3%	11.6%
5+ chronic conditions	98.4%	99.9%	99.8%	96.5%	97.1%	81.4%	59.6%	79.1%
Percent with one or more "top-5" conditions	72.3%	70.3%	83.8%	78.9%	80.7%	42.1%	39.1%	49.6%
Percent with other high-cost condition	78.5%	74.4%	96.6%	78.0%	71.2%	53.0%	45.0%	56.7%
Panel B: Baseline period health outcomes – January to September 2005								
Mean monthly health	1,765	1,677	1,915	1,062	759.6	550.1	1,257	868.7
expend. ($)	[58.94]	[108.7]	[66.04]	[22.88]	[25.63]	[9.30]	[44.31]	[10.3]
Percent with expend. > 0	98.9%	98.5%	99.4%	99.0%	97.6%	97.6%	94.5%	97.5%
Mean monthly E.D.	247.6	178	328.3	109.1	61.19	30.96	124.1	80.3
expend. ($)	[2.34]	[17.35]	[6.52]	[22.68]	[9.60]	[22.93]	[9.12]	[2.50]
Admission days, per month	0.58	0.76	0.71	0.25	0.18	0.12	0.47	0.25
	[0.036]	[0.075]	[0.037]	[0.010]	[0.020]	[0.006]	[0.026]	[0.006]
Inpatient/E.R. events per 1000 patients, per month								
Asthma	8.61	12.41	16.96	8.24	11.64	3.76	8.02	6.19
COPD	3.91	3.86	9.60	5.21	2.78	0.86	2.14	2.32
Diabetes	6.07	5.13	14.91	9.05	8.05	1.44	5.28	4.20
CAD	4.67	2.66	7.43	7.44	7.00	0.95	4.54	3.19
Heart Failure	4.27	4.30	10.14	7.82	5.10	0.95	5.77	3.47
Mean no. of unique	2.81	2.02	3.63	3.55	2.53	1.41	1.33	1.95
prescriptions	[0.059]	[0.063]	[0.087]	[0.031]	[0.068]	[0.012]	[0.024]	[0.011]
Mean no. of unique	3.18	2.84	3.34	2.04	1.84	1.57	2.38	1.93
providers	[0.046]	[0.050]	[0.055]	[0.015]	[0.032]	[0.007]	[0.023]	[0.007]

(Table continued on next page)

Table 19: GEC program summary statistics (continued)

Initial risk group (Jan. 2006)	(1) High	(2) Low	(3) Mod.	(4) Mod.	(5) Low	(6) Low	(7) Eligible after Jan.	(8)
Risk group switch June/July 2006		Low to High	Mod. to High		Low to Mod.	Risk / Other	2006	All
Panel C: Initial treatment period health outcomes – October 2005 to June 2006								
Mean monthly health	1,318	1,102	1,743	960.4	817.1	386.9	1,386	748.7
expend. ($)	[40.42]	[43.62]	[61.67]	[16.96]	[23.61]	[5.41]	[41.01]	[8.91]
Percent with expend. > 0	80.2%	81.7%	89.4%	84.5%	83.1%	58.1%	80.3%	68.9%
Mean monthly E.D.	178.6	161.7	283.7	92.9	91.2	27.2	133.1	72.5
expend. ($)	[1.66]	[16.14]	[5.34]	[19.13]	[12.83]	[18.24]	[7.74]	[2.10]
Admission days, per month	0.35	0.30	0.49	0.18	0.15	0.06	0.59	0.20
	[0.029]	[0.023]	[0.026]	[0.008]	[0.011]	[0.003]	[0.024]	[0.005]
Inpatient/E.R. events per 1000 patients, per month								
Asthma	8.20	14.18	13.56	7.54	9.42	2.46	6.67	4.92
COPD	4.05	4.75	9.50	4.00	2.94	0.72	3.14	2.13
Diabetes	6.02	4.18	10.97	7.89	6.41	1.13	4.95	3.48
CAD	2.60	3.60	4.84	4.93	6.42	0.88	4.32	2.50
Heart Failure	3.28	6.28	12.97	7.21	6.74	0.70	7.83	3.65
Mean no. of prescriptions	3.33	2.64	4.37	4.33	3.57	1.63	1.40	2.20
	[0.068]	[0.074]	[0.103]	[0.036]	[0.074]	[0.013]	[0.021]	[0.011]
Mean no. of unique	2.93	2.85	3.47	2.24	2.19	1.41	2.22	1.84
providers	[0.047]	[0.048]	[0.054]	[0.016]	[0.031]	[0.006]	[0.019]	[0.006]
Panel D: Later treatment period health outcomes – July 2006 to December 2007								
Mean monthly health	897.8	735.2	1,406	818.1	626.7	292.6	426.5	461.5
expend. ($)	[31.43]	[31.17]	[54.88]	[14.60]	[19.67]	[3.76]	[9.76]	[4.32]
Percent with expend. > 0	64.0%	65.9%	76.9%	70.5%	68.6%	45.0%	33.7%	47.5%
Mean monthly E.D.	111.1	102.1	231.1	84.82	55.89	21.39	48.76	47.39
expend. ($)	[8.99]	[12.07]	[17.37]	[4.24]	[5.90]	[1.02]	[2.30]	[1.14]
Admission days, per	0.27	0.19	0.47	0.21	0.13	0.06	0.14	0.12
month	[0.019]	[0.017]	[0.030]	[0.009]	[0.011]	[0.002]	[0.005]	[0.002]
Inpatient/E.R. events per 1000 patients, per month								
Asthma	5.65	6.96	11.33	5.35	4.28	1.71	2.26	2.85
COPD	3.70	4.35	6.91	3.56	2.41	0.63	1.51	1.63
Diabetes	5.19	2.38	8.93	6.83	4.86	0.90	1.86	2.41
CAD	2.41	2.78	4.91	4.93	3.93	0.86	1.69	1.90
Heart Failure	2.93	2.50	6.73	6.57	3.59	0.66	2.43	2.27
Mean no. of prescriptions	2.77	2.43	4.02	3.77	3.34	1.40	1.06	1.79
	[0.068]	[0.077]	[0.105]	[0.038]	[0.078]	[0.013]	[0.014]	[0.010]
Mean no. of unique	1.92	1.83	2.46	1.59	1.43	0.84	0.89	1.06
providers	[0.042]	[0.043]	[0.053]	[0.015]	[0.029]	[0.006]	[0.010]	[0.005]

Table provides summary statistics on demographic and health outcomes. Columns indicate the patient's classification into High, Moderate, and Low risk categories by January 2005, and if they were reclassified in July 2006 when the administrative error (described in the text) was fixed. Total medical expenditures exclude DM program fees. Standard errors in brackets.

Table 20: Normalized differences between columns in summary statistics Table 19

Comparison between groups: (columns)	**Switch Low to High vs. Initially High** (2) – (1)	**Switch Mod. to High vs. Initially High** (3) - (1)	**Switch Low to Mod. vs. Initially Mod.** (5) - (4)
Panel A: Individual Characteristics			
Percent Female	-0.001	0.048	-0.043
Percent under 18 years	-0.024	**-0.258**	0.105
Age	-0.074	0.165	-0.135
Percent with a chronic condition*	0	0	0.014
Mean number of chronic conditions	0.048	**0.269**	-0.028
Percent with:			
1 chronic condition	-0.021	-0.021	-0.001
2 to 4 chronic conditions	-0.109	-0.107	-0.023
5 or more chronic conditions	0.110	0.109	0.024
Percent with one or more "top-5" conditions	-0.031	0.195	0.032
Percent with other high-cost condition	-0.069	**0.373**	-0.111
Panel B: Baseline health outcomes - January to September 2005			
Mean monthly health expend. ($)	-0.018	0.040	-0.123
Percent with positive expenditures	-0.041	0.068	-0.105
Mean E.R. expend. ($)	-0.058	0.069	-0.065
Admission days, per month	0.055	0.058	-0.060
Inpatient/E.R. events per 1000 patients, per month			
Asthma	0.033	0.082	0.036
COPD	-0.001	0.085	-0.050
Diabetes	-0.015	0.092	-0.012
CAD	-0.046	0.050	-0.006
Heart Failure	0.001	0.078	-0.036
Mean number of unique prescriptions	-0.209	0.192	-0.246
Mean number of unique providers	-0.115	0.057	-0.098

Table provides normalized differences between selected columns in Panels A and B of Table 19. Differences with an absolute value greater than 0.25 are in bold. *All individuals ever assigned to the high risk group have at least one chronic condition.

Table 21: GEC program DM interventions in the initial treatment period, October 2005 to June 2006

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Initial risk group (Jan. 2006)	**High**	**Low**	**Mod.**	**Mod.**	**Low**	**Low**	**Eligible after**	
Risk group switch June/July 2006		**Low to High**	**Mod. to High**		**Low to Mod.**	**Risk / Other**	**Jan. 2006**	**All**
Percent with:								
Initial intake assessment	73.4%	35.2%	46.2%	27.2%	18.3%	23.2%	6.9%	23.4%
Comprehensive assessment	44.0%	12.5%	34.1%	17.0%	4.9%	0.6%	1.2%	6.0%
Mean number of tasks*								
Total completed	5.33	2.71	4.81	2.49	1.06	1.01	0.25	1.39
	[0.173]	[0.141]	[0.218]	[0.056]	[0.086]	[0.014]	[0.012]	[0.016]
Task via field visit	0.05	0.01	0.18	0.09	0.01	0.00	0.00	0.02
	[0.010]	[0.004]	[0.017]	[0.004]	[0.002]	[0.000]	[0.000]	[0.001]
Task with provider	0.46	0.25	0.44	0.22	0.11	0.08	0.02	0.12
	[0.027]	[0.020]	[0.031]	[0.007]	[0.013]	[0.002]	[0.002]	[0.002]
Completed non-assessment calls	3.14	1.38	2.81	1.46	0.60	0.53	0.16	0.78
	[0.108]	[0.085]	[0.141]	[0.035]	[0.052]	[0.009]	[0.009]	[0.010]
Education and training tasks	0.97	0.48	1.25	0.69	0.31	0.29	0.09	0.37
	[0.041]	[0.036]	[0.067]	[0.019]	[0.029]	[0.006]	[0.006]	[0.005]
Coordination tasks and referrals	0.68	0.32	0.44	0.22	0.11	0.10	0.02	0.14
	[0.038]	[0.031]	[0.042]	[0.009]	[0.017]	[0.003]	[0.002]	[0.003]

Table provides summary statistics on selected disease management interventions, where the columns indicate the patient's classification into High, Moderate, and Low risk categories by January 2005, and if they were reclassified in July 2006 when the administrative error (described in the text) was fixed. Standard errors in brackets. *Excludes attempted contacts (e.g., no answer to a telephone call).

Table 22: Intensive margin OLS estimates of the effect of DM on log(Total Medical Expenditures)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
DM	-0.0433***		-0.0993***		-0.0999***		-0.098***	-0.0341***	
	(0.010)		(0.028)		(0.0275)		(0.0275)	(0.00651)	
DM x (Any chronic disease)			0.058**		-0.0147		-0.0325		
			(0.027)		(0.0311)		(0.0287)		
DM x ("top-5" chronic disease)					0.0815***				
					(0.0179)				
DM High		-0.170***		-0.313***		-0.314***			-0.153***
		(0.0144)		(0.073)		(0.0731)			[0.0115]
DM High x (Any chronic disease)				0.145**		0.129			
				(0.073)		(0.0792)			
DM High x ("top-5" chronic disease)						0.0187			
						(0.0339)			
DM Mod		-0.00902		-0.0832***		-0.0841***			0.00916
		(0.0104)		(0.029)		(0.029)			[0.00678]
DM Mod x (Any chronic disease)				0.077***		-0.0162			
				(0.028)		(0.0333)			
DM Mod x ("top-5" chronic disease)						0.104***			
						(0.0288)			
DM x (CHF)							0.0815***		
							(0.0152)		
DM x (CAD)							0.0525***		
							(0.0138)		
DM x (Diabetes)							0.0958***		
							(0.0114)		
DM x (Ashtma)							-0.00989		
							(0.0125)		
DM x (COPD)							0.0217*		
							(0.0131)		
Individual Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Data								Include low-risk	Include low-risk
Observations	453,490	453,490	453,490	453,490	453,490	453,490	453,490	1,215,509	1,215,509
Number of individuals	17,297	17,297	17,297	17,297	17,297	17,297	17,297	60,233	60,233
Average T	26.2	26.2	26.2	26.2	26.2	26.2	26.2	20.2	20.2

All models include unreported individual and month fixed effects and report robust standard errors, clustered at the individual level, in parentheses. The variable DM equals one if the individual has ever been classified as high or moderate risk before or during the month. The variables DM Mod and DM High equal one if the individual has ever been classified as moderate or high risk before or during the month, respectively, with the exception that DM Mod always equals zero when DM High equals one. Total medical expenditures do not include DM program fees. ***p<0.01, **p<0.05, *p<0.1.

Table 23: Intensive margin OLS estimates of the effect of DM on health expenditures, by type of service

	(1)	(2)	(3)	(4)
Dependent Variable	log(ED Expend.)		log(RX Expend.)	
DM	-0.037		0.0085	
	(0.0786)		(0.00967)	
DM High		-0.0288		-0.0383***
		(0.0902)		(0.0140)
DM Mod		-0.0398		0.0207**
		(0.0817)		(0.00980)
Individual Fixed Effects	Yes	Yes	Yes	Yes
Month Fixed Effects	Yes	Yes	Yes	Yes
Observations	8,614	8,614	404,424	404,424
N	4,361	4,361	17,173	17,173
Average T	1.98	1.98	23.55	23.55

All models include unreported individual and month fixed effects and report robust standard errors, clustered at the individual level, in parentheses. The variable DM equals one if the individual has ever been classified as high or moderate risk before or during the month. The variables DM Mod and DM High equal one if the individual has ever been classified as moderate or high risk before or during the month, respectively, with the exception that DM Mod always equals zero when DM High equals one. Total medical expenditures do not include DM program fees. ***p<0.01, **p<0.05, *p<0.1. For all columns, the observation is dropped if expenditures equal zero dollars.

Table 24: OLS estimates of the effect of DM on other outcomes

Dependent Variable	DM	DM High	DM Mod	Obs.	N	Avg. T
Number of Days Admitted to Hospital	-0.044** (0.015)	-0.222** (0.023)	0.007 (0.015)	587,070	17,349	33.8
Hosp/ED Admissions for Asthma	-0.003** (0.001)	-0.004** (0.001)	-0.003** (0.001)	557,605	17,349	32.1
Hosp/ED Admissions for Diabetes	-0.001 (0.001)	-0.001 (0.001)	-0.001 (0.001)	557,605	17,349	32.1
Hosp/ED Admissions for CAD	-0.001* (0.001)	-0.001 (0.001)	-0.001* (0.001)	557,605	17,349	32.1
Hosp/ED Admissions for Heart Failure	-0.002* (0.001)	-0.003** (0.001)	-0.001 (0.001)	557,605	17,349	32.1
Number of unique prescriptions	0.007 (0.024)	-0.011 (0.034)	0.012 (0.026)	563,184	17,349	32.5
Number of unique providers	-0.232** (0.021)	-0.611** (0.031)	-0.122** (0.022)	557,605	17,349	32.1

Each row corresponds to two regressions, of the same form as columns (1) and (2) of Table 7. All models include unreported individual month fixed effects and report robust standard errors, clustered at the individual level, in parentheses. The variable DM equals one if the individual has ever been classified as high or moderate risk before or during the month. The variables DM Mod and DM High equal one if the individual has ever been classified as moderate or high risk before or during the month, respectively, with the exception that DM Mod always equals zero when DM High equals one. ***p<0.01, **p<0.05, *p<0.1.

Table 25: Extensive margin estimates of the effect of DM on health utilization

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dependent Variable	**Pr(Medical Expend. > 0)**		**Pr(ED Admission)**		**Pr(Inpatient Admission)**		**Pr(Prescription Drug)**		**Pr(Office Visit)**	
DM	-0.00861***		-0.0422***		-0.0617***		-0.0358***		-0.0490***	
	(0.00879)		(0.00416)		(0.00903)		(0.00312)		(0.00356)	
DM Mod		-0.00834***		-0.0221***		-0.0359***		-0.0391***		-0.0454***
		(0.00083)		(0.00422)		(0.00911)		(0.00305)		(0.00354)
DM High		-0.00657***		-0.0948***		-0.138***		-0.0213***		-0.0571***
		(0.00072)		(0.00453)		(0.00968)		(0.00336)		(0.00401)
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	432,035	432,035	532,561	532,561	259,365	259,365	479,156	479,156	556,454	556,454
N	12,984	12,984	15,689	15,689	7,659	7,659	14,276	14,276	16,383	16,383
Mean of dependent variable	0.693	0.693	0.221	0.221	0.0854	0.0854	0.643	0.643	0.475	0.475

Table provides the marginal effect for discrete change in the dummy variable from 0 to 1, evaluated at the mean of Z. Total medical expenditures do not include DM program fees. In all columns, the model is a conditional logit regression with individual and month fixed effects. The sample was limited to individuals who had a least one observation with and without the observed outcome. Standard errors, clustered at the individual level, in parenthesis. ***p<0.01, **p<0.05, *p<0.1.

Table 26: Estimates of the effect of DM on the probability of a "potentially avoidable" hospitalization

	(1)	(2)	(3)	(4)
Dependent Variable	Pr(Potentially avoidable hospitalization related to chronic condition)		Pr(Potentially avoidable hospitalization)	
DM	-0.0625***		-0.0594***	
	(0.00567)		(0.00528)	
DM Mod		-0.0623***		-0.0582***
		(0.00570)		(0.00530)
DM High		-0.0615***		-0.0629***
		(0.00707)		(0.00641)
Individual FE	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes
Observations	367,819	367,819	410,511	410,511
N	10,724	10,724	11,987	11,987
Mean of dependent variable	0.205	0.205	0.205	0.205

Table provides the marginal effect for discrete change in the dummy variable from 0 to 1, evaluated at the mean of Z. In all columns, the model is a conditional logit regression with individual and month fixed effects. Potentially avoidable hospitalizations were identified by the PQI algorithm from AHRQ (2007b). The sample was limited to individuals who had a least one observation with and without the observed outcome. Standard errors, clustered at the individual level, in parenthesis. ***p<0.01, **p<0.05, *p<0.1.

Table 27: OLS estimates of the effect of DM on health expenditures, with quarterly data

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dependent Variable	log(Total Expenditures + $1)		log(Emergency Dept. Expenditures + $1)		log(Inpatient Expenditures + $1)		log(Outpatient Expenditures + $1)		log(Pharmacy Expenditures + $1)	
DM	-0.403		-0.107		-0.020		-0.435		-0.228	
	(0.029)***		(0.019)***		(0.010)**		(0.028)***		(0.027)***	
DM Mod		-0.336		-0.019		-0.001		-0.334		-0.255
		(0.032)***		(0.020)		(0.010)		(0.030)***		(0.030)***
DM High		-0.498		-0.233		-0.046		-0.577		-0.188
		(0.040)***		(0.025)***		(0.012)***		(0.037)***		(0.036)***
Constant	7.224	7.224	0.401	0.400	0.075	0.075	5.645	5.644	5.776	5.776
	(0.015)***	(0.015)***	(0.013)***	(0.013)***	(0.006)***	(0.006)***	(0.016)***	(0.016)***	(0.015)***	(0.015)***
Observations	197,400	197,400	197,400	197,400	197,400	197,400	197,400	197,400	197,400	197,400
N	17,349	17,349	17,349	17,349	17,349	17,349	17,349	17,349	17,349	17,349
Mean of Y	6.26	6.26	0.34	0.34	0.07	0.07	4.85	4.85	5.09	5.09

This table uses aggregated data, with one observation per quarter per individual. All models include unreported individual quarter fixed effects and report robust standard errors, clustered at the individual level, in parentheses. The variable DM equals one if the individual has ever been classified as high or moderate risk before or during the quarter. The variables DM Mod and DM High equal one if the individual has ever been classified as moderate or high risk before or during the quarter, respectively, with the exception that DM Mod always equals zero when DM High equals one. Total medical expenditures do not include DM program fees. ***p<0.01, **p<0.05, *p<0.1.

Table 28: Estimates of the effect of DM across the distribution of health care costs

Dependent		Quantile						
Variable	**Mean**	**5%**	**10%**	**25%**	**50%**	**75%**	**90%**	**95%**
All medical expenditures								
Dollars		-69	-78	-102	-157	-186	-199	171
		(7.6531)	(8.9286)	(16.0714)	(27.2959)	(52.2959)	(105.867)	(223.725)
Log(dollars)	-0.0443	-0.181	-0.137	-0.127	-0.076	-0.005	0.07	0.225
	(0.0262)	(0.0515)	(0.0418)	(0.0342)	(0.0324)	(0.0367)	(0.0365)	(0.0587)
All medical expenditures, with control variables								
Dollars		-57	-70	-89	-153	-174	-112	329
		(14.5408)	(9.949)	(15.051)	(28.0612)	(48.7245)	(103.827)	(191.582)
Log(dollars)	-0.0464	-0.17	-0.138	-0.096	-0.087	-0.024	0.072	0.214
	(0.0265)	(0.063)	(0.0383)	(0.0332)	(0.0319)	(0.0367)	(0.0395)	(0.0505)
Inpatient expenditures								
Log(dollars)	-0.3888	-0.902	-0.902	-0.360	-0.207	-0.234	-0.438	-0.277
	(0.2421)	(0.4434)	(0.5523)	(0.527)	(0.3533)	(0.3426)	(0.2773)	(0.2592)
Emergency department expenditures								
Log(dollars)	-0.0551	0.009	0.121	0.015	-0.076	-0.294	-0.139	-0.144
	(0.1136)	(0.4546)	(0.1306)	(0.1661)	(0.0995)	(0.1704)	(0.2719)	(0.2939)
Outpatient expenditures								
Log(dollars)	-0.0465	-0.136	-0.127	-0.085	-0.059	-0.014	0.002	0.133
	(0.0229)	(0.0342)	(0.0255)	(0.0224)	(0.0276)	(0.0367)	(0.0469)	(0.0709)

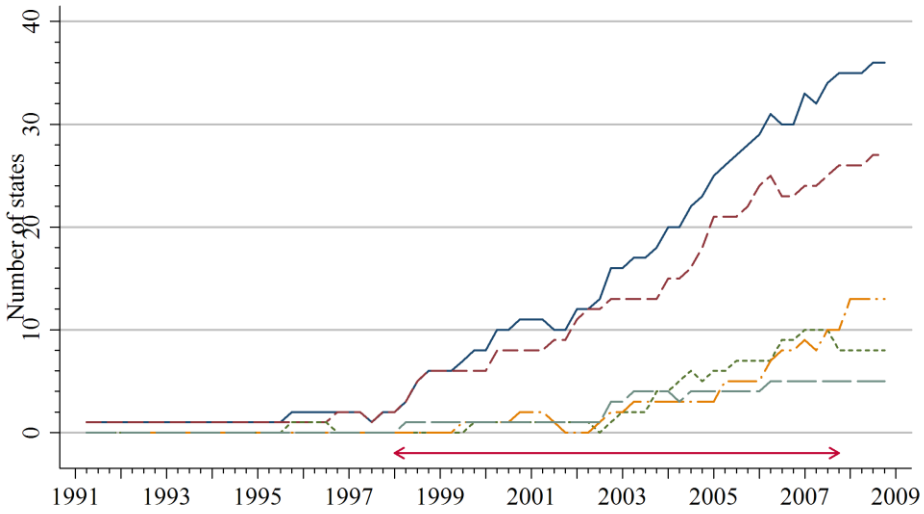
Table presents the estimate of the effect of treatment on the treated group in a Changes-in-Changes model. Bootstrapped standard errors are in parenthesis. The data was restricted to the average health care expenditure for each individual during January through September 2005 in the pre-period and October 2005 through June 2006 in the post period (9 months of pre and post data). The data was restricted to individuals assigned to the high and moderate risk groups in the program start up period, or were reclassified in July 2006 when the administrative error (described in the text) was fixed. Total medical expenditures do not include DM program fees.

The estimates in this table are presented graphically in Figure 18.

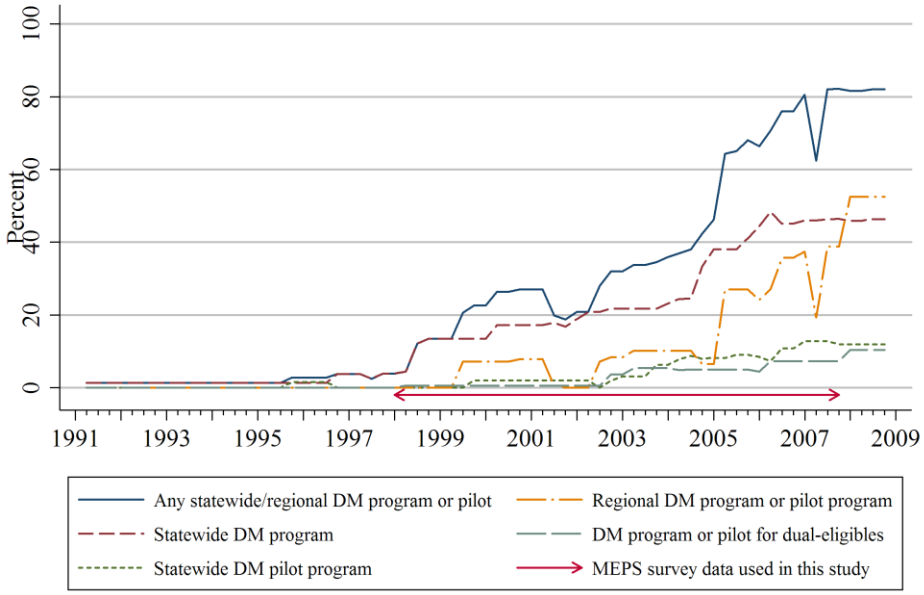
Figures

Figure 1: The increase of states with Medicaid Disease Management, 1991-2008

Panel A: Number of states with Medicaid Disease Management

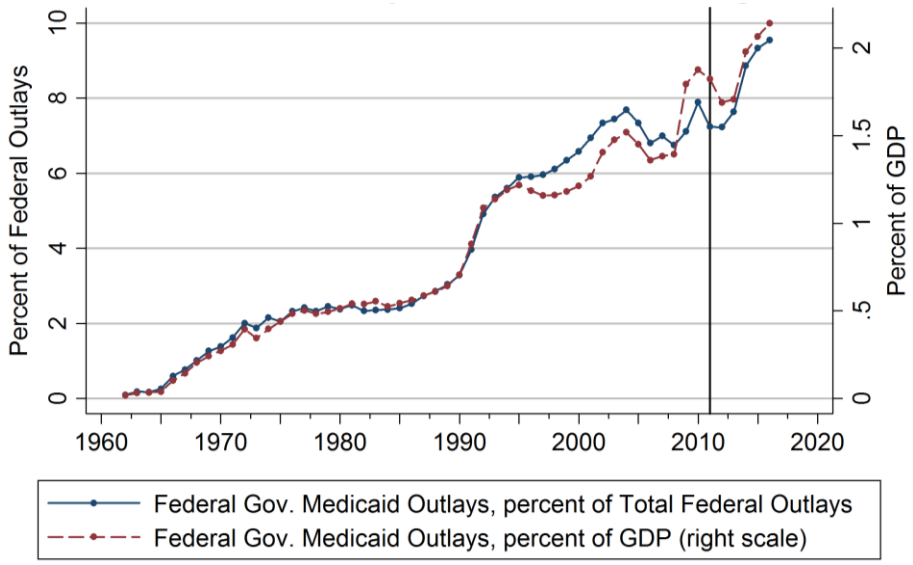


Panel B: Weighted percent of states with Medicaid Disease Management



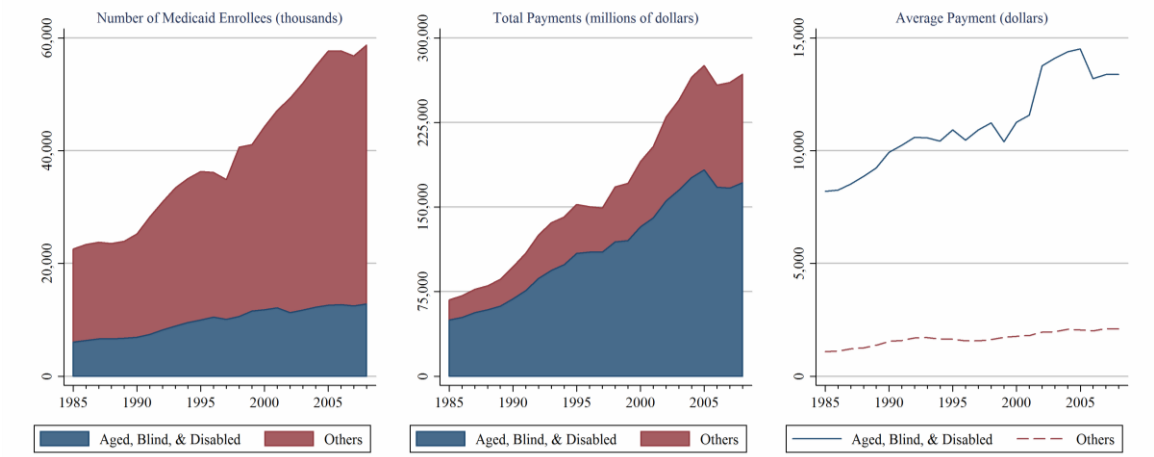
Source: Tabulations from author's survey of Medicaid disease management programs. Panel A presents the number of states with a program/pilot. Panel B depicts the percent of states with a Medicaid Disease Management program or pilot program, where each state is weighted by its total number of Medicaid enrollees in 2007 (state enrollment data from KFF 2011a).

Figure 2: Total federal outlays on the Medicaid program, FY 1962-2016



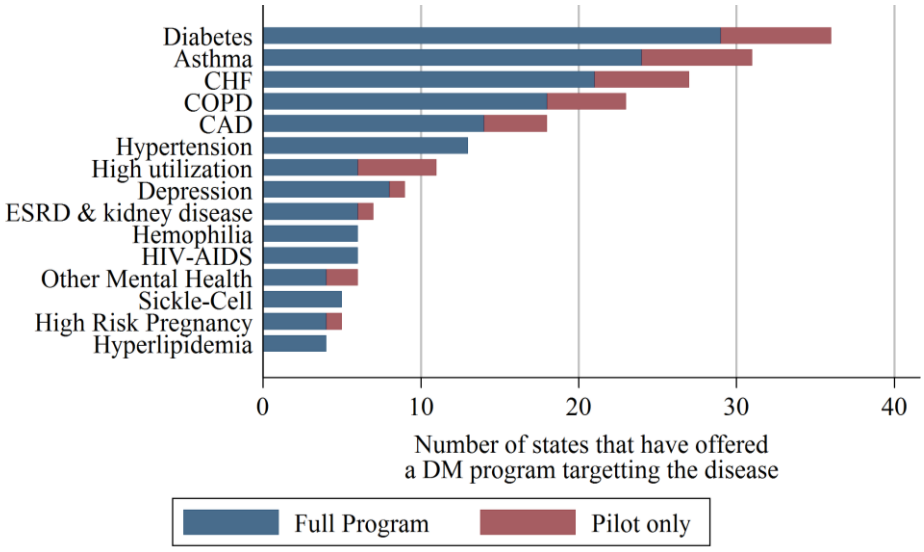
Source: U.S. Office of Management and Budget. (2011 Table 16.1) Graph provides estimates for FY2011-16.

Figure 3: Medicaid enrollees and payments, FY 1985-2005



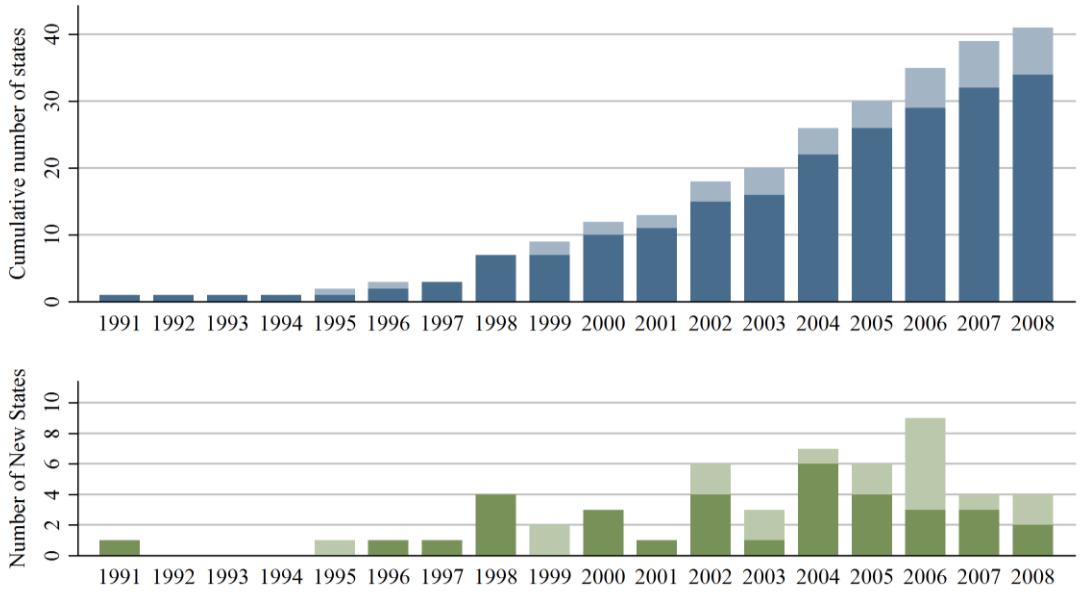
Source: Author's calculations, base on SSA (2011, p. Table 8.E2). Expenditures adjusted to January 2005 dollars using the CPI-U.

Figure 4: Diseases commonly targeted by Medicaid Disease Management programs



Source: Tabulations from author's survey of Medicaid disease management programs.

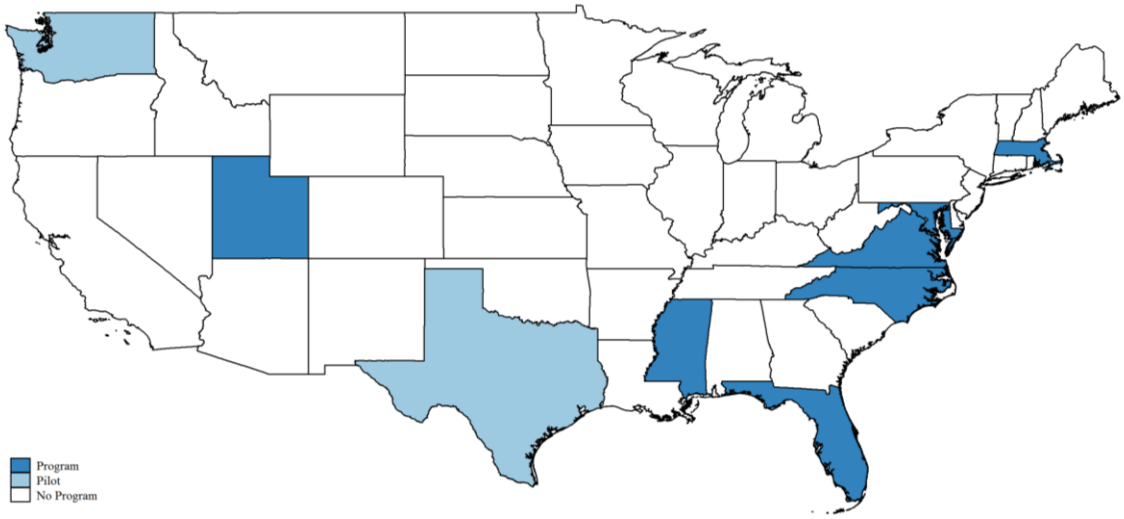
Figure 5: Cumulative number of states who have implemented a Medicaid Disease Management program or pilot program



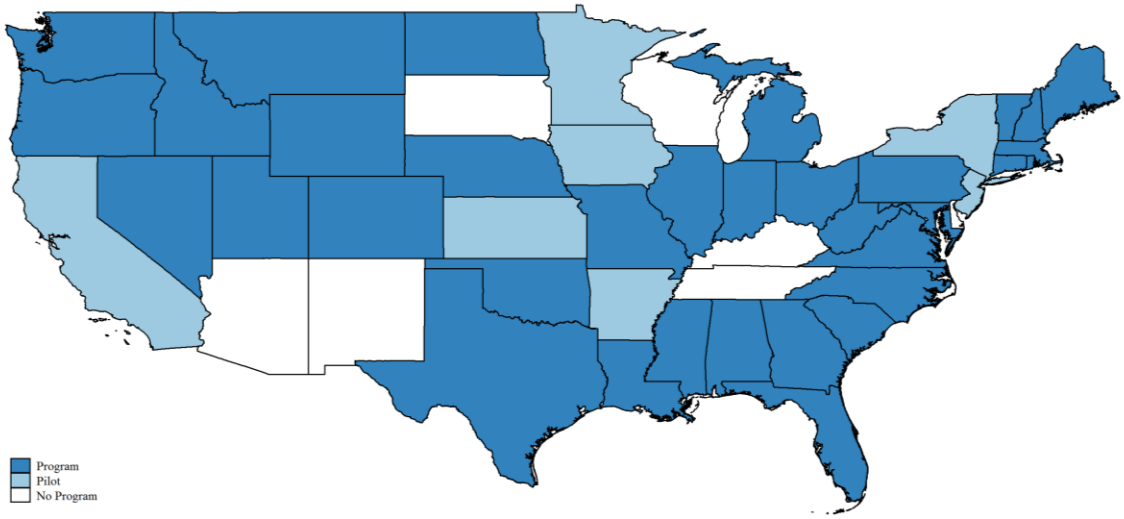
Source: Tabulations from author's survey of Medicaid disease management programs. The dark-shaded bars represent states that have ever implemented a full program at any point prior to the year on the horizontal axis; the light-shaded bars add states that have implemented a pilot program. The top panel graphs the *cumulative* number of states who implemented a program and the bottom panel graphs the change from the previous year. In the bottom panel, states are counted twice if they first had a pilot program and later a full program.

Figure 6: States that have implemented a Medicaid Disease Management program

Panel A: States that implemented at least one Medicaid DM Program, 1991 through 1999

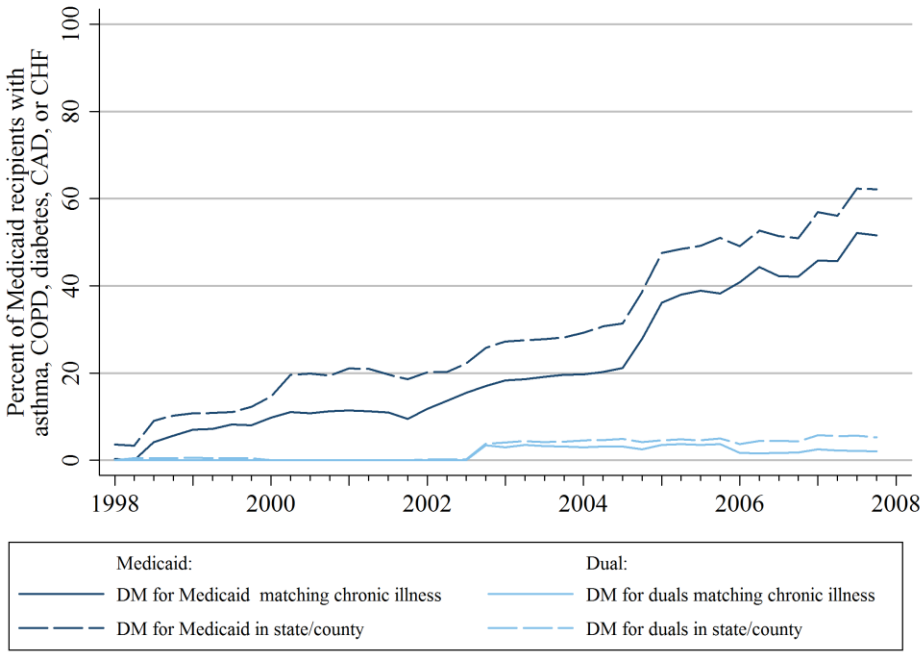


Panel B: States that implemented at least one Medicaid DM Program, 1991 through 2008



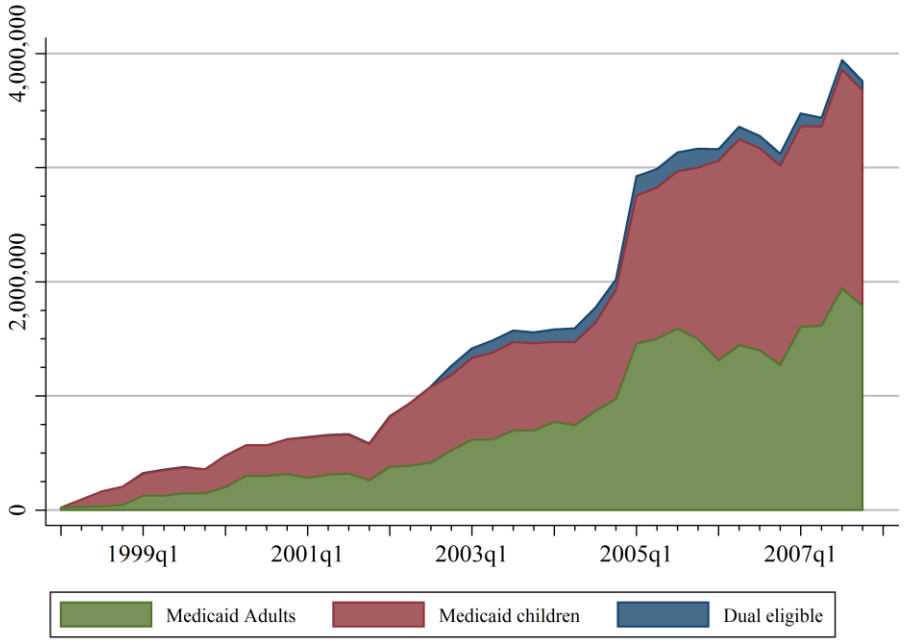
Source: Tabulations from author's survey of Medicaid disease management programs. The dark-shaded regions identify states that implemented a full DM program during the period; the light=shaded regions indicate pilot programs. Alaska and Hawaii did not implement a DM program or pilot.

Figure 7: Percent of chronically ill Medicaid recipients who have DM in their state, MEPS 1998-2007



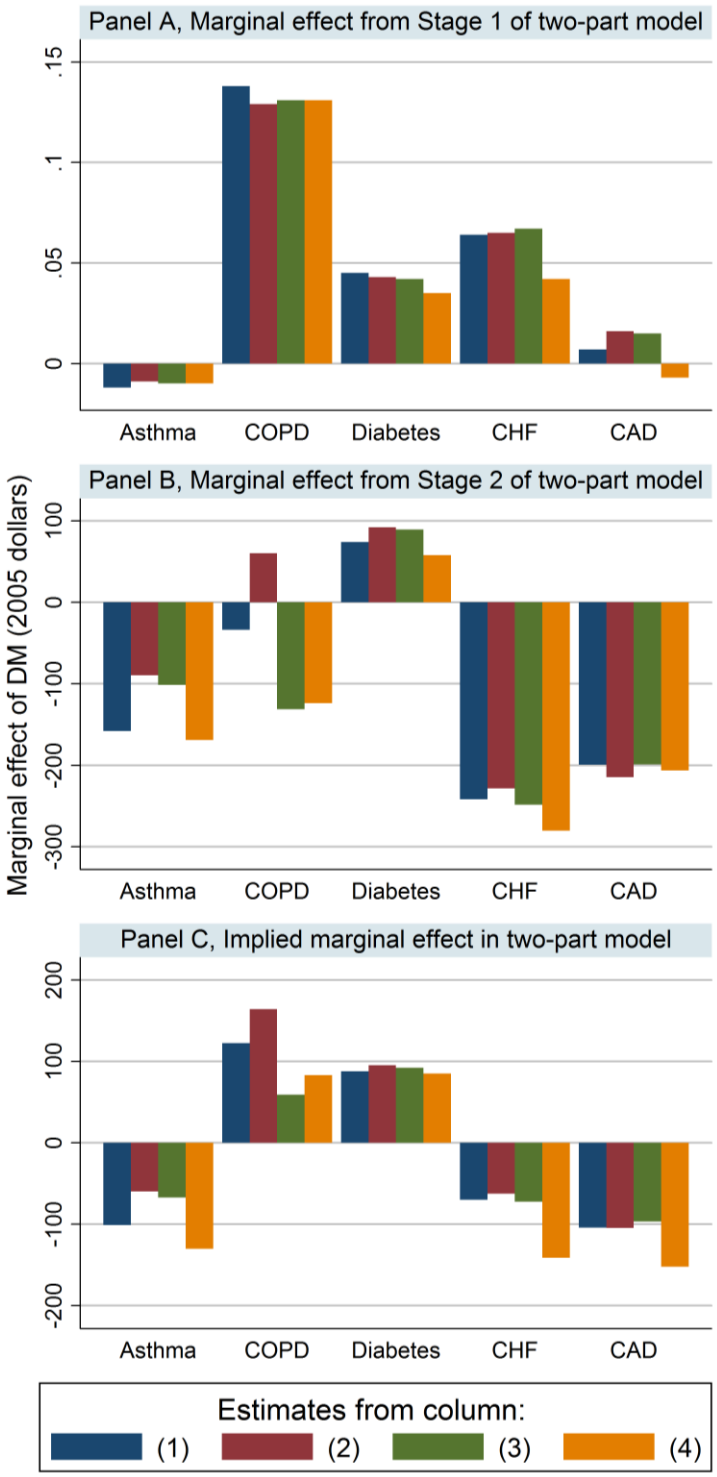
Source: Author's tabulations using DM program information merged with MEPS (1998-2007). Plot of the (unweighted) percent of individuals in MEPS with Medicaid insurance and a chronic disease (asthma, COPD, diabetes, CAD, or CHF) who live in a state/county (solid line) with a DM program that matches their insurance group and their chronic disease or (dashed) with a DM program that matches their insurance group, regardless of disease.

Figure 8: Weighted estimates of the number of individuals with DM coverage, MEPS 1998-2007



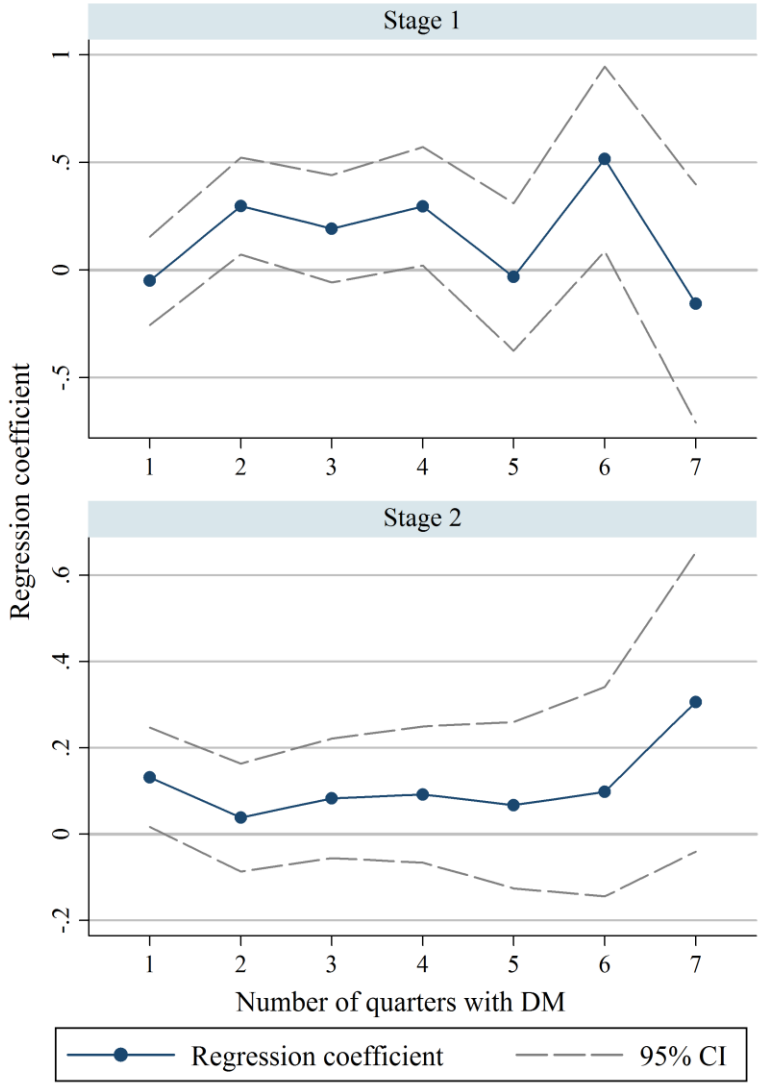
Source: Author's tabulations of MEPS data. Disease Management data, from author's survey of Medicaid disease management programs, was merged with MEPS as described in text. Figure plots estimates of the *weighted* number of people who are matched to a DM program using my DM_{it} coverage variable (see text for details). Observations are weighted using the MEPS survey weights. This is a plot of columns 1, 2, and 3 in **Table 3**.

Figure 9: Net effect of Medicaid Disease Management on quarterly health expenditures, by DM target disease



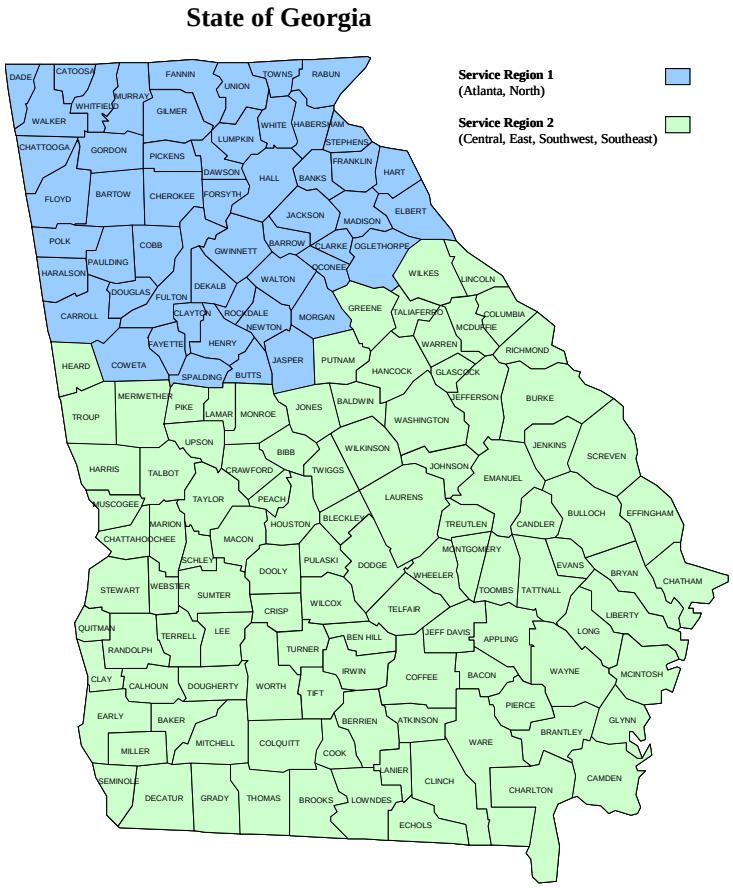
Notes: This figure plots coefficients from Panels B and C of **Table 9**. For disease "X", the height of the bar equals the sum of the marginal effects for the variables "DM" and "X DM." For example, the first blue bar in the Panel C is $-37.46 - 63.66 = -101.12$ dollars, corresponding to the two-part model marginal effect for DN for asthma (only) patients, taken from Column 1, Panel C of Table 9.

Figure 10: Effect of Medicaid DM on quarterly health expenditures for individuals with variation in DM coverage during MEPS survey



Notes: This figure plots the regression coefficients and 95 percent confidence intervals from **Table 14**, column 2 (top) and column 4 (bottom). See table for details.

Figure 11: Map of the administrative regions of the GEC program



Source: Georgia Department of Community Health (2005, p. 73)

Figure 12: Timeline of the GEC program and data set coverage

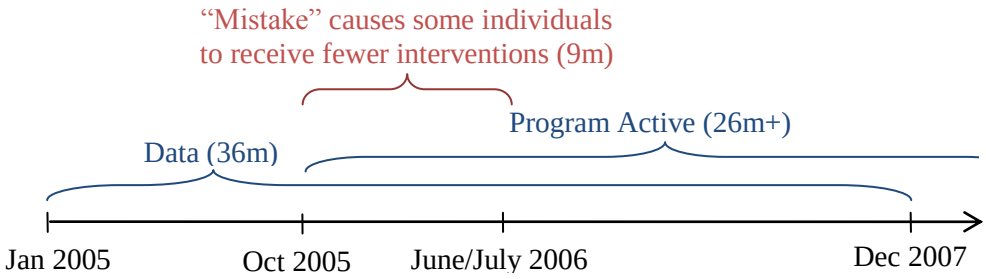
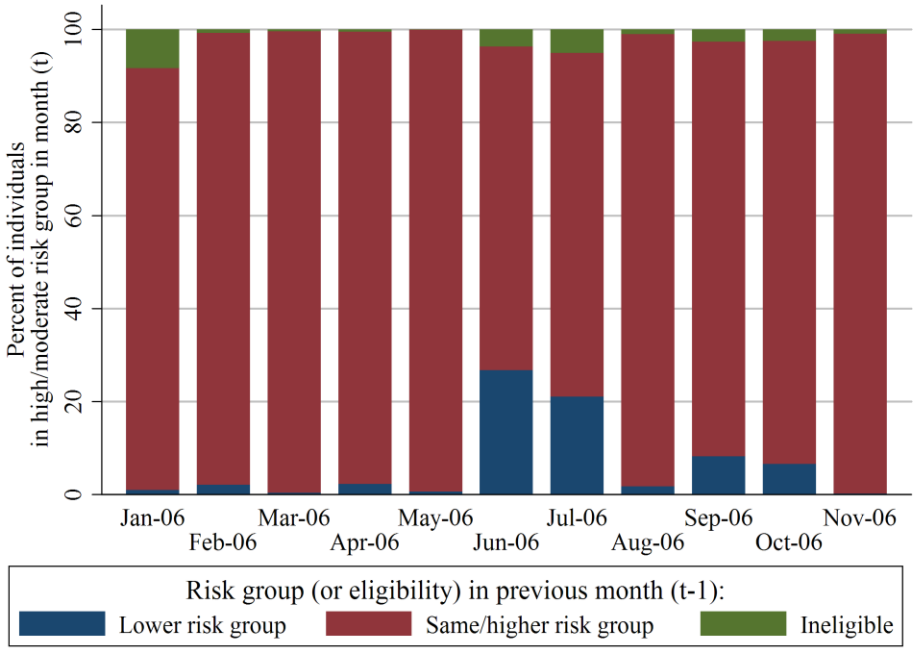


Figure 13: Changes in risk group assignment before/after June 2006

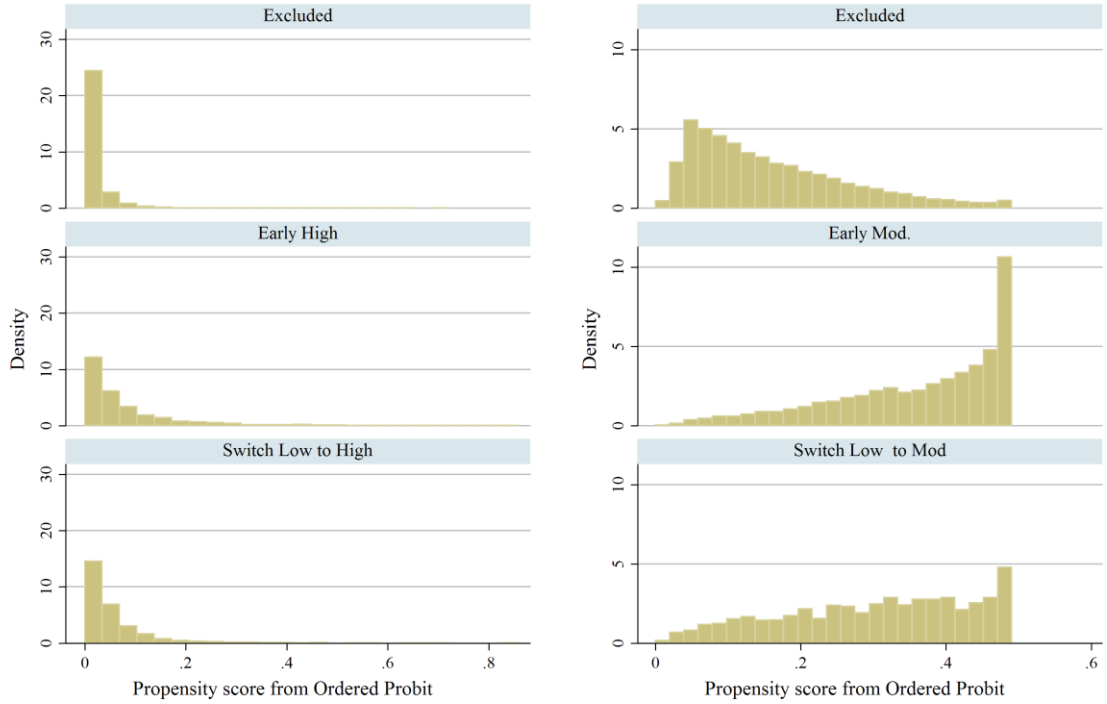


Source: Author's tabulations from GEC administrative data.

Figure 14: Propensity score of selection into GEC program moderate and high risk groups

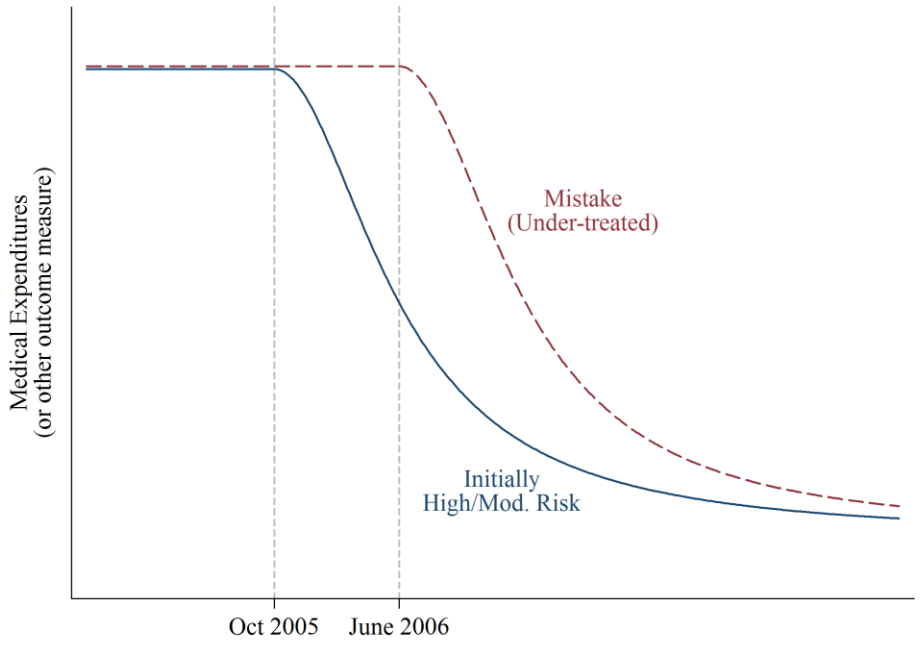
Panel A: Probability of selection into high risk group

Panel B: Probability of selection into moderate risk group



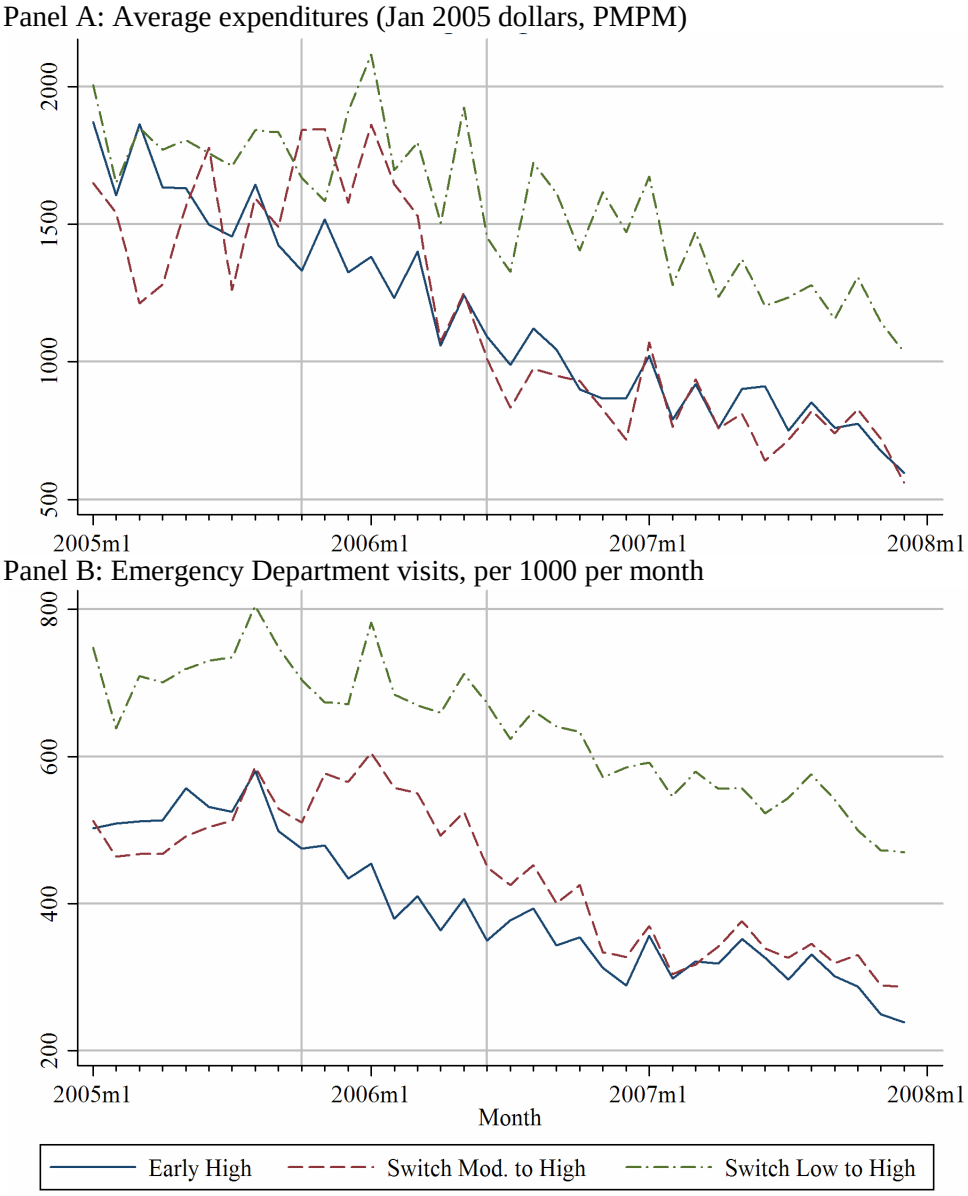
Notes: Histograms of propensity scores from an ordered probit regression of selection into the moderate or high risk groups on the following variables: a quadratic polynomial in pre-period health expenditures; the number of admissions to hospitals, emergency departments, and residential facilities; a large array of chronic disease dummy variables; and dummy variables for 10-year age groups interacted with gender. Rows separate individuals who were never assigned to the high [moderate] risk group ("excluded"), those who were assigned to a high [moderate] risk group "early" (in October 2005), and those who were delayed entry into this group but upgraded into the high [moderate] risk group when the mistake was fixed in Summer 2006.

Figure 15: Conceptual graph of the potential effect of DM on medical expenditures



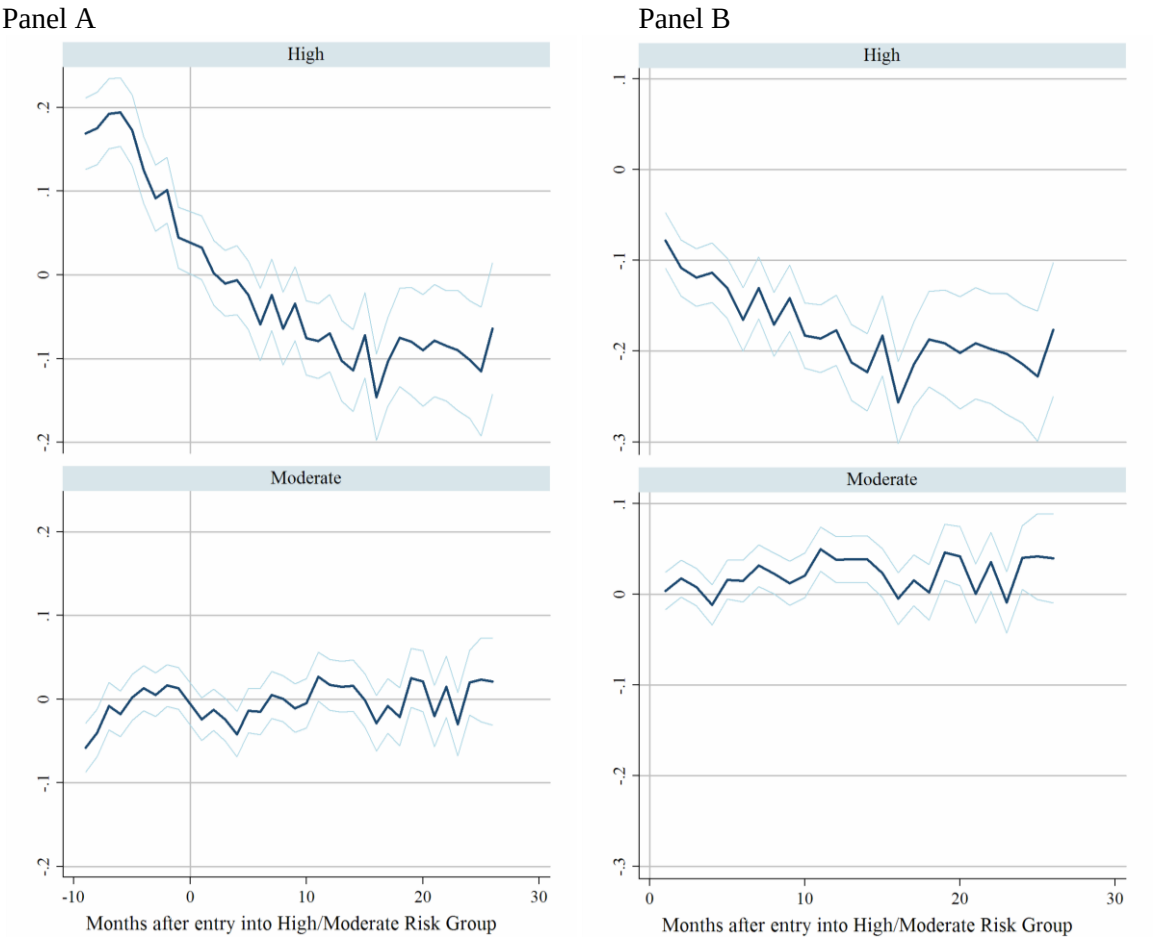
Notes: This graph shows a potential response to DM program interventions for a representative individual in the case that he or she was initially (1) assigned to the high or moderate risk groups as "intended" or (2) assigned to a lower risk category by "mistake." The graph abstracts away from typical month-to-month variation, the effect of entering the low-risk group in October 2005, and secular trends in medical expenditures.

Figure 16: Mean medical expenditures and number of emergency department visits for high-risk patients



Notes: Plot of simple averages, where each line indicates the patient's classification into the high-risk categories by January 2005, and if they were reclassified in July 2006 when the administrative error (described in the text) was fixed. Total medical expenditures do not include DM program fees. Emergency department visits indicates the number of unique days in a month a patient is newly admitted to an emergency department.

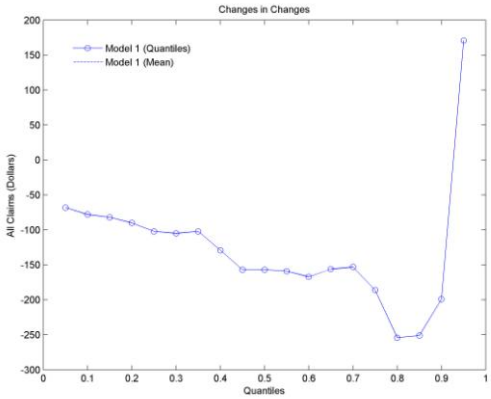
Figure 17: The effect of high and moderate-intensity DM interventions



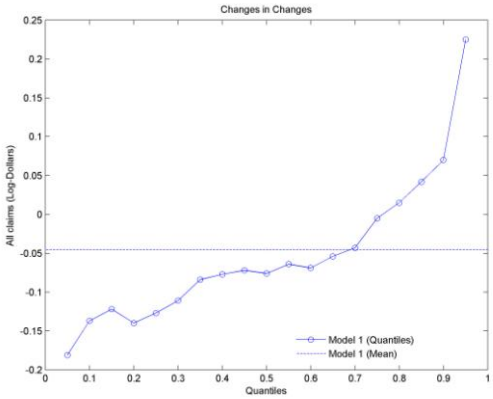
Notes: This graph plots coefficient estimates and 95 percent confidence intervals from a single regression of ln(total claims) on an array of dummies indicating months after entry into high and moderate groups, with month and individual fixed effects. Confidence interval is based on robust standard errors, clustered at the individual level. The coefficients are normalized to 0 in the month they enter the risk group in Panel A; in Panel B, the coefficients are fixed at 0 in all pre-program months. See text for details.

Figure 18: Estimates of the effect of DM across the distribution of health care costs

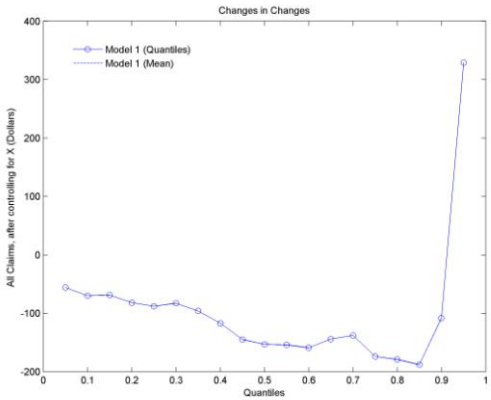
Panel A: Expenditures



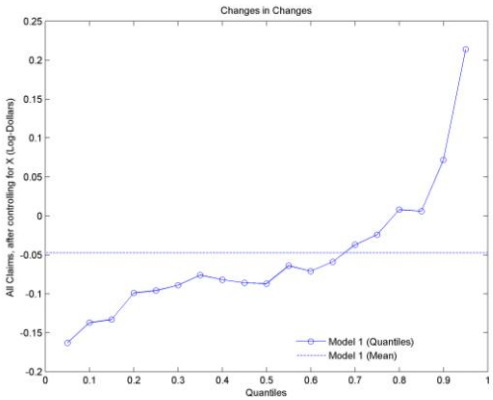
Panel B: log(Expenditures)



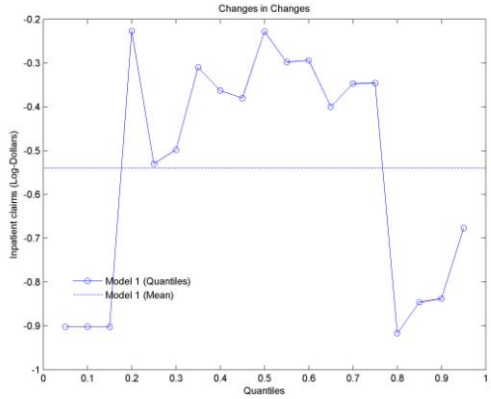
Panel C: Expenditures, with controls



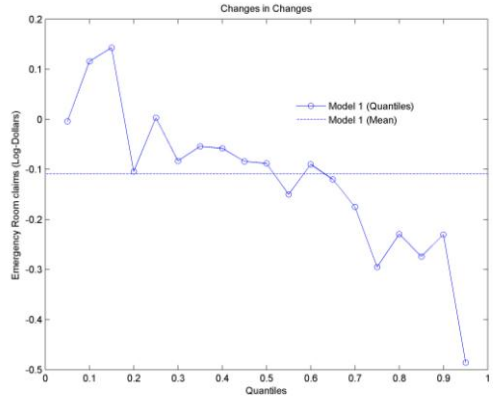
Panel D: log(Expenditures), with controls



Panel E: log(Inpatient medical expenditures)



Panel F: log(Emergency dept. expenditures)



Notes: These graphs present the information reported in Table 28, the estimates of the effect of treatment on the treated in a Changes-in-Changes model. Each panel corresponds to one of the rows in the table. The horizontal line is the mean CIC estimate. See the notes to Table 28 for details.

Appendices

Appendix A: Medicaid Disease Management program database

I compile a data set to describe the history of Medicaid DM programs for all 50 states plus the District of Columbia that have been implemented through 2008. For each program, I track the beginning and end dates of the program and group programs into broad categories based on program characteristics (e.g., full program or pilot program; statewide or limited regions; in-house, DMO, or hybrid). I also note how the program defines its target population: limiting or expanding eligibility according to disease diagnosis(es), past utilization, geographical limitations, categorical eligibility restrictions (e.g., the SSI/ABD population) or other factors. The programs are listed in Appendix Table A1, with the dates of activity, classification, and diseases targeted. States are listed in chronological order by the date of their first DM program or pilot.

Information on Medicaid DM programs was collected from a variety of sources. Although I am not aware of any references that describe all Medicaid DM programs that have been implemented, a number of sources discuss one or more Medicaid DM programs. The starting point for my research was the intersection of all programs discussed in the following documents:

- Abraham (2008)
- APS Healthcare (2009)
- Arora, Boehm, Chimento, Moldawer, and Tsien (2008)
- Bella, Shearer, LLanos, and Somers (2008)
- Brown and Matthews (2008)
- Coffey et al. (2004; 2006)
- Costich (2007)
- Faulkner (2003)
- Flowers (2007)
- Gillespie and Rossiter (2003)
- The Health Strategies Consultancy (2004ab)
- Healthcare Financial Management Association (2003)
- Kaye (2005)
- Kuo (2004)
- Lambert, Gale, Bird and Hartley (2001)
- National Association of State Medicaid Directors (2006, 2007)
- National Conference of State Legislatures (2003, 2007ab)
- National Governors Association Center for Best Practices (2004, 2006)
- Owens (2006)
- Rosenbaum, Markus, Scheer, and Harty (2008)
- Rosenman (2006)

- Saunders (2009)
- Schwartz, and Mollica (2007)
- Smith, V. et al. (2002; 2003; 2003; 2004; 2004; 2005, 2006, 2007, 2008)
- Wheatley (2001, 2002)
- Williams (2004)

In addition, these documents were supplemented with other information, gleaned state-by-state, from over 250 sources. These include state Medicaid offices and websites (and other government agencies), program status updates and evaluations, DMO companies, press releases or newspaper articles, personal communication with relevant parties, etc. For each state, I also used the state's Medicaid program website, which often included web pages on DM programs (when present) and other aspects of the Medicaid program. Finally, I also used documents from the Centers for Medicare & Medicaid Services (www.cms.gov), such the list of waiver programs and other initiatives. A full list references can be provided upon request.

My definition of DM has remained broad enough to reflect the evolution in DM program design over the last few decades, although several types of programs are specifically not included:

- First, I exclude programs that did not target Medicaid patients in particular, such as broad public health education campaigns that included Medicaid patients.
- Second, programs that were implemented by Medicaid Managed Care (MMC) plans were excluded because (i) these programs do not financially cost/benefit Medicaid through any mechanism other than the capitated rate the government pays for each enrollee and (ii) the inability to collect consistent, comprehensive information on the internal structure of MMC plans throughout the sample period.[75]
- Third, I restrict the data to programs that are designed to improve the health care of targeted individuals beyond usual care and do not track the broad evolution in Medicaid. For example, a number of states switched from a Fee-for-Service system to a "gatekeeper" Primary Care

[75] For information on disease management within MMC, see Kaye (2005).

Case Management (PCCM) system for their Medicaid enrollees, although this is not tracked as DM, *per se*.

- Forth, I exclude some types of recent programs that target quality improvements in health care, but use substantially different tools than the standard components of DM such as patient education, care coordination, and so on. For example, I exclude initiatives that would be classified as "Pay for Performance" (financial incentives to medical providers for improved health outcomes).

In some cases, these distinctions required subjective judgment (especially earlier years); I generally followed the existing literature, including programs referred to as "disease management" by other researchers and excluding programs that were not. In the chapter, I refer to a subset of programs meeting a more *strict* definition of "disease management." This group identifies the "traditional," telephonic-oriented programs targeted at subgroups of the population who are high-risk, typically outsourced to a third-party DM provider. This *strict* definition rules out (1) programs that feature relatively less-intensive interventions, (2) programs that are exclusively pharmaceutical-management programs, (3) programs that interact exclusively with health providers (not patients), (4) pilot programs with low enrollment, and (5) a few other programs on a case-by-case basis.

To provide one example of a program that does not meet the *strict* definition, the West Virginia Health Initiatives Project (WVHIP) is a diabetes program that is referred to as a "disease management program" by several sources. (Wheatley 2001; CSG and Costich 2007; MK Owens 2006; SS Brown and Matthews 2003; Coffey, Matthews, and McDermott 2004) The program included training for medical providers and staff, reimbursement to Medicaid providers for additional diabetic care services and education, and reports ("registries") for the providers listing their patients' hospital/ED activity and cost profiles. Thus, the program had no direct interaction with the patients and thus does not meet my "strict" definition of DM, although I include it in my broad list of DM programs (because of its inclusion as DM by the literature).

The process discussed above identified DM programs or pilots programs in 41 states, with the first program in 1991. In most remaining states (Arizona, Delaware, the District of Columbia, Hawaii, Kentucky, New Mexico, South Dakota, Tennessee, and Wisconsin), I did not identify a DM program that fit these criteria, although it was clear that at least some Medicaid patients receive DM-type services through their Medicaid Managed Care plans. Alaska, the remaining state, did not appear to have any DM program.

Appendix Table A1: List of Medicaid DM Programs

States are listed in chronological order by the date of first DM program or pilot

State	Program	Dates	Program or Pilot	Organ-ization	Statewide	Asthma	COPD	Diabetes	CHF	CAD	Mental Health	Other Disease	High Cost/Util.	Strict Definition
Maryland	Diabetes Care Program	6/1991-6/1997	Program	In-house	Statewide			Y						
Virginia	Virginia Health Outcomes Partnership (VHOP)	10/1995-9/1996	Pilot	In-house	Limited Regions	Y								
	Expansion of VHOP	10/1997-9/2001	Program	RX	Statewide	Y		Y	Y		Y	Y		Y
	Healthy Returns	6/2004-1/2006	Pilot	DMO	Limited Regions				Y	Y				
	"	1/2006-*	Program	DMO	Statewide	Y	Y	Y	Y	Y				Y
Massa-chusetts	Massachusetts Behavioral Health Partnership	10/1996-*	Program	DMO	Statewide						Y			
	Essential Care	12/2003-*	Pilot	DMO	Statewide	Y		Y					Y	
	PCC Site-Based Care Management Pilot Program	6/2005-*	Pilot	DMO	Limited Regions							Y		
Florida	Florida Medicaid Disease Management Initiative	8/1998-6/2002	Program	DMO	Statewide	Y		Y	Y			Y		
	Positive Healthcare Disease Management - Florida	9/1999-*	Program	DMO	Statewide									
	Florida: A Healthy State	7/2002-9/2005	Program	DMO	Statewide	Y		Y	Y		Y	Y		Y
	Continuation of Disease Management	10/2005-12/2006	Program	DMO	Statewide	Y	Y	Y				Y		Y
	Healthier Florida	1/2007-*	Program	DMO	Statewide	Y	Y	Y	Y			Y		Y
	Hemophilia	1/2007-*	Program	DMO	Statewide									Y
	Dual Eligible Demonstration	1/2008-*	Pilot	DMO	Limited Regions			Y	Y	Y				
Missis-sippi	RX management	10/1998-3/2003	Program	RX	Statewide	Y		Y				Y		
	DM Program	4/2003-10/2007	Program	DMO	Statewide	Y		Y				Y		Y
North Carolina	Community Care of North Carolina (Asthma)	7/1998-12/2002	Program	In-house	Limited Regions	Y								
	"	7/2002-*	Program	In-house	Statewide	Y								
	Community Care of North Carolina (Diabetes)	7/2002-*	Program	In-house	Statewide			Y						
	Community Care of North Carolina (CHF)	9/2004-6/2006	Program	In-house	Statewide				Y					

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Appendix Table A1: List of Medicaid DM Programs (continued)

States are listed in chronological order by the date of first DM program or pilot

State	Program	Dates	Program or Pilot	Organ-ization	State-wide	Asthma	COPD	Diabetes	CHF	CAD	Mental Health	Other Disease	High Cost/Util.	Strict Definition
North Carolina (cont.)	Community Care of North Carolina (COPD)	7/2006-*	Pilot	In-house	Limited Regions		Y							
	Community Care of North Carolina (Chronic Care Program)	4/2006-*	Pilot	In-house	Limited Regions								Y	
	Community Care of North Carolina (Heart Failure)	7/2006-*	Program	In-house	Statewide				Y					
	HIV/AIDS Case Manager Program	7/2007-*	Program		Statewide									
Utah	Hemophilia program	6/1998-1/2004	Program	DMO	Statewide									Y
Texas	Medicaid Diabetes Care pilot program in Bexar County	7/1999-6/2001	Pilot	In-house	Limited Regions			Y						
	Pediatric Asthma Management Pilot Program	9/2002-8/2004	Pilot	In-house	Limited Regions	Y								
	Texas Medicaid Enhanced Care Program	11/2004-*	Program	DMO	Statewide	Y	Y	Y	Y	Y				Y
	Integrated Care Management program	2/2008-*	Program	DMO	Limited Regions							Y		Y
Washing-ton	Diabetes collaborative	10/1999-6/2002	Pilot	In-house	Statewide			Y						
	Precursor to Chronic Care Management program	4/2002-6/2006	Program	DMO	Statewide	Y	Y	Y	Y					Y
	Chronic Disease Management Program	1/2007-*	Pilot	DMO	Statewide								Y	Y
Maine	Emergency Department prevention program	6/2000-7/2006	Program	In-house	Statewide								Y	Y
	Pain Management program	1/2001-*	Program	In-house	Statewide							Y		
	HIV/AIDS program	7/2002-*	Program	DMO	Statewide									
	Diabetes Program	8/2003-11/2005	Program	In-house	Statewide			Y						Y
	MaineCare Care Management Program	8/2006-7/2007	Pilot	DMO	Limited Regions								Y	
	"	7/2007-*	Program	DMO	Limited Regions								Y	Y

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Appendix Table A1: List of Medicaid DM Programs (continued)

States are listed in chronological order by the date of first DM program or pilot

State	Program	Dates	Program or Pilot	Organ-ization	State-wide	Asthma	COPD	Diabetes	CHF	CAD	Mental Health	Other Disease	High Cost/Util.	Strict Definition
Michigan	Diabetes Self Management Program	5/2000-*	Program	In-house	Statewide			Y						
West Virginia	West Virginia Health Initiative Project (WVHIP)	10/2000-9/2001	Program	In-house	Limited Regions			Y						
	"	7/2001-*	Program	In-house	Statewide			Y						
Rhode Island	Connect CARRE Care Management and Wellness Program	11/2001-*	Program	Hybrid	Statewide	Y	Y	Y	Y		Y			
	Connect Care Choice	8/2007-*	Program	In-house	Statewide	Y	Y	Y	Y		Y			Y
Arkansas	Arkansas Diabetes Prevention & Control Program	10/2002-*	Pilot	DMO	Limited Regions			Y						
	Antenatal and Neonatal Guidelines, Education, and Learning Systems (ANGELS)	2/2003-*	Pilot	DMO	Statewide							Y		
Idaho	Medicaid Asthma Medical Management Program	1/2002-*	Program	In-house	Statewide	Y								
	Medicaid Diabetes Medical Management Program	1/2002-*	Program	In-house	Limited Regions			Y						
	Pay for Performance pilot: diabetes	6/2006-*	Pilot	In-house	Limited Regions			Y						
Louisiana	DM Demonstration	11/2002-11/2005	Pilot	DMO	Limited Regions			Y	Y	Y				
	Asthma HELP	4/2005-8/2005	Pilot	DMO	Limited Regions	Y								
	"	1/2006-*	Program	DMO	Statewide	Y								Y
Missouri	Get Well program	2/2002-6/2006	Program	DMO	Statewide	Y	Y	Y	Y					Y
	Chronic Care Improvement Program	7/2006-*	Program	DMO	Statewide	Y	Y	Y	Y	Y	Y	Y		Y
Oregon	DM Program	10/2002-10/2007	Program	DMO	Statewide	Y	Y	Y	Y	Y			Y	Y
Indiana	Indiana Chronic Disease Management Program (ICDMP)	6/2003-11/2007	Program	DMO	Limited Regions			Y	Y				Y	Y
	"	6/2004-11/2007	Program	DMO	Statewide	Y							Y	Y

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Appendix Table A1: List of Medicaid DM Programs (continued)

States are listed in chronological order by the date of first DM program or pilot

State	Program	Dates	Program or Pilot	Organ-ization	State-wide	Asthma	COPD	Diabetes	CHF	CAD	Mental Health	Other Disease	High Cost/Util.	Strict Definition
Indiana (cont.)	Care Select	11/2007-*	Program	DMO	Limited Regions	Y	Y	Y	Y	Y				Y
	Care Select	3/2008-*	Program	DMO	Statewide	Y	Y	Y	Y	Y				Y
Iowa	Diabetes Disease Management Pilot Program	10/2003-9/2004	Pilot	DMO	Limited Regions			Y						
	Iowa Medicaid Care Management (IMCM)	7/2005-*	Pilot	DMO	Statewide	Y		Y	Y					
Alabama	Patient 1st In-Home Monitoring Program	10/2004-*	Program	Hybrid	Statewide			Y				Y		Y
	Medicaid Transformation Grant - Q4U Care Management Component	2/2008-*	Pilot	Hybrid	Limited Regions	Y		Y						
Colorado	Asthma Management Program	11/2004-*	Pilot	DMO	Statewide	Y								Y
	Disease Management Demonstration Pilot	7/2004-12/2004	Pilot	DMO	Limited Regions	Y	Y	Y			Y	Y		
	Diabetes Disease Management Program	2/2005-*	Program	DMO	Statewide			Y						Y
	Chronic Obstructive Pulmonary Disease (COPD) Program	11/2007-*	Pilot	DMO	Statewide		Y							
	Congestive Heart Failure (CHF) Program	7/2007-*	Pilot	DMO	Statewide				Y					
	High-Risk Pregnancy Program (a.k.a. High-Risk Obstetrics)	11/2007-*	Pilot	DMO	Statewide							Y		
	Telehealth Program for Chronic Conditions (CHF, COPD and Diabetes)	7/2007-*	Pilot	DMO	Statewide		Y	Y	Y					
	Weight Management Program	2/2008-*	Pilot	DMO	Statewide							Y		
Montana	Nurse First program	1/2004-*	Program	DMO	Statewide	Y		Y	Y			Y		Y
Ohio	Enhanced Care Management Program	10/2004-10/2005	Program	DMO	Limited Regions	Y	Y	Y	Y			Y		Y

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Appendix Table A1: List of Medicaid DM Programs (continued)

States are listed in chronological order by the date of first DM program or pilot

State	Program	Dates	Program or Pilot	Organ-ization	State-wide	Asthma	COPD	Diabetes	CHF	CAD	Mental Health	Other Disease	High Cost/Util.	Strict Definition
Oklaho-ma	SoonerCare Emergency Room Utilization Project (ERU)	1/2004-5/2006	Program	In-house	Statewide								Y	
	Health Management Program	2/2008-*	Program	DMO	Statewide	Y	Y	Y	Y	Y		Y	Y	Y
Wyom-ing	Healthy Together Total Population Management Program	7/2004-*	Program	DMO	Statewide	Y	Y	Y	Y	Y	Y	Y		Y
Califor-nia	Central Valley Diabetes Project	4/2005-3/2007	Pilot	In-house	Limited Regions			Y						
	California Disease Management Pilot	7/2007-*	Pilot	DMO	Limited Regions	Y	Y	Y	Y	Y				Y
Georgia	Georgia Enhanced Care	10/2005-*	Program	DMO	Statewide	Y	Y	Y	Y	Y	Y	Y		Y
	Georgia Medicaid Management Program (GAMMP)	1/2007-*	Program	DMO	Statewide	Y	Y	Y	Y		Y			Y
New Hamp-shire	Medicaid Value Program: Health Supports for Consumers with Chronic Conditions	2/2005-8/2007	Pilot	DMO	Limited Regions			Y	Y					
	New Hampshire Medicaid Health Management Program	3/2005-*	Program	DMO	Statewide	Y	Y	Y	Y	Y				Y
	GraniteCare Enhanced Care Coordination Pilot Program	7/2007-*	Program	DMO	Statewide	Y	Y	Y	Y					Y
Pennsyl-vania	ACCESS Plus	3/2005-*	Program	DMO	Statewide	Y	Y	Y	Y	Y		Y		Y
Illinois	Your Healthcare Plus	6/2006-*	Program	DMO	Statewide	Y	Y	Y	Y	Y	Y	Y		Y
Kansas	Enhanced Care Management Program	3/2006-*	Pilot	DMO	Limited Regions								Y	
Minneso-ta	Intensive Care Coordination	1/2006-*	Pilot	DMO	Statewide								Y	Y
New York	Medicaid Disease and Care Management Demonstration Programs	7/2006-*	Pilot	DMO	Limited Regions	Y	Y	Y	Y	Y	Y			Y
Vermont	Care Coordination Program	6/2006-*	Program	In-house	Limited Regions	Y	Y	Y	Y	Y	Y	Y	Y	Y
	Chronic Care Management Program	7/2007-*	Program	DMO	Statewide	Y	Y	Y	Y	Y	Y	Y		Y

(continued on next page)

Appendix Table A1: List of Medicaid DM Programs (continued)

States are listed in chronological order by the date of first DM program or pilot

State	Program	Dates	Program or Pilot	Organ-ization	State-wide	Asthma	COPD	Diabetes	CHF	CAD	Mental Health	Other Disease	High Cost/Util.	Strict Definition
Connect-icut	Easy Breathing Program	1/2007-*	Program	DMO	Statewide	Y								
	Disease Management Project	1/2008-*	Program	DMO	Statewide			Y	Y	Y		Y		
New Jersey	NJ HealthyLiving: APS project	3/2007-*	Pilot	DMO	Limited Regions	Y	Y	Y	Y					
North Dakota	Experience HealthND	11/2007-*	Program	DMO	Statewide	Y	Y	Y	Y					Y
South Carolina	Medical Homes Network Program	8/2007-*	Program	DMO	Limited Regions	Y	Y	Y	Y	Y		Y		Y
Nebraska	Enhanced Care Coordination Program	9/2008-*	Program	DMO	Statewide								Y	Y
Nevada	Care Management	1/2008-*	Program	DMO	Statewide							Y		Y

Source: Programs from author's survey of Medicaid disease management programs.
*End-date top-coded to 2008q4

Appendix B: Supplementary results from the Medical Expenditure Panel Survey

Appendix Table B1: GLM coefficients and marginal effects for all control variables in Table 4 (selected columns)

Dependent Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Total Expenditures				**Medicaid Expenditures**			
Corresponding Regression	Table 4, Column 1				Table 4, Column 5			
Stage of Two-Part Model	Stage 1 (Panel A)		Stage 2 (Panel B)		Stage 1 (Panel A)		Stage 2 (Panel B)	
	Coeffi-cients	Marginal Effect	Coeffi-cients	Marginal Effect	Coeffi-cients	Marginal Effect	Coeffi-cients	Marginal Effect
DM	0.0375*	0.0148*	-0.0882	-86.69	0.129***	0.0503***	-0.0113	-8.520
	[0.0223]	[0.00875]	[0.0631]	[60.02]	[0.0220]	[0.00867]	[0.0725]	[54.46]
Insured by Medicare	0.157***	0.0614***	0.285***	318.3***	-1.227***	-0.367***	-0.872***	-523.9***
	[0.0319]	[0.0123]	[0.0677]	[82.11]	[0.0319]	[0.00627]	[0.0971]	[48.15]
Insured as Dual Eligible	0.222***	0.0858***	0.254***	282.7***	1.437***	0.508***	0.699***	672.1***
	[0.0275]	[0.0104]	[0.0492]	[59.90]	[0.0286]	[0.00723]	[0.0807]	[98.98]
Insured by Neither	-0.561***	-0.221***	-0.175***	-167.9***	-1.487***	-0.434***	0.102	81.32
	[0.00854]	[0.00329]	[0.0676]	[60.01]	[0.0119]	[0.00225]	[0.221]	[185.1]
Diagnosis of Asthma	0.454***	0.171***	0.193***	208.9***	0.408***	0.160***	0.185**	147.8**
	[0.0126]	[0.00450]	[0.0610]	[71.15]	[0.0120]	[0.00473]	[0.0750]	[64.55]
Diagnosis of COPD	0.170***	0.0660***	0.318***	378.9***	0.139***	0.0542***	0.255***	217.9**
	[0.0441]	[0.0167]	[0.0759]	[104.8]	[0.0361]	[0.0143]	[0.0898]	[86.41]
Diagnosis of Diabetes	0.643***	0.231***	0.461***	568.0***	0.499***	0.197***	0.402***	357.6***
	[0.0244]	[0.00756]	[0.0405]	[60.43]	[0.0197]	[0.00768]	[0.0439]	[45.81]
Diagnosis of CHF	0.311***	0.118***	0.296***	342.9***	0.229***	0.0899***	0.177***	144.5***
	[0.0278]	[0.0101]	[0.0501]	[65.56]	[0.0232]	[0.00923]	[0.0563]	[49.31]
Diagnosis of CAD	0.214***	0.0827***	0.341***	408.1***	0.178***	0.0699***	0.294***	254.9***
	[0.0375]	[0.0141]	[0.0596]	[82.67]	[0.0300]	[0.0119]	[0.0697]	[68.61]
Male	-0.129***	-0.0510***	0.1000**	103.4**	-0.131***	-0.0504***	0.0557	42.49
	[0.00830]	[0.00328]	[0.0403]	[43.02]	[0.00840]	[0.00321]	[0.0459]	[35.66]
Age <10	-0.684***	-0.267***	0.252**	271.4**	-0.424***	-0.159***	0.154	120.2
	[0.0345]	[0.0130]	[0.104]	[119.3]	[0.0307]	[0.0112]	[0.111]	[90.06]
Age 10-19	-0.744***	-0.290***	0.0440	45.58	-0.558***	-0.203***	-0.0726	-53.87
	[0.0337]	[0.0125]	[0.0877]	[92.03]	[0.0298]	[0.0101]	[0.0940]	[68.42]
Age 20-29	-0.536***	-0.211***	0.00590	6.050	-0.326***	-0.120***	0.145*	116.9*
	[0.0333]	[0.0126]	[0.0665]	[68.25]	[0.0293]	[0.0101]	[0.0794]	[67.89]
Age 30-39	-0.456***	-0.180***	0.0341	35.35	-0.256***	-0.0952***	0.0427	32.96
	[0.0336]	[0.0130]	[0.0698]	[73.35]	[0.0296]	[0.0105]	[0.0817]	[64.17]
Age 40-49	-0.242***	-0.0962***	0.170**	186.8**	-0.0566*	-0.0217*	0.152*	123.3
	[0.0342]	[0.0136]	[0.0713]	[84.13]	[0.0298]	[0.0113]	[0.0884]	[76.30]
Age 50-59	0.0769**	0.0301**	0.159***	174.1**	0.169***	0.0662***	0.181**	148.5**
	[0.0359]	[0.0140]	[0.0607]	[70.95]	[0.0302]	[0.0120]	[0.0743]	[65.65]
Age 70-79	-0.0671*	-0.0266*	-0.111**	-108.4**	-0.123***	-0.0465***	-0.105	-75.81
	[0.0402]	[0.0160]	[0.0534]	[49.43]	[0.0314]	[0.0117]	[0.0704]	[48.52]
Age 80-89	0.121**	0.0473**	0.0504	52.70	0.0205	0.00792	0.119	95.59
	[0.0492]	[0.0189]	[0.0599]	[64.28]	[0.0378]	[0.0146]	[0.0788]	[67.03]
Age ≥90	-0.0545	-0.0216	0.505	671.6	0.179	0.0701	0.765	870.0
	[0.137]	[0.0543]	[0.323]	[547.5]	[0.114]	[0.0453]	[0.524]	[853.1]
Education = No degree	-0.154***	-0.0609***	-0.0359	-36.38	-0.0182	-0.00700	0.0355	27.15
	[0.0158]	[0.00627]	[0.0350]	[35.22]	[0.0151]	[0.00580]	[0.0402]	[30.93]
Education = GED	-0.0587**	-0.0232**	-0.150***	-143.2***	0.0259	0.0100	-0.105*	-75.52**
	[0.0276]	[0.0110]	[0.0502]	[44.91]	[0.0270]	[0.0105]	[0.0544]	[37.59]
Education = Bachelor's	0.0711*	0.0279*	-0.0477	-47.66	-0.104***	-0.0393***	-0.0424	-31.50
	[0.0426]	[0.0166]	[0.0937]	[91.46]	[0.0399]	[0.0149]	[0.125]	[91.01]
Education = Graduate	0.0462	0.0181	-0.140	-134.0	-0.0549	-0.0210	-0.0917	-66.44
	[0.0726]	[0.0284]	[0.170]	[151.2]	[0.0703]	[0.0267]	[0.178]	[123.3]
Education = Other/Ukn	0.0276	0.0109	0.105	113.0	0.0215	0.00831	0.176*	145.6*
	[0.0380]	[0.0149]	[0.0707]	[79.95]	[0.0358]	[0.0139]	[0.0946]	[85.05]
Education = NA/Under-16	-0.0761***	-0.0300***	-1.079***	-1,105***	-0.0120	-0.00464	-0.897***	-688.3***
	[0.0213]	[0.00838]	[0.0790]	[85.99]	[0.0212]	[0.00817]	[0.0785]	[63.22]
Race/Ethn. = Black	-0.319***	-0.126***	-0.201***	-195.2***	-0.233***	-0.0883***	-0.165***	-119.9***
	[0.0116]	[0.00460]	[0.0405]	[38.13]	[0.0116]	[0.00429]	[0.0466]	[33.00]
Race/Ethn. = Hispanic	-0.223***	-0.0882***	-0.211***	-208.9***	-0.133***	-0.0510***	-0.177**	-130.6**
	[0.0118]	[0.00468]	[0.0625]	[59.67]	[0.0119]	[0.00454]	[0.0770]	[55.27]
Race/Ethn. = Asian/PI	-0.409***	-0.162***	-0.470***	-388.3***	-0.321***	-0.117***	-0.586***	-341.2***
	[0.0261]	[0.0101]	[0.114]	[75.59]	[0.0260]	[0.00883]	[0.101]	[45.47]
Race/Ethn. = Native Am.	-0.182***	-0.0723***	-0.0297	-29.88	-0.111**	-0.0420**	-0.0511	-37.79
	[0.0446]	[0.0178]	[0.104]	[102.8]	[0.0450]	[0.0168]	[0.125]	[90.40]
Race/Ethn. = Other	-0.0672**	-0.0266**	-0.0529	-52.71	-0.0697**	-0.0266**	-0.123	-88.02
	[0.0304]	[0.0121]	[0.0923]	[89.81]	[0.0304]	[0.0115]	[0.0924]	[62.61]
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year * quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Panel FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

(continued on next page)

Appendix Table B1 (continued)

Dependent Variable	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
	Total Expenditures				**Medicaid Expenditures**			
Corresponding Regression	Table 4, Column 3				Table 4, Column 7			
Stage of Two-Part Model	Stage 1 (Panel A)		Stage 2 (Panel B)		Stage 1 (Panel A)		Stage 2 (Panel B)	
	Coeffi-cients	Marginal Effect	Coeffi-cients	Marginal Effect	Coeffi-cients	Marginal Effect	Coeffi-cients	Marginal Effect
DM	0.0301	0.0119	-0.0835	-79.80	0.117***	0.0457***	-0.00590	-4.296
	[0.0227]	[0.00890]	[0.0565]	[52.16]	[0.0225]	[0.00887]	[0.0615]	[44.68]
Insured by Medicare	0.157***	0.0613***	0.319***	349.6***	-1.235***	-0.369***	-0.838***	-488.8***
	[0.0320]	[0.0123]	[0.0629]	[76.22]	[0.0319]	[0.00644]	[0.104]	[50.23]
Insured as Dual Eligible	0.224***	0.0865***	0.242***	261.1***	1.446***	0.510***	0.690***	636.1***
	[0.0275]	[0.0104]	[0.0476]	[55.91]	[0.0286]	[0.00737]	[0.0889]	[104.5]
Insured by Neither	-0.565***	-0.223***	-0.187***	-172.8***	-1.493***	-0.435***	0.0680	51.28
	[0.00857]	[0.00332]	[0.0644]	[54.95]	[0.0119]	[0.00292]	[0.229]	[178.8]
Diagnosis of Asthma	0.455***	0.172***	0.181***	189.3***	0.411***	0.162***	0.165***	126.7***
	[0.0126]	[0.00458]	[0.0425]	[47.40]	[0.0121]	[0.00483]	[0.0488]	[39.67]
Diagnosis of COPD	0.169***	0.0657***	0.327***	380.1***	0.143***	0.0561***	0.270***	224.5***
	[0.0443]	[0.0168]	[0.0717]	[97.37]	[0.0362]	[0.0143]	[0.0862]	[81.36]
Diagnosis of Diabetes	0.645***	0.231***	0.477***	574.0***	0.500***	0.197***	0.407***	349.8***
	[0.0245]	[0.00766]	[0.0411]	[60.96]	[0.0198]	[0.00776]	[0.0418]	[42.44]
Diagnosis of CHF	0.314***	0.120***	0.295***	331.7***	0.231***	0.0909***	0.171***	134.3***
	[0.0279]	[0.0102]	[0.0478]	[60.77]	[0.0233]	[0.00929]	[0.0532]	[44.65]
Diagnosis of CAD	0.210***	0.0812***	0.347***	403.7***	0.180***	0.0707***	0.304***	254.9***
	[0.0377]	[0.0142]	[0.0579]	[78.76]	[0.0302]	[0.0120]	[0.0666]	[64.02]
Male	-0.129***	-0.0511***	0.0822**	82.34**	-0.131***	-0.0505***	0.0372	27.25
	[0.00831]	[0.00330]	[0.0353]	[36.15]	[0.00841]	[0.00322]	[0.0389]	[28.84]
Age <10	-0.684***	-0.267***	0.231**	240.8**	-0.426***	-0.160***	0.128	95.54
	[0.0345]	[0.0131]	[0.0936]	[103.0]	[0.0307]	[0.0112]	[0.103]	[79.57]
Age 10-19	-0.745***	-0.290***	0.0628	63.42	-0.559***	-0.204***	-0.0421	-30.34
	[0.0337]	[0.0125]	[0.0785]	[80.77]	[0.0298]	[0.0101]	[0.0888]	[63.25]
Age 20-29	-0.539***	-0.212***	0.0132	13.21	-0.328***	-0.120***	0.149**	115.6*
	[0.0333]	[0.0127]	[0.0632]	[63.35]	[0.0294]	[0.0102]	[0.0750]	[62.02]
Age 30-39	-0.457***	-0.181***	0.0308	30.97	-0.258***	-0.0957***	0.0430	31.98
	[0.0337]	[0.0130]	[0.0668]	[68.03]	[0.0297]	[0.0105]	[0.0788]	[59.65]
Age 40-49	-0.245***	-0.0975***	0.172**	183.7**	-0.0588**	-0.0225**	0.158*	123.1*
	[0.0342]	[0.0136]	[0.0677]	[77.81]	[0.0298]	[0.0113]	[0.0843]	[70.57]
Age 50-59	0.0742**	0.0291**	0.162***	172.4***	0.167***	0.0652***	0.181**	142.7**
	[0.0360]	[0.0140]	[0.0584]	[66.49]	[0.0303]	[0.0120]	[0.0710]	[60.49]
Age 70-79	-0.0699*	-0.0277*	-0.140***	-130.7***	-0.125***	-0.0473***	-0.134**	-92.13**
	[0.0400]	[0.0159]	[0.0526]	[46.25]	[0.0314]	[0.0116]	[0.0669]	[43.46]
Age 80-89	0.116**	0.0452**	0.0256	25.72	0.0166	0.00639	0.0993	75.99
	[0.0492]	[0.0190]	[0.0583]	[59.22]	[0.0379]	[0.0147]	[0.0752]	[60.29]
Age ≥90	-0.0378	-0.0150	0.475	602.6	0.193*	0.0757*	0.669	693.8
	[0.132]	[0.0525]	[0.335]	[534.8]	[0.114]	[0.0453]	[0.562]	[800.4]
Education = No degree	-0.156***	-0.0617***	-0.0325	-32.04	-0.0192	-0.00739	0.0418	30.79
	[0.0158]	[0.00630]	[0.0337]	[32.97]	[0.0152]	[0.00582]	[0.0388]	[28.87]
Education = GED	-0.0614**	-0.0243**	-0.122**	-114.8***	0.0247	0.00955	-0.0842	-59.11
	[0.0278]	[0.0110]	[0.0489]	[43.46]	[0.0270]	[0.0105]	[0.0532]	[35.98]
Education = Bachelor's	0.0628	0.0246	-0.0211	-20.74	-0.108***	-0.0411***	-0.0157	-11.37
	[0.0429]	[0.0167]	[0.0891]	[86.59]	[0.0402]	[0.0150]	[0.123]	[88.21]
Education = Graduate	0.0528	0.0207	-0.101	-95.73	-0.0480	-0.0184	-0.110	-76.20
	[0.0710]	[0.0277]	[0.179]	[160.2]	[0.0700]	[0.0266]	[0.174]	[113.8]
Education = Other/Ukn	0.0245	0.00964	0.103	107.0	0.0220	0.00851	0.158*	124.3*
	[0.0379]	[0.0149]	[0.0668]	[73.13]	[0.0362]	[0.0140]	[0.0857]	[72.87]
Education = NA/Under-16	-0.0807***	-0.0318***	-1.108***	-1,102***	-0.0151	-0.00584	-0.927***	-686.1***
	[0.0213]	[0.00839]	[0.0689]	[75.36]	[0.0212]	[0.00819]	[0.0748]	[60.13]
Race/Ethn. = Black	-0.316***	-0.125***	-0.189***	-179.2***	-0.228***	-0.0864***	-0.147***	-103.5***
	[0.0117]	[0.00465]	[0.0374]	[34.35]	[0.0117]	[0.00435]	[0.0423]	[29.11]
Race/Ethn. = Hispanic	-0.221***	-0.0873***	-0.224***	-214.9***	-0.130***	-0.0500***	-0.198***	-141.0***
	[0.0119]	[0.00474]	[0.0512]	[47.84]	[0.0120]	[0.00458]	[0.0596]	[41.43]
Race/Ethn. = Asian/PI	-0.411***	-0.163***	-0.531***	-414.6***	-0.324***	-0.118***	-0.601***	-334.5***
	[0.0264]	[0.0102]	[0.0835]	[51.93]	[0.0262]	[0.00888]	[0.0857]	[37.65]
Race/Ethn. = Native Am.	-0.187***	-0.0743***	0.0114	11.33	-0.112**	-0.0424**	-0.0177	-12.80
	[0.0454]	[0.0181]	[0.105]	[105.4]	[0.0460]	[0.0171]	[0.127]	[90.99]
Race/Ethn. = Other	-0.0623**	-0.0247**	-0.00567	-5.607	-0.0683**	-0.0261**	-0.0839	-58.87
	[0.0307]	[0.0122]	[0.0865]	[85.38]	[0.0306]	[0.0116]	[0.0830]	[56.06]
State * year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Panel FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Quarter of year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Table presents GLM regression coefficients and marginal effects (calculated at the mean of the independent variables) for selected two-part models in **Table 4**. In addition to the variables shown, state, year-quarter, and panel fixed effects and a constant are included in the model but not reported. The left-out group is composed of white non-Hispanic 60 to 69-year-old females with a high-school education in panel 10 in 2005q1 who are in insured by Medicaid (only) and do not have a top-5 chronic disease. The data consists of all available quarterly observations for individuals in the MEPS, 1998-2007, who receive Medicaid in at least one quarter. Robust standard errors (in brackets) are based on exchangeable within-individual correlation structure. ***p<0.01, **p<0.05, *p<0.10

Appendix Table B2: Effect of Medicaid DM on quarterly health expenditures (all payers) using *strict* DM definition, by type of service

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Total medical expenditures (by service type)							
Dependent variable	Office-Based Medical Provider Visits	Prescribed Medicines	Emergency Department Visits	Inpatient Hospital Events	Outpatient Hospital Events	Home Health	Dental and Vision	Other Medical Expenses
Panel A: Marginal effect from stage 1 of two-part model								
Marginal effect	0.00143	0.0196**	-0.000924	-0.00220	0.000142	0.000965	0.00234	0.00306
	[0.00830]	[0.00959]	[0.00260]	[0.00134]	[0.00223]	[0.00131]	[0.00438]	[0.00216]
ATET, stage 1	0.00141	0.0174	-0.00153	-0.00465	0.000241	0.00247	0.00267	0.00497
Panel B: Marginal effect from stage 2 of two-part model								
Marginal effect	-11.47	2.69	1.05	-369.1	72.06	749.4	5.25	18.56
	[16.19]	[10.23]	[36.37]	[973.3]	[164.4]	[538.9]	[14.52]	[29.82]
ATET, stage 2	-15.56	5.32	1.09	-339.2	74.85	599.9	5.59	18.76
Panel C: Implied marginal effect in two-part model								
Marginal effect	-3.72	5.66	-0.37	-25.75	2.12	8.21	0.91	1.28
ATET	-8.39	10.00	-0.65	-56.99	5.14	24.78	1.22	2.94
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year * quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Panel FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
DM Definition	Strict	Strict	Strict	Strict	Strict	Strict	Strict	Strict
N of obs., stage 1	289,452	289,452	289,452	289,452	289,452	289,452	289,452	289,452
N of individuals, stage 1	40,419	40,419	40,419	40,419	40,419	40,419	40,419	40,419
N of obs., stage 2	106,647	104,811	15,929	8,592	11,809	7,032	27,989	11,482
N of individuals, stage 2	32,474	28,195	11,121	6,482	6,935	2,079	15,436	8,339

Notes: This table repeats the analysis in **Table 11** with the *strict* definition of DM. Panels A and B present marginal effects of DM on expenditures from two separate GLM regressions calculated as (i) the marginal effect (at the mean of the other independent variables) and (ii) the average treatment effect for the treated observations (ATET). The DM independent variable equals one if an observation is matched to a DM program in the individual's state/county based on their chronic disease(s) and insurance, and zero otherwise. In panel A, the regression estimates the effect of DM on $\Pr(y_{it} > 0 | DM_{it}, z_{it})$ using a Bernoulli distribution and a probit link function. In panel B, the regression estimates the effect of DM on $\mathrm{E}(y_{it} | y_{it} > 0, DM_{it}, z_{it})$ using a gamma distribution and a log link function. Robust standard errors (in brackets) are based on exchangeable within-individual correlation structure. In addition to fixed effects, these models include dummies for gender; age; highest education completed; race/ethnicity; diagnosis/reporting of asthma, COPD, diabetes, chronic heart failure, and/or coronary arty disease; and insurance coverage by Medicaid and/or Medicare in the quarter. The data consists of all available quarterly observations for individuals in the MEPS, 1998-2007, who receive Medicaid in at least one quarter. ***p<0.01, **p<0.05, *p<0.10.

Appendix Table B3: Effect of Medicaid DM on quarterly health expenditures paid by Medicaid using *strict* DM definition, by type of service

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Medical expenditures paid by Medicaid (by service type)							
Dependent variable	**Office-Based Medical Provider Visits**	**Prescribed Medicines**	**Emergency Department Visits**	**Inpatient Hospital Events**	**Outpatient Hospital Events**	**Home Health**	**Dental and Vision**	**Other Medical Expenses**
Panel A: Marginal effect from stage 1 of two-part model								
Marginal effect	0.0119*	0.0477***	-0.00009	-0.000781	0.000864	0.000944	0.00272	0.00177
	[0.00721]	[0.00839]	[0.00200]	[0.00110]	[0.00159]	[1.30e11]	[0.00318]	[0.00131]
ATET, stage 1	0.0142	0.0575	-0.000200	-0.00204	0.00195	0.00296	0.00402	0.00418
Panel B: Marginal effect from stage 2 of two-part model								
Marginal effect	5.67	4.04	7.73	616.1	-48.17	928.5*	13.86	25.14
	[13.13]	[10.48]	[25.91]	[675.2]	[66.76]	[495.0]	[13.34]	[31.79]
ATET, stage 2	8.27	7.73	9.97	849.6	-54.97	806.9	16.01	28.11
Panel C: Implied marginal effect in two-part model								
Marginal effect	3.86	10.54	0.22	4.79	-0.34	7.11	1.14	0.73
ATET	8.42	25.27	0.83	24.30	-1.67	27.93	2.33	3.01
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year * quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Panel FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
DM Definition	Strict	Strict	Strict	Strict	Strict	Strict	Strict	Strict
N of obs., stage 1	289,452	289,452	289,452	289,452	289,452	289,452	289,452	289,452
N of individuals, stage 1	40,419	40,419	40,419	40,419	40,419	40,419	40,419	40,419
N of obs., stage 2	79,491	73,674	12,193	7,036	8,613	4,911	20,748	6,929
N of individuals, stage 2	27,727	23,340	8,843	5,499	5,354	1566	12,252	5,400

Notes: This table repeats the analysis in **Table 12** with the *strict* definition of DM. Panels A and B present marginal effects of DM on expenditures from two separate GLM regressions calculated as (i) the marginal effect (at the mean of the other independent variables) and (ii) the average treatment effect for the treated observations (ATET). The DM independent variable equals one if an observation is matched to a DM program in the individual's state/county based on their chronic disease(s) and insurance, and zero otherwise. In panel A, the regression estimates the effect of DM on $\Pr(y_{it} > 0 | DM_{it}, z_{it})$ using a Bernoulli distribution and a probit link function. In panel B, the regression estimates the effect of DM on $\mathrm{E}(y_{it} | y_{it} > 0, DM_{it}, z_{it})$ using a gamma distribution and a log link function. Robust standard errors (in brackets) are based on exchangeable within-individual correlation structure. In addition to fixed effects, these models include dummies for gender; age; highest education completed; race/ethnicity; diagnosis/reporting of asthma, COPD, diabetes, chronic heart failure, and/or coronary arty disease; and insurance coverage by Medicaid and/or Medicare in the quarter. The data consists of all available quarterly observations for individuals in the MEPS, 1998-2007, who receive Medicaid in at least one quarter. ***p<0.01, **p<0.05, *p<0.10.

Appendix Table B4: Effect of Medicaid DM on quarterly health expenditures paid by Medicaid, by service type, with alternative samples

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Medical expenditures paid by Medicaid (by service type)							
Dependent variable	**Office-Based Medical Provider Visits**	**Prescribed Medicines**	**Emergency Department Visits**	**Inpatient Hospital Events**	**Outpatient Hospital Events**	**Home Health**	**Dental and Vision**	**Other Medical Expenses**
Panel A: Marginal effect from stage 1 of two-part model								
Marginal effect	0.00736	0.00771	0.000860	2.77e-05	-0.00134	0.00218	0.00328	0.00316
	[0.00799]	[0.00934]	[0.00246]	[0.00172]	[0.00276]	[0.00134]	[0.00473]	[0.00246]
ATET, stage 1	0.00681	0.00626	0.00140	4.51e-05	-0.00166	0.00524	0.00322	0.00395
Panel B: Marginal effect from stage 2 of two-part model								
Marginal effect	-21.69	4.53	5.86	-1,353.31	23.21	468.69	0.04	23.29
	[20.67]	[11.81]	[38.46]	[858.8]	[144.9]	[370.0]	[19.70]	[31.68]
ATET, stage 2	-21.29	6.15	4.94	-1,050.31	18.83	487.36	0.03	16.42
Panel C: Implied marginal effect in two-part model								
Marginal effect	-6.97	4.84	0.77	-31.59	-0.51	8.56	0.90	1.91
ATET	-10.07	6.66	1.23	-55.97	-0.32	27.00	0.65	2.31
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year * quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Panel FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Disease * insurance FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
DM Definition	Regular	Regular	Regular	Regular	Regular	Regular	Regular	Regular
N of obs., stage 1	479,376	479,376	479,376	479,376	479,376	479,376	479,376	479,376
N of individuals, stage 1	66,423	66,423	66,423	66,423	66,423	66,423	66,423	66,423
N of obs., stage 2	214,780	237,451	26,317	16,988	30,512	10,951	60,808	25,909
N of individuals, stage 2	56,269	52,092	18,695	12,500	16,435	3,709	28,785	18,482

Notes: This table repeats analysis in **Table 4** Column 6, but disaggregates medical expenditures by type of service (as in Table 11). The data consists of quarterly observations for individuals in the MEPS, 1998-2007, who (1) receive Medicaid in at least one quarter *and/or* (2) have one of the top-5 chronic diseases. Panels A and B present marginal effects of DM on expenditures from two separate GLM regressions calculated as (i) the marginal effect (at the mean of the other independent variables) and (ii) the average treatment effect for the treated observations (ATET). The DM independent variable equals one if an observation is matched to a DM program in the individual's state/county based on their chronic disease(s) and insurance, and zero otherwise. In panel A, the regression estimates the effect of DM on $\Pr(y_{it} > 0|DM_{it}, z_{it})$ using a Bernoulli distribution and a probit link function. In panel B, the regression estimates the effect of DM on $\mathrm{E}(y_{it}|y_{it} > 0, DM_{it}, z_{it})$ using a gamma distribution and a log link function. Robust standard errors (in brackets) are based on exchangeable within-individual correlation structure. In addition to fixed effects, these models include dummies for gender; age; highest education completed; race/ethnicity; diagnosis/reporting of asthma, COPD, diabetes, chronic heart failure, and/or coronary arty disease; and insurance coverage by Medicaid and/or Medicare in the quarter. ***p<0.01, **p<0.05, *p<0.10.

Appendix C: Alternative model specification – the tobit model

This chapter provides results for both Chapter 2 and Chapter 3 using an alternative modeling approach: the tobit model. Whereas the results in the main text of this dissertation rely on the well-known "two-part model" that is commonly recommended in health economics, this appendix reproduces the main results under the assumption that health care expenditures can be modeled using a latent variable, y^*, that depends on the model covariates:

$$y_{it}^* = x_{it}\beta + \varepsilon_{it} \quad \text{C-1}$$

where $\varepsilon_{it}|x_{it} \sim N(0, \sigma^2)$ across observations and the vector of covariates, x_{it}, are the same as in Chapter 2 and Chapter 3. Observed health expenditures, y_{it}, is censored at \$0 as follows:

$$\begin{aligned} y_{it} &= \max(0, y_{it}^*) \\ &= \max(0, x_{it}\beta + \varepsilon_{it}) \end{aligned} \quad \text{C-2}$$

This formulation dates back to Tobin (1958) and can be solved with maximum likelihood. Given the skewness of total expenditures in both chapters, I also run models on the natural logarithm of expenditures, with the dependent variable censored at $\log(\$0.01)$. To account for potential heteroskedasticity, I calculate robust standard errors, clustered at the individual level.

Tobit model results for Chapter 2

The results of the tobit model estimation are presented in Appendix Table C1. Here, the data sample and covariates are comparable to the two-part models presented in Table 4 (columns 1, 3, 5, and 7). The coefficient indicates a positive, but statistically insignificant, effect of DM on the latent variable, y_{it}^*, of \$123.93 (SE 93.05). The latent variable refers to a theoretical "optimal" level of health expenditures, which is censored at zero because patients who have negative demand for health care cannot actually have negative expenditure. With the natural logarithm of expenditures as the dependent variable, the results also indicate a positive effect of DM on health spending. Results are similar with state-year interaction effects added as control variables.

As can been seen in equation C-1, the tobit model does *not* allow for different mechanisms to drive (1) the decision to consume health care expenditures and (2) the level of

expenditures conditional on a positive utilization. That is, there is not a separate "stage 1" and "stage 2" effect. DM here only has an effect on the probability of expenditures because the effect of DM may shift some individuals' latent demand for health care above/below the $0 cutoff. In row (c), we see an 80 basis point increase in the probability of positive expenditures when the individual is matched to a DM program. By definition, this effect must be in the same direction as the effect of DM on the latent variable, y_{it}^{*}, and the observed variable, y_{it}.

In Appendix Table C2, I demonstrate that these differences between the two-part model (e.g., Table 4) and the tobit model could result solely from different distributional assumptions on the error term. That is, the fact that DM has a positive effect on expenditures in the tobit model (where the two-part model finds a negative effect) appears to be the result of the normality assumption in the tobit model, not the differences between how the models handle "censoring" at zero. In Appendix Table C2, I demonstrate how the results for the two-part model differ if, instead of a gamma distribution, I assume either a Poisson distribution or a Gaussian (normal) distribution. This table shows that the effect of DM is close to zero and statistically insignificant under the Poisson distribution assumption. The third column indicates that the effect of DM is positive – and very similar to the tobit model – under the assumption of normality. The similarity of the results between the tobit model and the two-part model under normality appear to reconcile the difference between the models. In the text, I used a Park test with my data and argued that the gamma distribution best fits total expenditure data in the MEPS for the two-part model (following Manning and Mullahy 2001). It has been shown elsewhere that the tobit estimator, $\hat{\beta}$, is inconsistent if the error term is nonnormal (e.g., Cameron and Trivedi 2005, p. 538; Wooldridge 2002, p. 533), a finding that casts doubt onto the results of the tobit model in Appendix Table C1.

Tobit model results for Chapter 3

For Chapter 3, the two-part model estimates relied on individual fixed effects to control for all idiosyncratic, time-invariant aspects particular to the individual, including age, gender, and the presence of chronic conditions and co-morbidities. A parametric tobit model with fixed

effects suffers from the classic incidental parameters problem as long as the number of periods is fixed. (Chamberlain 1984, p. 1256; Lancaster 2000; Arellano and Honoré 2001, p. 3270; Wooldridge 2002, p. 484)[76] As a substitute, I use a random effect tobit model (as implemented in Stata 11.2). This requires the fairly strong assumption that the random effect has a normal distribution.[77]

Results are presented in Appendix Table C3. The regression coefficient for the DM variable (see equation 7, p. 85) for the logarithm of dollars is -0.759 and is statistically significant at the 99 percent level. That is, the model indicates that DM interventions lower medical expenditures. In column 2, we see that the effect is larger for high-intensity interventions than it is for the moderate-intensity interventions. Regressions with total medical expenditures (i.e., not log( ) transformed) indicate that the high-risk interventions decrease spending $196 ($y_{it}^{*}$) and moderate-intensity decrease spending $41, or about $76 on average. These findings are qualitatively consistent with the results presented in section 3.6.1.

[76] For a nonparametric estimator for tobit regression with fixed effects, see Honore (1992).
[77] I do not expect to find a large discrepancy between the two-part model and the tobit model because the error term is assumed to be normally distributed in both cases.

Appendix Table C1: Effect of Medicaid DM on quarterly health expenditures using tobit model, MEPS 1998-2007

Dependent variable	(1)	(2) Total medical expenditures	(3)	(4) log(Total medical expend.)
(a) DM_{it} regression coefficient	123.93	92.90	0.5769***	0.5480***
	[93.05]	[96.23]	[0.1268]	[0.13068]
(b) y_{it}^*				
Marg. effect on y_{it}^*	45.80	34.21	0.3750***	0.3561***
	[34.72]	[35.68]	[0.0840]	[.0868]
ATET on y_{it}^*	60.47	45.42	0.4468	0.4249
(c) $\Pr(y_{it}^* > 0)$				
Marg. effect on $\Pr(y_{it}^* > 0)$	0.00799	0.005989	0.0271***	.02592***
	[0.00602]	[0.0062]	[0.0059]	[0.00614]
ATET on $\Pr(y_{it}^* > 0)$	0.00799	[0.005992]	0.0250	0.01960
(d) $\mathrm{E}(y_{it}\|y_{it}^* > 0)$				
Marg. effect on $\mathrm{E}(y_{it}\|y_{it}^* > 0)$	36.82	27.54	0.2629***	0.2497***
	[27.81]	[28.65]	[0.0589]	[0.0609]
ATET on $\mathrm{E}(y_{it}\|y_{it}^* > 0)$	45.146	33.90	0.3332	0.3172
State FE	Yes	Yes	Yes	Yes
Panel FE	Yes	Yes	Yes	Yes
Year-quarter FE	Yes		Yes	
Year FE		Yes		Yes
Quarter of year FE		Yes		Yes
State-Year FE		Yes		Yes
N of obs.	289,452	289,452	289,452	289,452
N of individuals	40,419	40,419	40,419	40,419

Notes: Tobit model regression coefficients, marginal effects (at the mean), and ATET for the effect of DM on total medical expenditures. The model accounts for left-censoring at $0 (columns 1-2) and log($0.01) (columns 3-4). Covariates are comparable to Table 4 (columns 1, 3, 5, and 7). In addition to fixed effects, these models include dummies for gender; age; highest education completed; race/ethnicity; diagnosis/reporting of asthma, COPD, diabetes, chronic heart failure, and/or coronary arty disease; and insurance coverage by Medicaid and/or Medicare in the quarter. The data consists of all available quarterly observations for individuals in the MEPS, 1998-2007, who receive Medicaid in at least one quarter. Robust standard errors, clustered at the individual level (in brackets) ***p<0.01, **p<0.05, *p<0.10.

Appendix Table C2: Effect of Medicaid DM on quarterly health expenditures, alterative two-part model distribution assumptions

	(1)	(2)	(3)
Dependent variable	**Total medical expenditures**		
Panel A: Marginal effect from stage 1 of two-part model			
Marginal effect	0.0148*	0.0148*	0.0148*
	[0.00875]	[0.00875]	[0.00875]
ATET, stage 1	0.0111	0.0111	0.0111
Panel B: Marginal effect from stage 2 of two-part model			
Marginal effect	-86.69	0.48	45.46
	[60.02]	61.81	[71.99]
ATET, stage 2	-132.75	0.52	79.15
Panel C: Implied marginal effect in two-part model			
Marginal effect	-33.46	15.13	35.36
ATET	-97.29	12.77	75.30
State FE	Yes	Yes	Yes
Year * quarter FE	Yes	Yes	Yes
Panel FE	Yes	Yes	Yes
GLM link function, stage 1	Probit	Probit	Probit
GLM error distribution, stage 1	Bernoulli	Bernoulli	Bernoulli
GLM link function, stage 2	Log	Log	Log
GLM error distribution, stage 2	**Gamma**	**Poisson**	**Gaussian**
N of obs., stage 1	289,452	289,452	289,452
N of individuals, stage 1	40,419	40,419	40,419
N of obs., stage 2	155,901	155,901	155,901
N of individuals, stage 2	36,618	36,618	36,618

Notes: Table construction, GLM models, covariates, and data sample are identical to Table 4 (column 1), except that the distribution assumption for stage-2 of the model is substituted with a Poisson or Gaussian distribution in columns 2 and 3, respectively. Robust standard errors (in brackets) are based on exchangeable within-individual correlation structure ***p<0.01, **p<0.05, *p<0.10.

Appendix Table C3: Effect of Medicaid DM on monthly health expenditures in the GEC program, using a random effect tobit model

	(1)	(2)	(3)	(4)
Dependent variable	**log(Total medical expend.)**		**Total medical expenditures**	
(a) Regression coefficient				
DM	-0.759***		-125.9***	
	[0.0300]		[20.91]	
DM Mod		-0.712***		-68.07***
		[0.0306]		[21.24]
DM High		-0.927***		-331.3***
		[0.0363]		[25.03]
(b) ATET on y_{it}^*				
DM	-0.674		-76.03	
DM Mod		-0.634		-41.49
DM High		-0.817		-196.3
(c) ATET on $\Pr(y_{it}^* > 0)$				
DM	-0.0243		-0.0141	
DM Mod		-0.0223		-0.0076
DM High		-0.0309		-0.0373
(d) ATET on $\mathrm{E}(y_{it} \mid y_{it}^* > 0)$				
DM	-0.530		-53.49	
DM Mod		-0.501		-29.17
DM High		-0.638		-138.4
Individual random effects	Yes	Yes	Yes	Yes
Month dummy variables	Yes	Yes	Yes	Yes
N of obs.	587,070	587,070	587,070	587,070
N of uncensored obs.	453,490	453,490	453,490	453,490
N of individuals	17,349	17,349	17,349	17,349

Notes: Tobit model regression coefficients and ATET for the effect of DM on Medicaid expenditures. The model accounts for left-censoring at log($0.01) (columns 1-2) and $0 (columns 3-4). All regressions include month-of-year dummy variables and individual random effects (normal distribution). Standard errors (in brackets) ***p<0.01, **p<0.05, *p<0.10.

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