Three Essays in Preventive Health Economics

by

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Dedication

For Cristin and Sam

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Abstract

This dissertation examines the oftentimes complicated nature of preventive care utilization decisions to help inform preventive health policy. The first paper examines how patient experiences with two seemingly disparate types of preventive care—mammography and medication adherence—can significantly affect each other. Using Marketscan Medicaid and commercial claims data and a difference-in-difference approach, I show that a false-positive mammogram leads to improved cholesterol medication adherence for the Medicaid insured population, with suggestive evidence showing that the improvement may be due to increased interaction with the health care system for a population with access problems. However, I find reduced adherence for the commercially insured, possibly resulting from reduced trust in the health care system following the negative experience of a false-positive. The second paper provides evidence that a false-positive mammogram can lead to significant increases in the probability of initiating depression or anxiety medication utilization compared to women who have a true-negative mammogram for both the Medicaid and commercially insured populations. In terms of magnitude the effect is 4 to 6 times smaller than for women diagnosed with breast cancer (true-positives). I also find several factors that appear to increase the risk of initiating depression or anxiety medication for women experiencing a false-positive mammogram—the first experienced false-positive for women with Medicaid; and invasiveness of the test and greater time to resolution for the commercially insured. The final paper analyzes how anticipated unemployment and uninsurance affect preventive health utilization using MEPS data. To find

more exogenous sources of variation in unemployment and uninsurance, my primary analyses restrict analysis to individuals in the 75th percentile or greater of predicted unemployment or uninsurance and separate unemployment into layoffs and non-layoffs. Across both methods, I find evidence of significant stocking up for a variety of preventive care services in anticipation of future unemployment but evidence of significant delaying for individuals who anticipate becoming uninsured. Particularly strong delays for cancer screenings, suggest the delays may result from lower expected net benefits of screening for people who anticipate becoming uninsured and therefore facing high out-of-pocket costs for any treatments.

Chapter 1.
Introduction

There is strong evidence that preventive health care including vaccinations, screenings, and medication adherence can help improve health (Masiosek et al., 2006; Cutler and Everett, 2010) and in the case of certain vaccinations and medication adherence can even potentially lead to health care cost savings (Cohen, Neumann, and Weinstein, 2008; Roebuck et al., 2011). Despite this evidence, many preventive services are under-utilized (McGlynn et al., 2003). For example, nearly 1/3 of eligible women have not had a mammogram within the past two years (Centers for Disease Control, 2015); and nearly 40 percent of individuals are not adequately adherent to cardiovascular and antidiabetic medications (Cramer et al., 2007).

In an effort to increase the utilization rates of select preventive services, the Affordable Care Act (ACA) includes provisions that require all health insurance plans to cover 15 preventive services without cost sharing for all adults as well as an additional 22 preventive services for women (U.S. Department of Health and Human Services, 2015). However, by just focusing on patient cost-sharing, the ACA may miss important nuances about individual level preventive care decision-making that may be important to ensuring optimal levels of preventive care utilization. This dissertation examines the oftentimes complicated nature of preventive care utilization decisions to help better inform preventive health care policy.

One type of important preventive care not explicitly targeted by the ACA is chronic disease medication adherence. Despite numerous studies on the importance of medication adherence and factors that may affect it, none have examined how an individual's experience with seemingly unrelated types of preventive care may have important effects on medication adherence. In the second chapter, I demonstrate that the interaction of disparate types of preventive care is important to understanding the complicated issue of medication adherence. I show that the experience of a false-positive mammogram can have significant spillover effects onto medication adherence. On one hand following a false-positive, the increased interaction with the health care system during the follow-up visits may improve adherence, while on the other hand the negative experience may lower an individual's trust in the health care system thereby lowering adherence. Using Marketscan Medicaid and commercial claims data and a difference-in-difference approach, this paper shows that a false-positive mammogram leads to improved cholesterol medication adherence for the Medicaid insured population but reduced adherence for the commercially insured. I then provide suggestive evidence that the improved adherence within the Medicaid population is likely due to this population with poor access having increased interaction with the health care system following a false-positive.

The third chapter examines the related topic of how a false-positive mammogram can lead to significant increases in the probability of initiating depression or anxiety medication utilization. Again using Marketscan Medicaid and commercial claims data, I compare rates of depression or anxiety medication initiation for women who undergo a screening mammogram and receive either a false-positive, a true-positive, or a true-negative result. I find significant increases in depression or anxiety medication initiation for women who experience a false-positive mammogram compared to women who have a negative result for both those with

Medicaid and with commercial health insurance. I also find a significant increase for women who have a true-positive mammogram result, with the magnitude nearly 4 to 6 times larger than that for women who experience a false-positive. I also find several factors that appear to increase the risk of initiating depression or anxiety medication for women experience a false-positive mammogram—the first experienced false-positive for women with Medicaid insurance; as well as invasiveness of the test and greater time to resolution for the commercially insured. Finally, I find a significant but modestly sized increase in the number of fills and amount spent on depression and anxiety medication for women experiencing a false-positive mammogram who have been previously taking those medications.

The fourth chapter analyzes how anticipated future unemployment and uninsurance affects preventive health utilization. Using 2000-2012 Medical Expenditure Panel Survey (MEPS) data, I estimate how future unemployment and uninsurance affects current preventive care utilization among a population that is both employed and insured. With concerns about the endogeneity of utilization and unemployment or uninsurance, I implement two methods to attempt to find more exogenous sources of variation in unemployment and uninsurance—restricting the analysis to individuals in the 75^{th} percentile or greater of predicted unemployment or uninsurance and separating unemployment into layoffs and non-layoffs. Across both methods, I find evidence of significant stocking up for a variety of types of preventive care services in anticipation of future unemployment but not uninsurance. Conversely and with more consistent evidence across methods, I find evidence of significant delaying for individuals who anticipate becoming uninsured. With the results particularly strong for cancer screenings, I hypothesize that preventive care, especially screenings, may be more limited in terms of net benefits to individuals who anticipate becoming uninsured because they are more likely to face high out-of-

pocket costs for any treatments. This suggests that preventive health care policy during economic downturns may be best targeted at individuals likely to become uninsured, in particular trying to ensure that these individuals continue to have access to affordable coverage for both preventive as well as curative care.

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Chapter 2.
Heterogeneous spillover effects of false-positive mammograms on medication adherence

2.1 Introduction and Background

Medication adherence is an important component of public health efforts to improve health outcomes for individuals with chronic diseases (Osterberg et al., 2005; Cutler and Everett, 2010). In particular, for conditions such as dyslipidemia and diabetes, improved medication adherence has been shown to prevent serious health complications and lower overall medical costs (Sokol et al., 2005; Roebuck et al., 2011). Despite these benefits, medication adherence for dyslipidemia and diabetes remains suboptimal, with patients often missing or delaying their prescriptions (Cramer, 2004; Ellis et al., 2004).

As such a central issue in pharmacoeconomics and health policy, there have been thousands of studies on the topic of anti-hyperlipidemia (i.e. cholesterol) and anti-diabetic (i.e. diabetes) medication adherence. As summarized by three systematic reviews (Balkrishnan, 2005; Odegard and Capoccia, 2007; Pedan et al., 2007; and Williams, Manias, and Walker (2008)), the existing studies have focused on three main factors that may affect medication adherence: individual-level characteristics (e.g. demographics, expected health benefits, health literacy, health beliefs or knowledge, and cognitive or psychological ability to follow a medication regimen), medication-level characteristics (e.g. cost, side effects, and complexity of the

regimen), and provider-level characteristics (e.g. relationship with the patient, adequate follow-up resources, and adequate patient support). However, an important area that has not received attention is how experience with other unrelated types of preventive care may have significant spillover effects onto medication adherence.

The emerging importance of these types of spillover effects has been described by Cutler and Glaeser (2005) in their study showing typical observables explain only a small portion of the large variation in preventive health behaviors and care utilization such mammography and medication utilization. The authors find that genetics are one important explanation but that "randomly encountered situation differences" have potentially the largest effect. Despite citing it as a "high priority for future research", the situational factors have yet to receive much attention in the literature.

This study provides the first evidence of how a seemingly unrelated type of care, namely mammography, can have significant spillover effects on cholesterol and diabetes medication. While several studies have examined medication adherence and other types of preventive behaviors such as mammography utilization, these studies have only focused on the positive correlation between preventive care utilization and medication adherence to demonstrate the "healthy adherer problem" (Brookhart et al., 2007; Dormuth et al., 2009). This problem highlights the selection problem—women who are more likely to get a mammogram may also be more likely to adhere to medication—that makes it difficult to make causal inferences about how mammography might affect medication adherence.

I ameliorate this problem empirically, by exploiting the random nature of a false-positive mammogram. I argue that conditional on receiving a mammogram, a false-positive is plausibly

exogenous to patients' other preventive health care use because it is generally a function of technical error or radiologist judgment (Elmore et al., 2001). I choose to focus on false-positive mammograms because they are common—over 10 years of screening an estimated 1/3 to 1/2 of all women will experience a false-positive (Elmore et al., 1998; Hubbard et al., 2011)—and, as the emerging literature has shown, appear to have potentially significant effects on health beliefs and behavior (Bond et al., 2013; Brewer et al., 2007; Brett et al., 2005), making them a good candidate for having spillover effects onto other types of care such as medication adherence.

To better understand the possible factors affecting medication adherence, in the theoretical framework section, I use a model of adherence developed by Egan and Philipson (2014) to highlight three important factors from the standard theory of medication adherence—price, expected effectiveness of the medication, and immediateness of the feedback regarding the effectiveness of the medication. I then explain how a false-positive may affect the first two, leading to spillover effects on medication adherence. Specifically, a false-positive requires follow-up tests that may effectively lower the non-monetary costs of adherence by providing patients with additional opportunities to be reminded and encouraged to take their medications or even possibly to have a refill written during these additional visits. This would lead to improved medication adherence. Conversely, the negative experience surrounding a false-positive may lower the patient's trust in medical care, including the effectiveness of medication, thereby lowering adherence.

Empirically, using longitudinal data on patients before and after screening mammograms, I estimate a difference-in-difference model that compares changes in medication adherence before and after mammograms between women who receive a false-positive (treatment group) and women who receive a true-negative (control group) using a validated claims-based

algorithm. The estimates show that a false-positive mammogram significantly reduces the number of days with no cholesterol medication (improves adherence) for the Medicaid insured but significantly increases the number of days with no cholesterol medication (lowers adherence) for the commercially insured. I find no statistically significant effect on diabetes medication adherence. Auxiliary analyses suggest that increased interaction with the health care system, as a result of the follow-up tests, may help to explain the cholesterol medication adherence improvement in the Medicaid population.

The significance of the connection between these two seemingly unrelated types of care is that future health policy may need to think more critically about how experience with one type of care may have important spillover effects onto other types of care. More specifically, this suggests preventive health care policy as well as communication between clinicians and patients may need to account for varying experiences with different types of preventive care. For example, women experiencing a false-positive may need increased support to not only deal with the anxiety surrounding a false-positive but to also ensure that they continue to utilize all necessary preventive care. In addition, the results showing that increased contact with the health care system, even for a seemingly unrelated type of care, improves mediation adherence for the Medicaid population suggests an important role for physicians or other health care providers to help improve preventive health behaviors even when that may not be the specific reason for the medical visit. Especially for patients who may otherwise have difficulty accessing the health care system, policies to encourage physicians to ask all patients about their medication adherence, remind them to continue to take their medication, or to write a refill prescription may be important.

This study makes contributions to two sets of literature. First, this study adds to the large literature on medication adherence. While most studies focus on how individual characteristics or drug-specific attributes, including prices or side effects, affect medication adherence, this study highlights that a more global perspective of an individual's health experience may be necessary. I show there may be important spillover effects from experiences with different types of preventive care, and also demonstrate that these spillover effects may vary by insurance status. For example, a false-positive mammogram may improve medication adherence for the Medicaid population as it increases contact with the medical care system, which they may have had previous difficulty navigating, while for the privately insured population the negative effects may feature more prominently as they already have adequate access to the health care system.

Second, this study makes an important contribution to the growing literature on the effects of false-positive mammograms. Although there have been many studies, including three large meta-analyses, to suggest that anxiety may increase[1] following a false-positive and may even persist for up to 3 years and mixed results on whether women return for subsequent screening (Bond et al., 2013; Brewer et al., 2007; Brett et al., 2005)[2], this is the first study to show that the effects may extend to other types of preventive care such as medication adherence highlighting the complex effects of a false-positive mammogram result. This has important health policy implications suggesting the need to find the best ways to help women manage care following a false-positive mammogram.

[1] In the following chapter, I provide the first evidence that a false-positive mammogram has an effect on anxiety rising to the level of clinical significance by showing a significant increase in depression or anxiety medication initiation.

[2] Most of the existing studies on false-positives mammograms tend to be somewhat limited by sample size, length of follow-up, or generalizability to the overall population.

2.2 Theoretical Framework

In this section I provide a theoretical framework for how a false-positive mammogram can affect medication adherence by relying on a model described by Egan and Philipson (2014). The model builds on the Grossman health capital model (1972) and several existing models of medication switching (Crawford and Shum, 2005; Ching, 2010; and Dickstein, 2014) to provide theoretical predictions about why individuals may switch cholesterol medications, although it could be applied to any type of medication.

In the Egan and Philipson model, the medication adherence decision can be thought of as an iterative process. An individual decides to initiate medication use, then, based upon her experience taking the medication, she decides whether to continue taking the medication in full or in part using a Bayesian learning process. Similar to the Grossman model, an individual's utility is a function of health and other consumption, where health may or may not be improved by utilization of a prescription drug. Within each period t, an individual's utility is a function of the net health benefits of the medication (h) and the price of the medication (p), with the parameter γ showing the health consumption tradeoff. [3] The net health benefits include positive health benefits of the medication less any side effects, while the price includes both monetary and non-monetary costs of medication utilization.[4] Non-monetary costs may include time costs, effort, and, importantly for this study, access to care difficulties. This leads to the following indirect utility function :

$$U(h_t, p) = h_t - \gamma p$$

[3] Gamma can be thought of as demonstrating the tradeoff between health spending and outside consumption (i.e. any dollar spent on health production could have been spent on consumption).
[4] For empirical examples of the effect of monetary and non-monetary costs on medication adherence, see Balkrishnan, 1998; Goldman et al., 2007; Jin et al., 2008.

While an individual does not initially know the true effectiveness of the medication, she does have an initial prior about what she believes to be her individual net health benefits from taking the medication. In each ensuing period, depending on her experience, an individual updates her belief about the personal effectiveness of the medication using a Bayesian learning process[5]. Over time, as an individual learns the true individual-level effectiveness of the medication she weights her experience more heavily than her initial prior. Ultimately, an individual decides to continue to adhere to a medication if the expected net health benefits outweigh the total costs. Applying the model to partial adherence, an individual chooses the level of adherence that provides the highest expected utility weighing both the expected net health benefits and costs for each level of adherence from none to partial to full.

From the model, the authors provide six conclusions about how different factors may affect medication adherence.

1) As would be expected from standard economic theory, the price of the medication, including both monetary and non-monetary costs negatively affects adherence.
2) The higher the expected effectiveness of the medication, the higher the adherence.
3) The more immediate the feedback from taking the medication, the more quickly an individual determines the true effectiveness of the medication for her. For example, if non-adherence quickly leads to adverse health consequences, an individual will more quickly choose a higher adherence level. Conversely, if the medication is not appropriate for the individual and serious side effects appear quickly an individual is more likely to non-adhere.

[5] The model mentions the possibility of suboptimal non-adherence, i.e. some individuals stop treatment even though they would benefit from it. But the study provides little discussion about this process or what might drive this under-adherence. The study actually focuses more on over-adherence (i.e. those who should stop taking the medication but do not).

4) The higher the quality of the outside treatment option, the lower the adherence to the medication. For example, if a new, more effective treatment is developed then an individual will become more non-adherent or stop taking the medication entirely.
5) Similarly, the less expensive the outside treatment option, the lower the adherence to the medication. The negative cross-price elasticity is not surprising given that the outside treatment option is a substitute for the medication.

For this paper I focus on the first three conclusions as the ones that may potentially be affected by the experience of a false-positive mammogram. More generally, the theoretical framework in this paper shifts strongly from the Egan and Philipson model but relaxing some of the assumptions about optimality since most of the effects of a false-positive I discuss are behavioral in nature and therefore tend to involve reactions to the experience that would not be predicted under the usual assumptions about optimality in a health economic framework. In the following section I describe how a false-positive mammogram fits into this framework and provide some predictions about the effect of a false-positive mammogram on medication adherence.

Spillover effect of a false-positive mammogram on medication adherence

Because most existing studies have focused on medication switching they have exclusively focused on how experience with the specific medication itself affects continued use[6] of this medication. But I demonstrate how the experience with a seemingly unrelated type of care, namely a false-positive mammogram, may actually have a significant spillover effect on medication adherence. I use the specific case of a false-positive mammogram for several reasons.

[6] Egan and Philipson (2014) talk a little about the issue of spillover effects, noting that comorbidities may raise the signal noise as individuals take multiple drugs and that taking multiple drugs may be more taxing than taking a single drug raising the effective price of taking all of the drugs. However, they do not discuss non-medication types of care nor do they discuss how a change in the experience with an alternative type of care may change the adherence decision.

First, multiple studies have shown that a false-positive mammogram can have significant effects on health beliefs and behaviors (Bond et al., 2013; Brewer et al., 2007; Brett et al., 2005), the types of factors described in the theoretical framework that may affect medication adherence. Second, the false-positive result is plausibly exogenous to medication adherence in the sense that that conditional on receipt of a mammogram, whether the result is a true-negative or a false-positive is unrelated to medication adherence. Third, false-positive mammograms are relatively common (Elmore et al., 1998; Hubbard et al., 2011) and have become increasingly important part of the health policy discussion surrounding preventive care (Biller-Andorno and Juni, 2014; Welch and Passow, 2014). However, one difficulty with predicting the effect of a false-positive mammogram on medication adherence is that from theoretical perspective it can have both a positive and a negative effect.

The main positive effect on adherence (reducing the number of days with no drugs) that I examine in this study is the reduction of the non-monetary costs (included in p) of medication adherence[7]. This comes about as a result of the increased interaction with the medical care system during the follow-up visits to rule out breast cancer. The increased interaction reduces the non-monetary costs both by allowing the patient additional opportunities to have a physician write a refill prescription but more importantly by increasing interaction with health care professionals who can remind and encourage the individual to continue taking her medications. This is similar to the evidence showing that individuals increase medication adherence surrounding a physician visit (see Feldman et al., 2007; Brookhart et al., 2007). Two important corollaries to this anticipated effect is that it is likely concentrated in the population who begin

[7] One other possible positive effect is that a false-positive mammogram could put an individual above her deductible and therefore reduce the monetary costs of adherence. I do not focus on this effect in this paper because most of the positive effects I find are in the Medicaid population for which there is no deductible, although this issue could be examined in more depth in subsequent work.

with high non-monetary costs of medication adherence (i.e. those with health care access problems such as the Medicaid insured) and the effect is expected to be higher for those with more follow-up visits to rule out breast cancer.

A second possible effect of a false-positive is the potential negative effect on adherence that may result from a false-positive lowering an individual's belief in the overall treatment effectiveness of medical care. In effect, a false-positive may be an example of the medical care system providing the patient with a negative experience by initially providing an incorrect diagnosis suggesting the fallibility of medical care. Although the false-positive test result is ultimately resolved, a lot of evidence suggests that the negative affect surrounding the event may continue (Bond et al., 2013; Brewer et al., 2007; Brett et al., 2005). Therefore the effect of a false-positive mammogram might be to lower the prior of the effectiveness for all treatments generally, including medication adherence.[8]

This negative effect represents a modification of the model described in Egan and Philipson (2014) and requires a bit of a behavioral economics explanation because the individual is not necessarily receiving precise information that treatments are lower quality. In fact, from a traditional economic perspective a false-positive provides no additional information compared to a true-negative (i.e. the individual does not have breast cancer). However, the experience itself may be very different, including making the fallibility of medical care more salient (Kahneman, 2003; Frank, 2004). In addition, the effect may be the opposite of the effect of optimism on decision making described by Brunnermeier and Parker (2005). They describe how individuals

[8] Although we might expect the effect to be largest for mammograms themselves, a key point of this study is that it is important to think about some of the possible spillover effects of an experience such as a false-positive mammogram on other types care such as medication adherence. Rather than think about each treatment in a vacuum, I suggest thinking about them as interconnected experience with the medical care system that can potentially affect decisions about all types of care.

may have overly optimistic beliefs about the future that lead to suboptimal behaviors but that the optimism may increase current utility. Similarly, overly pessimistic beliefs may help individuals avoid future unpleasant medical interactions. Here overly pessimistic beliefs about treatment may lead to suboptimal adherence behavior. Finally, the negative effect through a lower prior of treatment effectiveness could be a result of an individual feeling less control over her care similar to the effect described in Sloan et al. (2009). I do not distinguish between the different possible pathways, instead focusing only on whether or not the negative effect appears to exist empirically.

A final possible effect is that a false-positive mammogram may act as a "wake-up call" for an individual to improve her health thereby leading to improved medication adherence. This involves a change in the utility function or more specifically a change in γ (i.e. how an individual values health). However, this does not follow the Bayesian learning process described by Egan and Philipson (2014), but again involves a more behavioral economics explanation—for example, the importance of health may become more salient following a false-positive mammogram (Kahneman, 2003; Frank, 2004). However, I will not focus on this effect, primarily, because it cannot be clearly distinguished empirically. There are no obvious factors that would be associated with a particular group being more likely to experience a false-positive as a "wake-up call" unlike the factors that I describe above for why I expect the Medicaid population to be more likely to experience the improved access effect of a false-positive mammogram. Ultimately, this effect may operate in the background and be a part of the overall effect. While I am able to tease out the effect that is due to increased interaction with the health care system using information on the number of follow-up visits, empirically the "wake-up call" effect is likely to attenuate the negative effect of reduced trust in the health care system.

An important final note is that the Egan and Philipson (2014) model focuses exclusively on medication adherence as the treatment. However, this model could easily be extended to other types of repeated preventive care such as mammography screening or annual flu shot receipt. While the main dependent variables of interest in this study are still related to medication adherence, an important point of this study is to understand how experience with different types of preventive care may be intertwined. Due to data limitations this study focuses on the spillover effects of a false-positive on medication adherence but future work will look at how the experience of a false-positive may affect future mammogram screening.[9]

2.3 Hypotheses

To help motivate the empirical analyses, the theoretical framework leads to several testable hypotheses. The first set of hypotheses concern the relative effects of a false-positive mammogram on diabetes and cholesterol medication adherence, while the second set concern the relative effect for the Medicaid insured compared to the commercially insured.

Hypothesis 1a—the pre-mammogram medication adherence will be higher for diabetes medication than for cholesterol medication.

This is because as described by Egan and Philipson (2014), the more immediate the benefit of a medication is, the higher the adherence. Because an individual is likely to see adverse health effects begin to develop more quickly when not adhering to diabetes medication than cholesterol medication, the benefits of the diabetes medication are likely to be clearer leading to higher baseline medication adherence.

[9] Because the data are limited to 5 years of Medicaid claims and 2 years of commercial claims it is difficult to perform a difference-in-difference analysis of mammogram screening. I would likely need 8-10 years' worth of claims in order to fully estimate both the pre- and post-period patterns of mammogram screening.

Hypothesis 1b— the effect of a false-positive will be smaller in magnitude for diabetes medication than for cholesterol medications.

Relatedly, there is unlikely to be a significant effect of a false-positive mammogram on diabetes medication adherence. The negative effect is likely small because if an individual decides to stop taking diabetes medication, the negative health consequences are likely to appear more quickly than for cholesterol medication. The positive effect is likely small for a similar reason—as mentioned non-adherence in the pre-period is less likely so improvement is also less likely.

Hypothesis 2a—the baseline adherence level is expected to be lower for the Medicaid population than for the privately insured population.

This is because of the higher non-monetary costs, which can be defined as access to care difficulties, associated with the Medicaid population. This is supported by numerous studies that have shown that the Medicaid insured population has worse health care access than the privately insured (see e.g. Berk and Schur, 1998 ; Kellerman and Weinick, 2012).

Hypothesis 2b—the difference-in-difference estimate of the effect of a false-positive mammogram on medication adherence is expected to be more positive (i.e. improve adherence more) for the Medicaid population than for the privately insured population

This is a result of the fact that the price effect can be thought of as a reduction in the non-monetary costs of adherence that comes about through increased interaction with the health care system related to the false-positive follow-up tests. We would only expect a significant price effect for a population that has poor enough baseline access to have the increased interaction surrounding the false-positive mammogram visits lead to a significant effect on medication

adherence. As mentioned above, the issue of poor health care access is more relevant to the Medicaid insured population than the commercially insured population.

Hypothesis 2c— a greater number of visits involved in the resolution of the false-positive should lead to a greater price effect and a greater difference-in-difference effect on medication adherence for the Medicaid population.

This is because, if as hypothesized, the positive medication adherence effect of a false-positive comes about through increased interaction with the health care system, then we would expect those who experience greater interaction with the medical care system to see the greatest improvement in medication adherence.

2.4 Methods

The analytic approach follows a difference-in-difference framework, where Figure 1 gives a broad overview of the approach. To identify the treatment group (i.e. false-positive mammogram) and control group (i.e. true-negative mammogram), I implement a three step claims-based algorithm. First, I identify all screening mammograms, which are defined as a claim with CPT code 76092 or ICD-9 procedure code V76.12 for a screening mammogram and no mammogram in the previous 12 months. The latter restriction is to eliminate any diagnostic mammograms that may have been coded as screening mammograms. Because none of the screening guidelines recommend screening more often than 12 months and Medicare does not reimburse for screening mammograms more often than every 12 months (US Preventive Services Task Force, 2009; National Cancer Institute, 2014), any mammogram within a year of a prior one is unlikely to be a screening mammogram.

Second, I determine whether the woman received additional follow-up testing in the 3 months following the screening mammogram to identify women who had an initial positive result. Follow-up testing is defined as having at least one claim for a subsequent mammogram (screening or diagnostic), a breast biopsy, a breast ultrasound, or other radiological testing of the breast (see **Table A-2-6** for codes based upon Cooper et al, 1999; Warren et al., 1999; Freeman et al., 2000; Randolph et al., 2002; Tan et al., 2006; Fenton et al., 2014). Although I do not have data on the actual screening mammogram results, it is unlikely a woman would receive any of these follow-up tests unless the initial mammogram came back as "positive" or at least "abnormal". This is supported by a study by Hubbard et al. (2014). Using actual mammogram test results linked to Medicare claims data the authors show that 99.4 percent of women with a follow-up test did indeed have an initial abnormal mammogram. And finally, I restrict the sample to only those women who never have a claim for breast cancer treatment, thus eliminating the true-positive and false-negative claims.

After defining the treatment and control groups, I define a series of dependent variables related to cholesterol and diabetes medication adherence. The primary dependent variable of interest is the number of days without the drug class of choice, measured in both the pre-period (time before the screening mammogram) and the post-period (time following the screening mammogram). This is calculated as the total days of drug supply in the period subtracted from the total number of days in the period, similar to the inverse of the commonly used medication possession ratio (Balkrishnan et al., 2003). I choose days with no drugs as the primary dependent variable for several reasons. First, while there are methodological debates about the best approach to measure medication adherence (see e.g. Balkrishnan. 2005), not having the medication in one's possession unequivocally implies non-adherence. Second, the continuous

nature of the variable allows for some insight into the magnitude of the effect. For the Medicaid analyses, the pre- and post-periods are each 12 months to allow for adequate identification of baseline levels of adherence and to observe longer term follow-up adherence. Due to the data limitation of only having two years of commercially insured data I have to limit the pre- and post-period to 9 months for the commercially insured population.

In addition, I also define adherence as an indicator variable for having access to the drug class for at least 80% of the days in the period, a commonly used cut-off of clinically significant adherence (see e.g. Rozenfeld et al., 2008).[10] Finally, I consider the most extreme version of non-adherence—complete discontinuation of the medication, defined as having no further medication claims beginning at least three months from the end of the post period. The purpose of the three month cut-off is to ensure that the lack of further medication claims is not an artifact of reaching the end of the available data. Ultimately, I do not focus on this dependent variable because I find no evidence that a false-positive mammogram has any effect on medication discontinuation. However, given the extreme nature of this measure and restricting the analysis sample to individuals who are not just initiating the medication, this is not surprising.

2.5 Econometric Model

The primary set of analyses relies on a difference-in-difference framework. Generally, the analyses are of the form:

$$Y_{i,t} = \beta_0 + \beta_1 X_{i,t} + \beta_2 Post_i + \beta_3 FP_i + \beta_4 FP_i \times Post_i + \varepsilon_{i,t}$$

Here $Y_{i,t}$ is the medication adherence outcome of interest for person i in time period t and $X_{i,t}$ is a vector of demographics and other variables to control for observable characteristics. The

[10] Another possible dependent variable was to just look at the number of refills in the period. However, nearly all of claims were for a 30 day supply so it did not offer any additional information over the preferred variable definition.

X vector includes variables such as age, race/ethnicity (which is only available in the Medicaid data), time since first prescription fill (to control for variability in adherence behavior over time), number of comorbidities based on the Charlson comorbidity index(CCI)[11], and year fixed effects. FP is an indicator variable that equals one for women who have a false-positive mammogram and zero for women whose mammogram result is a true-negative. The indicator variable Post refers to the period after the screening mammogram. The pre- and post-periods are each, respectively, aggregated into a single observation as is usually done with medication adherence for ease of interpretation, but which also has the additional benefit of helping to control for issues related to serial correlation (Bertrand, Duflo, Mullainathan, 2004). Since each individual has multiple observations in the data, I cluster the standard errors at the individual level.

One complication is that in a difference-in-difference analysis, the control group typically is not exposed to any treatment. The control group in this study receives a mammogram but no information that the mammogram may have been abnormal nor any follow-up testing. Therefore, the treatment is not the screening mammogram itself but the false-positive, which includes some unobservable level of communication to the patient that the initial mammogram was abnormal[12], then follow-up testing with varying intensity and invasiveness, and then ultimately being told that breast cancer is not present.[13]

[11] See Quan et al. (2005) for diagnosis codes.
[12] Because this is not observable it is possible the patient is never told anything except to return for follow-up testing. Other than possibly attenuating the observed effect of a false-positive, this does not affect the analysis, except that the false-positive "treatment" should be thought of as including some distribution of varying levels of communication rather than a single type of communication.
[13] This is identified as a lack of ever receiving any breast cancer treatment. Although it is possible that a woman could be diagnosed with breast cancer but never receive any treatment, this should be rare. Even within the National Breast and Cervical Cancer Early Detection Program for low income and under insured women, average time to treatment is about 60-70 days (Lantz and Soliman, 2009), which should be captured within the post-period.

For all analyses, β_4, the difference-in-difference estimate of the effect of a false-positive mammogram on medication adherence is the primary variable of interest. However, the models for each hypothesis vary slightly and require slightly different interpretations of the difference-in-difference estimate. For hypothesis 1b, the difference-in-difference estimate is predicted to be smaller in magnitude, regardless of sign, for a model where diabetes medication adherence is the dependent variable compared to when cholesterol medication adherence is the dependent variable. Therefore, for both the Medicaid insured and commercially insured samples, I compare the β_4's in the diabetes medication adherence regression to the cholesterol medication adherence regression. In addition I present the full triple difference-in-difference estimate. I use a similar approach for hypothesis 2b, where the difference-in-difference estimate is predicted to lower the days with no drugs (improve adherence) more in the Medicaid insured population than the commercially insured population. For each medication, I compare the β_4's for the Medicaid insured regression to the commercially insured regression as well as provide an estimate of the triple difference-in-difference.

In addition to these baseline regressions, I run two sets of robustness checks. First, I re-estimate all regressions for the Medicaid population using a 9 month pre- and post-period to match the commercially insured regressions. Second, to deal with possible differences between the false-positive and control groups, I re-estimate the regressions using propensity score matching, specifically kernel matching, to further ensure close matching of the control group to the false-positive group (Smith and Todd, 2005). I match based on age, race/ethnicity, comorbidities, days to first medication refill claim in the data, baseline depression/anxiety medication utilization, and baseline days with no drugs. For each set of regressions, I then bootstrap standard errors using 1000 iterations. I specifically choose kernel matching because the

matching procedure includes all individuals in the matching process, weighting each individual in the control group by how similar they are to the false-positive group. The advantage of this is that is unlikely to suffer from the bootstrap problems associated with nearest neighbor matching (Abadie and Imbens, 2008).

2.6 Data

All analyses are based on two sets of claims data—the 2003-2007 Truven Health MarketScan Medicaid Multistate Database and the 2003-2004 Truven Health MarketScan Commercial Claims and Encounters Database. The Medicaid database includes administrative claims data for approximately 7 million Medicaid recipients from 8 geographically diverse, but not identifiable states. While the states are meant to be representative, the idiosyncrasies of each state's Medicaid program may somewhat limit the representativeness of these 8 states. The commercial claims database includes claims and enrollment data from over 13 million commercially insured beneficiaries and their dependents collected from employer and health plan data from a variety of types of health plans including fee-for-service, preferred provider organizations, health maintenance organizations, and others (Adamson, Chang, and Hansen, 2006). The plans primarily come from self-insured, medium, and large employers. Comparing the Marketscan commercial enrollees to the nationally representative sample in the Medical Expenditure Panel Survey with employer sponsored health insurance, Aizcorbe et al. (2012) find the Marketscan sample to be generally similar demographically except for being over-representative of the southern region. Both sets of claims data include inpatient, outpatient, and prescription drug claims in addition to enrollment and plan characteristic data as well as a limited number of demographic variables (Adamson, Chang, and Hansen, 2006). The primary advantage of the Marketscan claims data for these analyses is they are the only data set with both a large,

representative population and information on all types of medical care utilization.[14] Given the research design, a large claims database is necessary because the analysis sample includes many restrictions leading to a significant reduction in sample size from the full data set.

To get to the final analysis sample, the sample is first restricted to the female population between the ages of 40 and 64. Although males can get breast cancer it is rare (less than 1% of all cases worldwide) so they are not routinely screened and are therefore excluded from the analyses (Korde et al., 2010). Women under age 40 are not included because current guidelines do not recommend screening for this population (US Preventive Services Task Force, 2009; National Cancer Institute, 2014), while women ages 65 and older are not included due to their eligibility for Medicare and therefore the high possibility of having incomplete claims data for them. The sample is further restricted to women who have a screening mammogram, defined as a screening mammogram and no claim for a prior mammogram within the past 12 months, and have at least one claim for a cholesterol medication or a diabetes medication in the pre-period.[15] To avoid the complication of women initiating the medication partially through the pre-period, the sample excludes women whose first ever observed fill for the cholesterol or diabetes medication is in the pre-period.

Finally, to ensure that no claims are missed in the pre- or post-period, the sample is further restricted to women who are continuously enrolled in Medicaid for 12 months before and after the screening mammogram or in commercial insurance for 9 months before and after the

[14] Although Medicare claims would offer similar advantages, the rate of mammography false-positive declines significantly with age (Elmore et al., 1998).

[15] The sample does not require that women have at least one fill in the post-period to allow for the possibility that a women completely discontinues medication following a false-positive mammogram. One possible issue is that this means a woman may discontinue medication before the screening mammogram. However, this issue is mitigated by two factors. First, given the difference-in-difference set up this is only an issue if false-positive or control group disproportionately discontinue medication, which is not the case empirically. And second, I find complete discontinuation beginning in the pre-period is rare.

screening mammogram. The reason for the difference in sample periods is due to a data limitation—with only two years of commercial claims, no women have 12 months of continuous enrollment before and after the screening mammogram. After all of these sample restrictions, the final analysis sample ranges from 9,598 to 14,520 observations for the Medicaid insured diabetes and cholesterol medication population, respectively and 8,866 to 27,936 observations for the commercially insured diabetes and cholesterol medication population, respectively.

2.7 Results

Table 2-1 shows baseline means for the Medicaid and commercially insured false-positive and control groups for both diabetes and cholesterol drugs. Overall demographics such as age and race/ethnicity, where available, are similar between treatment and control groups. The control group tends to have slightly more screening mammograms[16] than the false-positive group with the exception of the Medicaid diabetes population. Within the Medicaid population, for both therapeutic classes the false-positive group appears to have slightly more comorbidities based on the CCI (although not clearly higher for any given comorbidity) and somewhat higher medical costs. However, for the commercially insured population, costs and comorbidities appear similar between treatment and control.

Table 2-2 shows pre- and post-period means for the number of days with no drugs and percent adherent defined as having medication for at least 80% of days in the observation period. The first thing to note is that across the board, adherence declines over time (days with no drugs

[16] Number of screening mammograms is somewhat complicated by the fact that it includes both frequency of screening and time in the data. However, it is the best estimate of how recently a woman received a screening mammogram. The slightly higher rates for the false-positive group are not necessarily unexpected as not having a previous screening mammogram to refer or going a long time between mammograms may increase the likelihood of a false-positive (Christiansen et al., 2000).

increase), which is typical of medication adherence (Chapman et al., 2005). Second, for all but the diabetes drugs in the Medicaid insured population, adherence in the pre-period tends to be better for the control group. While the difference-in-difference identifying assumption of parallel trends is not necessarily violated with these baseline differences, as a robustness check I also run propensity score weighted models where the control group is matched to the false-positive group on the baseline variables age, race/ethnicity, comorbidities, days to first fill in data, baseline depression/anxiety medication utilization, and baseline days with no drugs to try to mitigate some of the differences between the two groups. I present these results following the main results. As predicted in hypothesis 1a, baseline adherence is higher (fewer days without drugs) for diabetes medication than for cholesterol medications for both the Medicaid and commercially insured. Since adverse health outcomes present more quickly for diabetes medications than for cholesterol medications, the consequences of partial adherence to diabetes medication appear more quickly leading to better adherence beginning in the pre-period. The baseline adherence means also support hypothesis 2a that the Medicaid insured likely have lower baseline adherence for both diabetes and cholesterol medications due to greater non-monetary costs as a result of greater access problems. As Table 2-2 shows for each medication, baseline medication adherence is lower for the Medicaid insured than the commercially insured.

Table 2-3 shows the difference-in-difference estimates for each of the insurance group and drug class combinations (see Table A-2-2 for all coefficients). For ease of interpretation I present OLS results for the days with no drugs and adherence regressions. However, the non-linear models (i.e. negative binomial regression for the days with no drugs regressions and logistic regression for the adherence regressions) yield substantively similar results. As predicted by hypothesis 1b in the theoretical framework, there is a smaller (and not statistically significant)

spillover effect of a false-positive mammogram for the diabetes medications. Although the triple difference-in-difference estimate in Table A-2-3 indicates that the difference is not statistically significant, in all regressions the magnitude of the difference-in-difference estimate is larger for the cholesterol medications than for the diabetes medications.

In addition, Table 2-3 shows that as predicted by hypothesis 2b, the false-positive mammogram only improves adherence (lowers the number of days with no drugs) for cholesterol medication in the Medicaid population—significantly decreasing the number of days with no cholesterol drugs by 7.25 days for the Medicaid population (an approximate 8.4% improvement in adherence off of the baseline adherence for the false-positive group). To better understand this improvement, as Table 2-2 and Table 2-3 indicate, adherence is actually declining over time for both the false-positive and control groups. Therefore, the cholesterol medication adherence improvement seen for the false-positive group is a result of a reduced rate of decline for the false-positive group compared to the control group. However, as Table 2-2 shows this improvement has practical significance as the false-positive group actually goes from worse pre-period adherence (85.9 days with no cholesterol drugs vs. 81.6 days for the control group) to better post-period adherence (97.7 days with no cholesterol drugs vs. 100.6 days for the control group).

Results from Table 2-3 also confirm hypothesis 2b from the theoretical framework—the positive effect of a false-positive mammogram on medication adherence only occurs for the Medicaid insured population, likely due to worse baseline access to the health care system. For the commercially insured, while the model does not predict a strong price effect, the overall sign of the difference-in-difference term is not clear because it represents an average effect over both the negative effect (lower trust in the medical care system) and the positive effect, which are

difficult to empirically disentangle. Therefore for the commercially insured, I can only report this overall effect with little additional evidence to suggest the mechanism driving it. For the commercially insured, I find a false-positive leads to a significant 4.05 day increase in the days with no cholesterol drugs (an approximate 7.7% worsening of adherence compared to baseline baseline) and non-statistically significant increase of 3.12 days with no diabetes drugs. Again while both the false-positive and control groups see increasing days with no drugs over time, the increase is even greater for the false-positive group compared to the control group.

In addition to the days with no drug regression, Table 2-3 also shows results for the linear probability model for being adherent, defined as having medications for at least 80% of days. Although not statistically significant, all results are same-signed[17] as the days with no drugs results. For cholesterol medications, adherence improves by just under 3 percentage points for the Medicaid population, while adherence declines by 2 percentage points for the commercially insured population. Although these estimates may not seem large they represent potentially clinically meaningful effects.

The remaining supplemental results all seek to support a better understanding of the mechanism underlying the improvement in cholesterol medication adherence following a false-positive mammogram for the Medicaid insured population. The theoretical framework predicts that for the Medicaid insured one possible mechanism for this improvement is increased interaction with the health care system, which is characterized in the theoretical framework as a reduction in non-monetary costs. To demonstrate that improved access is the likely mechanism by which cholesterol medication adherence improves following a false-positive mammogram for

[17] Same signed in the sense of improved adherence for cholesterol medication for the Medicaid insured and worsening adherence for the commercially insured. However, greater days with no drugs is really worse adherence so the estimates are expected to have opposite signs.

the Medicaid insured population, I redefine the treatment variable to be the number of unique visits for one of the false-positive follow-up tests. This is necessarily set to zero for the control group. If improved interaction with the medical care system is the likely mechanism by which cholesterol medication adherence improves then the greater the number of follow-up visits the lower the non-monetary costs of adherence (hypothesis 2c). Again, this reduction in non-monetary costs operates either through increased opportunity for a refill prescription to be written or more likely through encouragement to maintain medication adherence. As Table 2-4 shows, the greater the number of follow-up visits, the larger the improvement in cholesterol medication adherence (i.e. fewer days with no drugs). For cholesterol medications, each additional follow-up visit leads to a 5.28 decrease in the number of days with no drugs with a mean of 1.17 follow-up visits.

The clinical importance of the baseline results is further demonstrated by dividing the sample into those who were adherent (at least 80 percent of days with the drug) and those who were not in the pre-period. As Table 2-5 shows, the significant false-positive effects are largely concentrated in the group most likely to be targeted by medication adherence improvement efforts (i.e. those who are not adherent in the pre-period and therefore most at risk for detrimental clinical outcomes arising from suboptimal medication adherence). On the positive side, this population is most likely to improve medication adherence with improved access among the Medicaid insured. However, on the downside, this already potentially vulnerable population is the one seeing the negative effect of a false-positive mammogram on medication adherence among the commercially insured.

In addition to the main results I run a series of robustness checks. The first sensitivity analysis is to re-run the Medicaid regressions using a 9 month pre- and post-period to more

closely match the commercially insured analyses. The results do not qualitatively change as Table 2-6 shows. This means despite being restricted to using 9 month pre- and post-periods for the commercially insured analyses, the differences in results for the two populations is unlikely to be due to this limitation. Although one point to note is that the 9 month effect is slightly smaller than the 12 month effect. Since this means the effect increases even after the follow-up visits have been completed, this suggests that at least part of the improvement in cholesterol medication is likely due to the "wake-up call" effect.

For the second set of sensitivity analyses, one concern is that the false-positive group would not have looked like the control group absent the false-positive due to baseline differences shown in **Table 2-1** and Table 2-2. I therefore run a series of propensity score matching regressions using kernel matching. Table 2-6 shows these results, which appear very similar to the primary results. Although the standard errors are higher, the point estimates are very similar. The cholesterol medication adherence improvement seen in the Medicaid population is even slightly higher. These results suggest the issues resulting from the differences in baseline observed value may not be too serious and are unlikely to be driving the observed effects.

The final set of sensitivity analyses attempt to determine whether the effects on medication adherence vary by the experience of the false-positive itself. The first set of results shown in Table A-2-4 examine how the length of time it takes to resolve the false-positive (i.e. the number of days from the first follow-up test to the final follow-up test) affect medication adherence. Not surprisingly the length variable main effect is small and not statistically significant, since this just demonstrates that the length of follow-up is not related to baseline adherence levels. The results for the Medicaid population are further suggestive of increased interaction with health care system in part explaining the adherence improvement following a

false-positive. For the Medicaid, population the longer the time to resolve the false-positive, the greater the adherence improvement (i.e. fewer days with no drugs), although the results are only statistically significant for diabetes medications. The results are more mixed for the commercially insured population, including a significant increase in days with no diabetes medications with each additional day it takes to resolve the false-positive. This suggests the effect of length of follow-up is likely different for the Medicaid and commercially insured populations. Although given data and sample size limitations it is difficult to further tease out exactly what might be driving these differences.

The second of results examine how the effects vary by whether the invasiveness of the false-positive, namely whether the woman receives a breast biopsy or not. However, as Table A-2-5 shows I find no significant effect on the biopsy, post-period interaction terms, although the small sample sizes of women undergoing a breast biopsy likely contribute to the lack of a finding. Interestingly, for the Medicaid population taking cholesterol medications, the sample undergoing a breast biopsy seems to have significantly better baseline medication adherence. A possible explanation is that these women are generally more likely to follow physician care directives or more likely to seek out or advocate for all possible care.

2.8 Discussion

Despite numerous studies examining factors affecting medication adherence, I find some of the first evidence that suggests experience with a seemingly unrelated type of care such as mammography can significantly affect medication adherence. Using a plausibly exogenous shock to the mammogram care experience, I find a significant decline in cholesterol medication adherence for the commercially insured but a significant improvement in adherence for the Medicaid insured. I then provide evidence that suggests the improvement for the Medicaid

population is a result of increased interaction with the health care system. The results suggest that medication adherence needs to be thought of more globally in terms of care received by the patient—negative care experiences can have significant spillover effects onto medication adherence and even unrelated interactions with the health care system afford an opportunity to ensure that patients, especially those with limited access, adhere to their medications. Further, the opposite effects for the Medicaid and commercially insured populations suggest that these spillover effects may be further complicated by individual level characteristics such as insurance status.

The negative effect on cholesterol medication adherence for the commercially insured population suggests that follow-up care for women experiencing a false-positive may be essential not just to appropriately handle increased depression and anxiety but also to help minimize potential negative spillover effects onto other types of preventive health care. One note, though, is that although I do not find a negative effect on adherence for the Medicaid population, this does not mean there is no negative effect for this population. It could be a result of the price effect (due to improved access) outweighing the potential negative effect arising from decreased trust in the medical system. Post false-positive mammogram care may therefore still be very important for the Medicaid insured to ensure that they receive appropriate care to deal with a potentially difficult experience.[18] However, additional research may be needed to better understand the mechanism that is leading to worse cholesterol medication adherence following a false-positive mammogram and whether this effect is present for other types of false-positive results and for other types of preventive care including other types of medication, vaccinations, or other types of screening.

[18] In my concurrent work showing increased depression/anxiety medication initiation for women experiencing a false-positive mammogram, I find significant increases for both the commercially insured and Medicaid insured.

The estimated improvement in cholesterol medication adherence for the Medicaid population suggests that policies that help improve interaction with the health care system, but without the serious negative consequences of a false-positive, are essential for improving medication adherence for vulnerable populations that may suffer from access problems. As the results show, increased interaction with the health care system is essential to improving medication adherence, but the key is to find the most cost-effective way of doing this. One example may be policies to ensure that even during visits for unrelated types of care, medical care providers (physician or non-physician) communicate with patients about all medications they may be taking. Another example would include policies to encourage more frequent contact with the health system but through low cost providers. However, this study is just one example of this type of spillover effect. Future research is needed on the spillover effects of other types of cancer screening (e.g. Pap test or colorectal cancer screening) on medication adherence for cholesterol, diabetes, and additional types of medications. With additional data I also plan on looking at the effect of a false-positive on future mammogram screening[19]. Finally, this approach could be applied to a number of other types of screening where false-positives are important (e.g. newborn screening).

Another important issue relates to some possible anticipated effects of the ACA. The ACA includes two important provisions that relate to the results of this study. First, the ACA requires all health insurance plans to cover annual mammograms for women ages 40 and older with no cost sharing. This has the possibility of increasing both the number of women receiving mammograms but also the number experiencing a false-positive. With potentially more women

[19] With limited years of data, this was not currently feasible. To get a sense of pre- and post-period screening I would need multiple years of data for each woman. The advantage of analyzing medication adherence is that it is not as data restrictive.

experiencing false-positive mammograms, policies should be in place to help with follow-up care to ensure that appropriate care continues and to help with the potentially significant anxiety that can surround a false-positive result.

Second, a large portion of the ACA health insurance expansion is expected to occur through Medicaid expansion in the states that have chosen to expand the program. The results of this study suggest that despite having insurance, it may be important to ensure that the Medicaid insured population has adequate access to physicians to maintain optimal levels of medication adherence. Especially for Medicaid insured individuals it may be important that medical care providers discuss health issues that may be unrelated to the specific visit and to proactively maintain regular contact with patients who have chronic conditions treated with medications.

2.9 Limitations

Although this is the first study to examine how medication adherence can be affected by the potential spillover effects of a false-positive mammogram, there are several limitations. First, due to the use of claims data, I do not actually have information on the specific test results so I do not definitively know whether the mammogram actually came back as positive or abnormal. However, in the case of the false-positive definition it is unlikely that a woman would receive a subsequent mammogram, a breast biopsy, or the other follow-up tests unless it was to further test for the likelihood of breast cancer. This is consistent with the findings in Hubbard et al. (2014) that finds that a claims-based algorithm for identifying abnormal screening mammograms has a sensitivity of 74.9 percent and specificity of 99.4 percent. In addition there is the possibility that a mammogram could come back as positive or abnormal but a woman did not return for follow-up testing. However, this is most likely to bias the results towards zero as some treatment woman would be misclassified as being in the control group.

A limitation of the Medicaid results is the significant sample restrictions that require two years of continuous Medicaid enrollment. While this is necessary to ensure that no claims are missed, it means the Medicaid results are not representative of the general Medicaid population, which is characterized by high rates of churn (Short and Graefe, 2003; Centers for Medicare and Medicaid Services, 2000). In ongoing work, I find the continuously enrolled population is poorer, sicker, and more likely disabled. So while this limits the generalizability of the Medicaid results to the overall Medicaid population, the restriction is necessary to ensure the internal validity of the results while also focusing on a population that may be important from a public health standpoint.

Another limitation is that I look at medication adherence within a therapeutic class so I do not look at drug switching. Although future work will examine the issue of medication switching, I assume that an individual has a medical need for at least one medication in the therapeutic class so looking at access to any drug is the most conservative level of adherence.[20] While I may miss partial non-adherence to one but not all medications, my measure of adherence can be thought of as the minimum required treatment—a day with no medication of any kind indicates a true lack of available treatment.

The final limitation is that I have a limited set of years, in particular for the commercially insured population, and a limited set of demographics. Future work will try to add years of commercially insured claims, while the issue of limited demographic information is hopefully mitigated by the rich set of health data. In particular, longer follow-up would be needed to assess whether the observed effects are transitory or more persistent.

[20] Given that the sample is restricted to individuals who have not just initiated medication and have not stopped completely during the pre-period, the analysis sample is largely individuals who have taken the medications for more than just a short period of time. Therefore it is more likely that they receive medical benefit from the medications.

2.10 Conclusions

While many studies have examined factors affecting medication adherence, this study provides the first example of how a woman's mammogram screening experience, a seemingly unrelated type of care, may have significant spillover effects onto cholesterol medication adherence. Focusing on the experience of a false-positive mammogram, a plausibly exogenous shock to the preventive care experience, I show that a false-positive leads to a significant improvement in cholesterol medication adherence for the Medicaid insured but a significant decline in adherence for the commercially insured. The results highlight the complicated nature of this type of spillover effect. On the one hand, for a vulnerable population with problems accessing the medical care system, a false-positive mammogram may lead to increased contact with the health care system and consequently improved cholesterol medication adherence. However, there may be significant negative consequences of a false-positive on medication adherence in addition to or as a result of the negative affect and lower trust in health care following a false-positive. The results of this paper suggest that future health policy needs to better help women who have experienced a false-positive mammogram to ensure appropriate care is received, as well as looking for improved ways to help women with limited access to the health care system gain access to improve medication adherence without the negative consequences of experiencing a false-positive.

2.11 References

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Figure 1 Diagram of the Analytic Approach

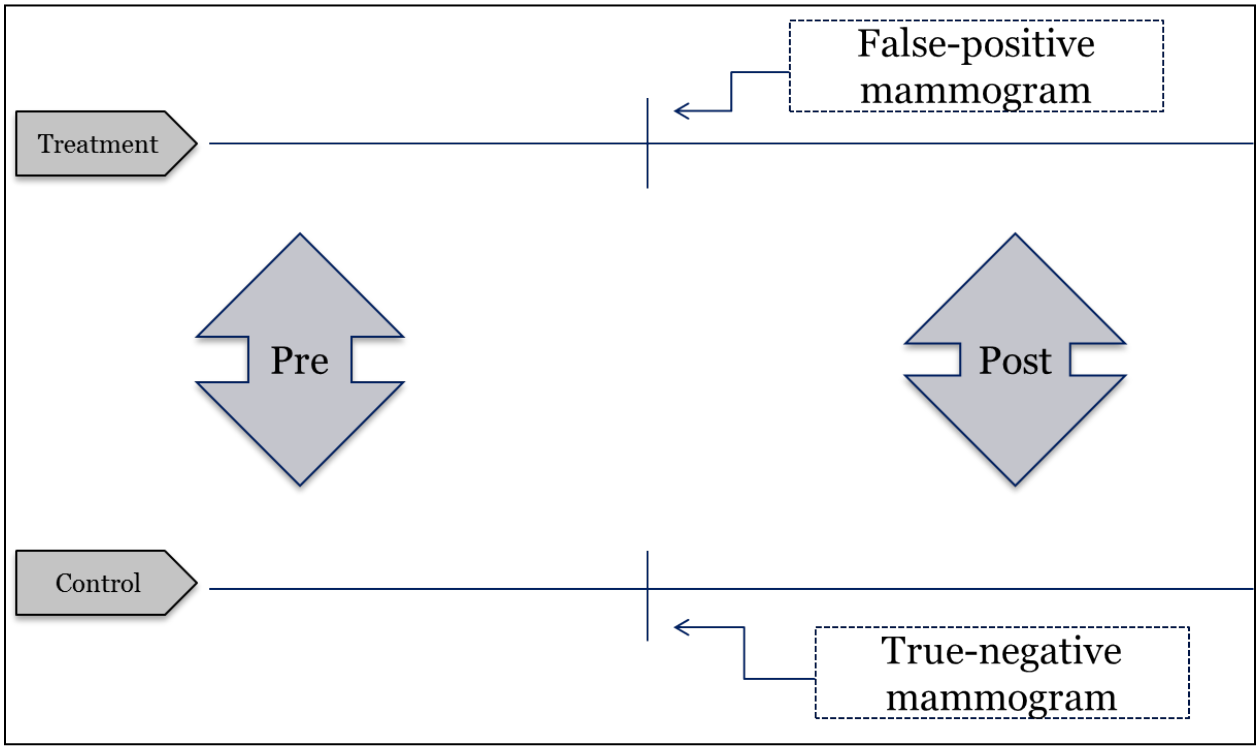

Table 2-1 Baseline means by insurance group and medication class

	Medicaid				Commercial			
	Diabetes		Cholesterol		Diabetes		Cholesterol	
Pre-period characteristics	Control group	FP group	Control group	FP group	Control group	FP group	Control group	FP group
Age	54.9	54.2	55.3	54.6	55.9	55.5	56.9	56.4
	(6.2)	(6.5)	(6.2)	(6.4)	(5.6)	(6.0)	(5.2)	(5.6)
White	0.39	0.40	0.49	0.50	-	-	-	-
	(0.49)	(0.49)	(0.50)	(0.50)				
Black	0.42	0.44	0.32	0.32	-	-	-	-
	(0.49)	(0.50)	(0.47)	(0.46)				
Hispanic	0.03	0.02	0.02	0.02	-	-	-	-
	(0.17)	(0.15)	(0.15)	(0.14)				
Other race	0.17	0.13	0.17	0.17	-	-	-	-
	(0.37)	(0.34)	(0.38)	(0.37)				
Number of screening mammograms	2.76	2.81	2.77	2.70	1.82	1.40	1.82	1.47
	(0.48)	(0.92)	(0.48)	(0.94)	(0.39)	(0.49)	(0.38)	(0.50)
Days to first fill in data	752.8	781.3	719.2	737.3	347.4	338.2	346.0	338.6
	(275.4)	(292.7)	(266.9)	(280.6)	(51.2)	(49.4)	(49.9)	(48.1)
Capitated Medicaid	0.43	0.40	0.42	0.41	-	-	-	-
	(0.49)	(0.49)	(0.49)	(0.49)				
Time between screening mammogram and FP resolution	-	26.5	-	26.4	-	24.21	-	21.12
		(21.4)		(21.2)		(20.55)		(18.99)
False-positive included biopsy	-	0.07	-	0.06	-	0.1	-	0.1
		(0.25)		(0.24)		(0.2)		(0.2)

Outpatient/ER costs	7,645	8,728	7,293	7,688	3,125	3,189	2,338	2,589
	(12,212)	(14,278)	(13,028)	(14,059)	(6.974)	(5,327)	(4,822)	(4,390)
Inpatient costs	2,798	4,272	2,135	3,036	1,040	1,381	667	821
	(12,082)	(16,121)	(10,045)	(13,828)	(5,465)	(7,264)	(4,247)	(4,436)
# of CCI comorbidities	1.37	1.49	0.94	0.97	1.31	1.29	0.44	0.42
	(1.50)	(1.52)	(1.31)	(1.32)	(0.81)	(0.82)	(0.73)	(0.72)
N	8,688	910	13,112	1,408	7,936	930	25,096	2,840

Means (SD)

FP: False-positive

* All values are for the pre-period

Table 2-2 Means for adherence variables

Medicaid

	Diabetes				Cholesterol			
Variable	Control pre	Control post	FP pre	FP post	Control pre	Control post	FP pre	FP post
Days no drugs	46.8	54.1	44.4	50.6	81.6	100.6	85.9	97.7
	(78.3)	(88.9)	(76.5)	(84.6)	(99.8)	(115.3)	(102.1)	(112.7)
At least 80% of days with medication	0.77	0.75	0.78	0.76	0.63	0.57	0.61	0.58
	(0.42)	(0.43)	(0.41)	(0.43)	(0.48)	(0.49)	(0.49)	(0.49)
Discontinue medication	0.03	0.03	0.03	0.03	0.06	0.06	0.06	0.06
	(0.17)	(0.17)	(0.17)	(0.17)	(0.24)	(0.24)	(0.24)	(0.24)
N	4,344	4,344	455	455	6,556	6,556	704	704

Commercial

	Diabetes				Cholesterol			
Variable	Control pre	Control post	FP pre	FP post	Control pre	Control post	FP pre	FP post
Days no drugs	31.1	38.1	36.1	46.3	45.9	59.0	52.1	69.2
	(55.7)	(65.5)	(57.5)	(72.1)	(63.5)	(76.8)	(67.3)	(83.7)
At least 80% of days with medication	0.79	0.75	0.73	0.69	0.71	0.64	0.68	0.59
	(0.40)	(0.43)	(0.44)	(0.46)	(0.45)	(0.48)	(0.47)	(0.49)
Discontinue medication	0.03	0.03	0.04	0.04	0.06	0.06	0.05	0.05
	(0.18)	(0.18)	(0.20)	(0.20)	(0.25)	(0.25)	(0.22)	(0.22)
N	3,968	3,968	465	465	12,548	12,548	1,420	1,420

Means (SD)
FP: False-positive

Table 2-3 Main regression results: difference-in-difference estimates

	Medicaid		Commercial	
Variable	Diabetes	Cholesterol	Diabetes	Cholesterol
		Days with no drugs		
False-positive	-1.73	4.21	4.51	5.99***
	(3.71)	(3.92)	(2.75)	(1.86)
Post	7.26***	19.08***	6.97***	13.05***
	(0.97)	(1.13)	(0.80)	(0.58)
False-positive x Post	**-1.04**	**-7.25****	**3.23**	**4.05****
	(3.43)	**(3.48)**	**(2.99)**	**(1.86)**
		Adherent		
False-positive	0.01	-0.03	-0.06***	-0.03***
	(0.02)	(0.02)	(0.02)	(0.01)
Post	-0.02***	-0.06***	-0.04***	-0.08***
	(0.01)	(0.01)	(0.01)	(0.01)
False-positive x Post	**<0.001**	**0.03**	**<0.001**	**-0.02**
	(0.02)	**(0.02)**	**(0.02)**	**(0.01)**
N	9,598	14,520	8,866	27,936

(Robust standard errors clustered by individual)
* p<0.10, ** p<0.05, *** p<0.01
Medicaid: 12 months pre/post, Commercial: 9 months pre/post
All regressions control for age, race/ethnicity (Medicaid only), time since first prescription, number of CCI comorbidities, year fixed effects.

Table 2-4 Difference-in-difference estimates for days with no drugs number of follow-up visits for the Medicaid population

Sample	Diabetes	Cholesterol
# follow-up visits x Post	-1.79	-5.28**
	(2.79)	(2.67)
N	9,598	14,520

(Robust standard errors clustered by individual)
* p<0.10, ** p<0.05, *** p<0.01
Medicaid: 12 months pre/post, Commercial: 9 months pre/post
All regressions control for age, race/ethnicity (Medicaid only), time since first prescription, number of CCI comorbidities, year fixed effects.

Table 2-5 Difference-in-difference estimates by pre-period adherence status for cholesterol medication

	Medicaid		Commercial	
Variable	Adherent in pre-period	Not adherent in pre-period	Adherent in pre-period	Not adherent in pre-period
False-positive x Post	1.64	-13.20**	1.27	13.74***
	(3.71)	(6.29)	(1.78)	(4.31)
N	9,124	5,396	19,816	8,120

(Robust standard errors clustered by individual)
* p<0.10, ** p<0.05, *** p<0.01
Medicaid: 12 months pre/post, Commercial: 9 months pre/post
All regressions control for age, race/ethnicity (Medicaid only), time since first prescription, number of CCI comorbidities, year fixed effects.

Table 2-6 Results from robustness checks for difference-in-difference estimates

	Days no drugs			
	Medicaid		Commercial	
	Diabetes	Cholesterol	Diabetes	Cholesterol
	Days no drugs			
Baseline	-1.04	-7.25**	3.23	4.05**
	(3.43)	(3.48)	(2.99)	(1.86)
Propensity score regressions	-1.45	-7.30	3.30	4.12
	(5.46)	(5.72)	(4.48)	(3.00)
9 month pre-/post-period	-0.66	-5.28*	-	-
	(2.67)	(2.56)		
	Adherent			
Baseline	<0.001	0.03	<0.001	-0.02
	(0.02)	(0.02)	(0.02)	(0.01)
Propensity score regressions	<0.001	0.03	< -0.001	-0.02
	(0.03)	(0.03)	(0.03)	(0.02)
9 month pre-/post-period	-0.01	0.01	-	-
	(0.02)	(0.02)		
N	9,598	14,520	8,866	27,936

(Robust standard errors clustered by individual)
* p<0.10, ** p<0.05, *** p<0.01 Medicaid: 12 months pre/post, Commercial: 9 months pre/post unless otherwise noted
All regressions control for age, race/ethnicity (Medicaid only), time since first prescription, number of CCI comorbidities, year fixed effects.

Table A-2-1 Baseline mean values for conditions within the CCI

	Medicaid				Commercial			
	Diabetes		Cholesterol		Diabetes		Cholesterol	
Pre-period characteristics	Control	FP	Control	FP	Control	FP	Control	FP
CCI: myocardial infarction (fraction)	0.03 (0.16)	0.02 (0.15)	0.03 (0.17)	0.03 (0.17)	0.01 (0.10)	0.01 (0.08)	0.01 (0.09)	0.01 (0.08)
CCI: congestive heart failure (fraction)	0.08 (0.27)	0.09 (0.28)	0.06 (0.24)	0.07 (0.25)	0.03 (0.16)	0.03 (0.18)	0.01 (0.11)	0.01 (0.12)
CCI: peripheral vascular disease (fraction)	0.05 (0.21)	0.07 (0.26)	0.04 (0.20)	0.05 (0.23)	0.02 (0.13)	0.02 (0.13)	0.01 (0.11)	0.01 (0.11)
CCI: peptic ulcer (fraction)	0.01 (0.08)	0.01 (0.10)	0.01 (0.07)	0.00 (0.00)	0.00 (0.02)	0.00 (0.05)	0.00 (0.02)	0.00 (0.00)
CCI: moderate/severe liver disease (fraction)	0.00 (0.05)	0.00 (0.07)	0.00 (0.02)	0.00 (0.04)	0.00 (0.03)	0.00 (0.00)	0.00 (0.01)	0.00 (0.00)
CCI: cerebrovascular disease (fraction)	0.07 (0.25)	0.08 (0.27)	0.08 (0.27)	0.09 (0.28)	0.05 (0.21)	0.03 (0.18)	0.04 (0.19)	0.03 (0.18)
CCI: chronic pulmonary disease (fraction)	0.21 (0.41)	0.24 (0.43)	0.22 (0.41)	0.26 (0.44)	0.10 (0.30)	0.10 (0.30)	0.09 (0.29)	0.08 (0.28)
CCI: dementia (fraction)	0.00 (0.05)	0.00 (0.00)	0.00 (0.05)	0.00 (0.04)	0.00 (0.04)	0.00 (0.00)	0.00 (0.02)	0.00 (0.03)
CCI: rheumatic disease (fraction)	0.03 (0.17)	0.02 (0.12)	0.03 (0.17)	0.02 (0.14)	0.02 (0.14)	0.02 (0.15)	0.02 (0.14)	0.02 (0.14)
CCI: mild liver disease (fraction)	0.01 (0.10)	0.01 (0.10)	0.01 (0.07)	0.01 (0.08)	0.01 (0.07)	0.01 (0.08)	0.00 (0.04)	0.00 (0.03)

	Medicaid				Commercial			
	Diabetes		Cholesterol		Diabetes		Cholesterol	
Pre-period characteristics	Control	FP	Control	FP	Control	FP	Control	FP
CCI: diabetes no complications (fraction)	0.59 (0.49)	0.63 (0.48)	0.30 (0.46)	0.29 (0.45)	0.90 (0.30)	0.87 (0.33)	0.19 (0.39)	0.18 (0.39)
CCI: diabetes with complications (fraction)	0.20 (0.40)	0.23 (0.42)	0.09 (0.29)	0.08 (0.27)	0.13 (0.33)	0.13 (0.34)	0.02 (0.15)	0.02 (0.15)
CCI: renal disease (fraction)	0.04 (0.20)	0.04 (0.18)	0.03 (0.17)	0.03 (0.18)	0.02 (0.13)	0.03 (0.17)	0.01 (0.10)	0.01 (0.11)
CCI: malignancy (fraction)	0.04 (0.19)	0.04 (0.18)	0.03 (0.17)	0.04 (0.19)	0.03 (0.18)	0.03 (0.18)	0.03 (0.16)	0.03 (0.16)
CCI: metastatic solid tumor (fraction)	0.00 (0.06)	0.00 (0.00)	0.00 (0.05)	0.00 (0.04)	0.01 (0.07)	0.00 (0.00)	0.00 (0.05)	0.00 (0.06)
CCI: HIV/AIDS (fraction)	0.01 (0.09)	0.01 (0.08)	0.01 (0.07)	0.00 (0.00)	0.00 (0.02)	0.00 (0.00)	0.00 (0.01)	0.00 (0.00)
CCI: hemiplegia or paraplegia (fraction) [21]	0.01 (0.08)	0.01 (0.10)	0.01 (0.08)	0.01 (0.09)	0.00 (0.05)	0.00 (0.00)	0.00 (0.04)	0.00 (0.05)
N	8,688	910	13,112	1,408	7,936	930	25,096	2,840

Means (SD)
* All values are for the pre-period

[21] All CCI variables fractions are for having any claim in the pre-period. Therefore it is also possible to have a condition but not have any claims, which explains why the diabetes column does not necessarily have a value of 1 for the CCI diabetes variables.

Table A-2-2 Full coefficients from main results (days with no drugs as dependent variable)

	Medicaid		Commercial	
	Diabetes	Cholesterol	Diabetes	Cholesterol
Age	-1.28***	-1.29***	-1.50***	-1.45***
	(0.21)	(0.2)	(0.16)	(0.11)
Black	16.86***	37.35***	-	-
	(2.73)	(2.97)		
Hispanic	11.96	41.59***	-	-
	(7.83)	(8.6)		
Other race	13.98***	19.44***	-	-
	(3.84)	(3.45)		
Days to first fill in data	-0.03***	-0.07***	-0.12***	-0.14***
	(0.01)	(0.01)	(0.03)	(0.02)
# of CCI comorbidities	-5.83	-54.30***	83.92***	6.46
	(36.07)	(12.76)	(30.49)	(23.81)
CCI: myocardial infarction	10.17	46.45***	-74.27**	-19.54
	(35.32)	(14.52)	(31.64)	(24.31)
CCI: congestive heart failure	10.09	51.99***	-81.95***	-11.09
	(36.26)	(13.45)	(31.01)	(24.2)
CCI: peripheral vascular disease	6.3	53.14***	-70.82**	-7.42
	(36.67)	(14.88)	(31.31)	(24.22)
CCI: peptic ulcer	28.02	51.24***	-90.99**	22.51
	(40.17)	(19.24)	(36.24)	(35.51)

	Medicaid		Commercial	
	Diabetes	Cholesterol	Diabetes	Cholesterol
CCI: moderate/severe liver disease	-	-	-	-6.05
				(30.61)
CCI: cerebrovascular disease	2.57	50.69***	-85.77***	-10.36
	(36)	(13.68)	(30.94)	(23.95)
CCI: chronic pulmonary disease	9.8	64.86***	-81.75***	-4.09
	(35.98)	(12.96)	(30.72)	(23.88)
CCI: dementia	42.63	52.57*	-99.17***	46.86
	(47.3)	(27.35)	(32.03)	(47.48)
CCI: rheumatic disease	26.24	70.80***	-75.42**	-2.91
	(37.4)	(14.19)	(30.86)	(24.15)
CCI: mild liver disease	52.92	80.06***	-84.85**	33.43
	(47.25)	(27.88)	(33.83)	(30.53)
CCI: diabetes no complications	5.37	47.87***	-107.44***	-11.64
	(36.13)	(13.19)	(30.69)	(23.89)
CCI: renal disease	16.18	53.80***	-69.53**	-12.91
	(36.93)	(14.59)	(31.91)	(24.19)
CCI: malignancy	-3.00	58.26***	-87.34***	-3.62
	(36.71)	(14.54)	(30.73)	(24.04)
CCI: metastatic solid tumor	33.04	58.32**	-101.88***	-17.84

	Medicaid		Commercial	
	Diabetes	Cholesterol	Diabetes	Cholesterol
	(45.71)	(26.1)	(31.51)	(25.94)
CCI: HIV/AIDS	-4.42	40.65**	-123.44***	-
	(37.2)	(19.67)	(30.84)	
CCI: diabetes with complications	3.96	53.94***	-83.74***	-8.84
	(36.45)	(13.46)	(30.59)	(24.31)
CCI: hemiplegia or paraplegia	27.42	54.11***	-66.26*	0.27
	(41.67)	(17.04)	(35.83)	(30.81)
FP	-1.73	4.21	4.51	5.99***
	(3.71)	(3.92)	(2.75)	(1.86)
Post	7.26***	19.08***	6.97***	13.05***
	(0.97)	(1.13)	(0.8)	(0.58)
FP x Post	-1.04	-7.25**	3.23	4.05**
	(3.43)	(3.48)	(2.99)	(1.86)
Constant	118.40***	204.44***	185.73***	183.26***
	(12.44)	(12.37)	(14.24)	(9.17)
N	9,598	14,520	8,866	27,936

(Robust standard errors clustered by individual)
* p<0.10, ** p<0.05, *** p<0.01 Medicaid: 12 months pre/post, Commercial: 9 months pre/post

Table A-2-3 Triple difference-in-difference estimates

	Medicaid		Commercial		DDD estimate (Medicaid vs. Commercial)	
	Diabetes	Cholesterol	Diabetes	Cholesterol	Diabetes	Cholesterol
		Days with no drugs			Days with no drugs	
DD estimate	-1.04	-7.25**	3.23	4.05**		
	(3.43)	(3.48)	(2.99)	(1.86)	-	-
DDD estimate	-	-6.21	-	0.81	-4.27	-11.30***
		(4.60)		(3.42)	(4.55)	(3.94)
		Adherent			Adherent	
DD estimate	<0.001	0.03	<0.001	-0.02		
	(0.02)	(0.02)	(0.02)	(0.01)	-	-
DDD estimate	-	0.03	-	-0.02	0.001	0.04*
		(0.03)		(0.03)	(0.03)	(0.02)
N	9,598	14,520	8,866	27,936		

(Robust standard errors clustered by individual)
* p<0.10, ** p<0.05, *** p<0.01
Medicaid: 12 months pre/post, Commercial: 9 months pre/post unless otherwise noted
All regressions control for age, race/ethnicity (Medicaid only), time since first prescription, number of CCI comorbidities, year fixed effects

Table A-2-4 Regression results by length of time to resolve false-positive

	Medicaid		Commercial	
Variable	Diabetes	Cholesterol	Diabetes	Cholesterol
		Days with no drugs		
False-positive	-7.86	6.37	7.12*	3.77
	(5.52)	(6.25)	(4.15)	(2.68)
Post	7.26***	19.08***	6.97***	13.05***
	(0.97)	(1.13)	(0.80)	(0.58)
False-positive x Post	7.93	-4.64	-2.76	6.74**
	(5.19)	(5.38)	(3.98)	
Length	0.19	-0.11	-0.12	0.09
	(0.19)	(0.17)	(0.12)	(0.09)
Length x Post	-0.34**	-0.10	0.25**	-0.13
	(0.15)	(0.15)	(0.12)	(0.09)
N	9,598	14,520	8,866	27,936

(Robust standard errors clustered by individual)
* p<0.10, ** p<0.05, *** p<0.01
All regressions control for age, race/ethnicity (Medicaid only), time since first prescription, number of CCI comorbidities, year fixed effects

Table A-2-5 Regression results by use of breast biopsy to resolve false-positive

	Medicaid		Commercial	
Variable	Diabetes	Cholesterol	Diabetes	Cholesterol
		Days with no drugs		
False-positive	-2.19	5.24	3.89	5.86***
	(3.82)	(4.14)	(2.81)	(1.92)
Post	7.26***	19.08***	6.97***	13.05***
	(0.97)	(1.13)	(0.8)	(0.58)
False-positive x Post	-1.53	-8.00**	2.8	4.25**
	(3.52)	(3.60)	(3.08)	(1.91)
Biopsy	-8.82	-29.22**	4.63	-2.60
	(14.49)	(12.3)	(12.28)	(7.56)
Biopsy x Post	7.37	11.79	8.4	-3.91
	(13.79)	(12.62)	(13.94)	(7.58)
N	9,598	14,520	8,866	27,936

(Robust standard errors clustered by individual)
* p<0.10, ** p<0.05, *** p<0.01
All regressions control for age, race/ethnicity (Medicaid only), time since first prescription, number of CCI comorbidities, year fixed effects

Table A-2-6 Diagnosis and procedure codes

Condition	Code type	Code #	Source
Screening mammogram	CPT	76092	Freeman et al. (2002); Tan et al. (2006); Randolph et al. (2002)
	ICD-9	V76.12	Adams and Koch (2014); Randolph et al. (2002)
Other mammogram	CPT	76090, 76091	Freeman et al. (2002); Tan et al. (2006); Randolph et al. (2002)
	ICD-9	87.37, V76.11	Freeman et al. (2002)
Breast biopsy	CPT	19100, 19101, 19120	Freeman et al. (2002); Tan et al. (2006)
	ICD-9	85.11, 85.12, 85.20, 85.21	Freeman et al. (2002); Tan et al. (2006)
Breast ultrasound	CPT	76645	Freeman et al. (2002); Tan et al. (2006)
Other breast radiological procedures	CPT	76003, 76086, 76087, 76088, 76095, 76098, 76100, 76101, 76102, 76120, 76125, 76140, 76150, 76350, 76355, 76360, 76362, 76365	Freeman et al. (2002)
	ICD-9	87.35, 87.36, 87.73, 88.85	Freeman et al. (2002)
Breast cancer treatment	CPT	19160, 19162, 19180, 19200, 19220, 19240, 38740, 38745	Warren et al. (1999); Cooper et al. (1999); Freeman et al. (2000)
	ICD-9	174.x, 233.0, V103	Warren et al. (1999); Cooper et al. (1999); Freeman et al. (2000)

Chapter 3.
The effect of false-positive mammograms on depression and anxiety medication initiation

3.1 Introduction and Background

There has been increasing research and attention paid to the possible adverse effects of false-positive mammograms. A recently published study by Ong and Mandl (2015) finds that the direct medical costs of a false-positive mammogram is $852 per beneficiary, leading to total costs of nearly $3 billion annually in the United States. Despite the substantial cost, this is likely an underestimate as it only includes the direct costs of the false-positive follow-up tests. There may be additional costs such as increased depression or anxiety, changes in future mammography utilization, or changes in decisions for other types of treatment. In this study I focus on the first effect, specifically the effect of a false-positive mammogram on the likelihood of initiating depression or anxiety medication.

While multiple studies, including several systematic reviews and meta-analyses have shown that a false-positive mammogram may lead to increased depression or anxiety (Brett et al., 2005; Brewer, Salz, and Lillie, 2007; Hafslund and Nortvedt, 2009; Salz, Richman, and Brewer, 2010; Bond et al., 2013), all of the extant studies have used survey or self-reported measures of depression or anxiety (e.g the Psychological Consequences Questionnaire, the Beck Depression Index, the General Health Questionnaire, and the Hospital Anxiety and Depression Scale among others) making it difficult to determine what the effect would be on clinical

utilization and what the potential costs of this adverse effect might be. This limitation is also true of several more recent studies that have examined the psychosocial effects of false-positive mammograms (Bredal et al., 2013; Brodersen and Siersma (2013); Tosteson et al., 2014; Bond et al., 2015; Heleno, Siersma, and Brodersen (2015a, b)).The current study is the first to demonstrate that this adverse effect of a false-positive rises to the serious clinical level of increasing the probability of depression or anxiety medication initiation. Importantly this demonstrates that the effect goes beyond reported depression and anxiety and it allows the estimation of an additional cost associated with a false-positive mammogram. The issue further highlights the importance of creating appropriate health policy to both improve post false-positive follow-up care especially for those most likely to suffer from increased anxiety or depression as well as trying to find ways to minimize the incidence of false-positive while maintaining proper identification of breast cancer cases. It therefore also provides an important input in the ongoing debate about optimal screening frequency and timing, which the US Preventive Task Force (2015) is in the process of revising.

In addition a few of the studies on the effect of false-positive mammograms on depression and anxiety have tried to identify the populations most at risk of adverse psychological consequences, typically focusing on women who receive invasive screening (i.e. a breast biopsy) and women who have a longer period of time to resolve the false-positive mammogram. There is somewhat mixed evidence on whether more invasive screening is more likely to lead to worse adverse psychological effects. Studies by Barton et al. (2001), Brett and Austoker (2001), and Lampic et al. (2001 all find worse psychosocial effects for women receiving a biopsy, while studies by Absetz (2003) and Espasa et al. (2012) do not find any difference. However, these studies all have limited sample sizes. A more recent and larger scale

study by Heleno, Siersma, and Brodersen (2015a) finds similar reported psychological consequences for women receiving an invasive procedure compared to those receiving a non-invasive procedure. However, this study also uses self-reported psychological measures and like many of the earlier studies is based in Europe, where the effects may be different from the United States.

A second set of studies have focused on whether increased time between the initial abnormal mammogram and final resolution leads to worse psychological consequences. A meta-analysis by Brett et al. (2005) finds increased waiting time to be a factor predicting worse psychosocial outcomes. Similarly, a study by Lindfors, O'Connor, and Parker (2001) find that immediate versus later work-up can reduce the stress of a false-positive mammogram. However, a more recent study by Heleno, Siersma, and Brodersen (2015b) finds no significant effect of time to resolution, although the sample was relatively small in terms of longer waiting times.

The importance of better understanding the harms surrounding a false-positive mammogram has been further highlighted by several studies comparing the rates of subsequent mammography screening for women who have a false-positive mammogram to those who have a true-negative mammogram. The results have been somewhat mixed but two findings stand out. First, several studies find significant decreases in the likelihood of future screening among women who receive a false-positive mammogram (Alamo-Junquera et al., 2012; Bond et al., 2013; Klompenhouwer et al., 2014), which is a particularly damaging effect of a false-positive. On the other hand, some studies have found the opposite—higher rates of subsequent screening for women experiencing a false-positive (Brewer et al., 2007; Maxwell et al., 2013). However, in these studies the mechanism appears to be that women believe they are at higher risk of breast cancer as a result of the false-positive. Furthermore, the higher rates of subsequent screening

may also need to be taken in context. In one of the largest recent studies, Setz-Pels et al. (2013) find comparable rates of subsequent mammography screening in the false-positive and true-negative populations but find that over the ensuing 4 years, the population that experiences a false-positive has significantly lower rates of screening. Again, one limitation with many of these studies is the smaller sample sizes and that many are based outside the United States, where effects may be very different. Although estimating the effect on future screening is outside the scope of this project, future work could use a similar framework to estimate this effect and also attempt to further connect this study's focus on depression and anxiety medication initiation to future screening behavior.

More generally, the issue of false-positive mammograms has been getting increased attention as both medical experts and public health policymakers debate the optimal frequency of mammography given growing evidence of potential harms (Pace and Keating, 2014; Tosteson et al., 2014; Welch and Passow, 2014). While the existing studies have focused on both false-positive mammograms as well as overtreatment (treatment of cancers that would not likely lead to mortality), I focus this study on false-positive mammograms. Highlighting the importance of false-positives in terms of mammography screening, there is increasing evidence that false-positive mammogram results are fairly common with estimates that between 1/3 to 1/2 of all women will experience a false-positive over 8 to 10 years of screening (Elmore et al., 1998; Hubbard et al., 2011; Jacobsen et al., 2015). As screening recommendations continue to change[22] (US Preventive Services Task Force, 2009; National Cancer Institute, 2014), it is important to get an accurate estimate of the potential adverse effects in order to best determine both appropriate screening frequency but more importantly to be able to best identify populations

[22] The current USPSTF mammography recommendations are also currently being updated with the current draft online with public comments just recently closed (US Preventive Task Force, 2015).

most at risk from harm in order to target harm minimization efforts—i.e. to better help the women who may need additional follow-up care or who might be less likely to screen in the future.

3.2 Methods

For all analyses I estimate the likelihood of initiating either depression or anxiety medication in the period following either a false-positive mammogram, a true-negative mammogram, or a true-positive mammogram while controlling for various individual level factors. I therefore first identify and restrict all analyses to the sample of women between the ages of 40 and 64 who have a screening mammogram. I exclude women under the age 40 since they are not currently included in any of the current screening guidelines for regular recommended screening (US Preventive Services Task Force, 2009; National Cancer Institute, 2014). I exclude women ages 65 and older because of their Medicare eligibility which could mean they have claims for additional mammography, follow-up screening, or medication utilization that I am unable to observe.

I further restrict the sample to women who have a screening mammogram, which I define as a claim for a screening mammogram (CPT code 76092 or ICD-9 procedure code V76.12) with no claim for a mammogram in the prior 12 months. The 12 month look back period is to ensure that the screening mammogram is in fact for screening, rather than for women at high risk or for short interval follow-up for "probably benign" findings (Raza et al., 2008). Additionally, none of the current sets of screening guidelines recommend screening more frequently than annually (US Preventive Services Task Force, 2009; National Cancer Institute, 2014) so it is unlikely that more frequent mammography is truly screening in nature. For similar reasons, I also exclude all women who have claims for breast cancer treatment prior to the screening mammogram.

For each screening mammogram, I classify it as either a true-negative[23] result, a true-positive result, or a false-positive result. I define a true-negative result as a screening mammogram with no subsequent claims for either follow-up testing or breast cancer. I define a true-positive as a screening mammogram with a subsequent claim for breast cancer. Finally, I define a false-positive as a screening mammogram followed by at least one claim for follow-up testing in the ensuing 3 months. Follow-up testing includes a subsequent mammogram (listed as either screening or diagnostic), a breast biopsy, a breast ultrasound, or other radiological breast testing (see Table A-3-5 for codes based upon Cooper et al, 1999; Warren et al., 1999; Freeman et al., 2000; Randolph et al., 2002; Tan et al., 2006; Fenton et al., 2014).

One potential concern when identifying a false-positive mammogram using a claims-based algorithm is that I do not actually observe the result of the mammogram itself. However, a recent study by Hubbard et al. (2015) using Medicare claims data linked to actual mammogram results finds 99.4 percent of false-positive mammograms identified by claims data are indeed false-positive mammograms. This is not surprising as women are unlikely to receive follow-up testing unless there is an abnormal finding in the initial screening mammogram. On the other hand, a lower but still relatively high 79.4 percent of abnormal mammograms were able to be identified using the claims algorithm. This lower percent could be a result of women who do not return for follow-up testing or for some reason are told not to return once the original scan has been re-examined. In either case, this population of women who actually have an abnormal result but are classified as true-negative results is more likely to actually bias my estimates towards zero. This is because women who are told they have abnormal result but do not return are placed in the control group and to the extent that they are more likely to initiate depression or anxiety

[23] One note is that technically it is difficult to distinguish between true-negative and false-negative results in the claims data comparing two actual mammogram films and see that there had been a growth missed. Although I refer to the results as true-negative this simply means the woman has no follow-up tests no breast cancer treatment.

medication, similar to their false-positive or true-positive counterparts, they bias my estimates downwards.

I then merge the analysis sample of women receiving a screening mammogram to the outpatient prescription drug claims files to identify any claims for depression or anxiety medications. Using the Marketscan therapeutic classes, I define depression or anxiety medications as a medication classified as an "anti-depressant", "benzodiazepines", or "anxiolytic/sedative/hypnotic" (Mark and Coffey, 2003). As an alternative analysis, I also analyze the effect of a false-positive mammogram on another set of psychiatric medications that are sometimes prescribed for anxiety, although often for other types of psychiatric conditions. These medications include "tranquilizers/anti-psychotics" and "barbiturates". The second set of medications is included as an alternative set of psychiatric medications that would be predicted to be less directly affected by a false-positive mammogram since the medications are less directly tied to depression or anxiety. For the two classes of medications, I create a series of dependent variables indicating whether the woman has initiated medication by 1, 2, …, 12 months post screening mammogram in the Medicaid analyses and 1-11[24] months in the commercially insured analyses.

I also restrict the analysis to women who do not have a claim for depression or anxiety medication prior to the index screening mammogram. Because I am focusing on medication initiation I want to ensure that they are not already taking the medication. As an alternate analysis I examine the effect of a false-positive on women who have a prior medication claim but not for at least 6 months prior to the screening mammogram.

[24] The difference in months for the two sets of analyses is due to only having two years of commercial claims data so I cannot create variables both 12 months before and after a screening mammogram.

Finally, to ensure that I am not missing any claims that might bias the results, I restrict the sample to women continuously enrolled in either the Medicaid or commercial claims data. The baseline restriction is that women must be continuously enrolled for 12 months prior to the screening mammogram to ensure they do not have a prior mammogram in the 12 months prior. However, due to limited years of data and therefore sample I relax this to include women continuously enrolled for the 9 months prior to the screening mammogram. In addition, in order to capture any depression or anxiety medication claims, I separately restrict the sample to women who are continuously enrolled for 6, 9, and 12 months after the screening mammogram for the Medicaid sample and 6, 9, and 11 months after the screening mammogram for the commercially insured sample. This slight difference is again due to the limited years of commercial claims. With only two years of commercial claims, I cannot include women both 12 months before and after a screening mammogram.

Econometrically, I estimate linear probability models with depression or anxiety medication initiation as the dependent variable for the Medicaid and commercially insured samples separately. The two independent variables of interest are then the false-positive and the true-positive variables, where true-negative is the omitted category. In addition I control for several additional individual level variables, which vary slightly depending on the data set. On the Medicaid side I control for age, race/ethnicity, reason for Medicaid eligibility (blind/disabled, adult based on unemployment, adult not based on unemployment, NBCCCEDP, and unknown), whether the plan is capitated, and whether the plans is a comprehensive Medicaid plan. On the commercially insured side I control for age, insurance type (comprehensive[25], exclusive provider organization (EPO), health maintenance organization (HMO), non-capitated point of service

[25] Namely, that there is no incentive for a patient to use a particular set of providers.

(POS), capitated point of service (POS capitated), preferred provider organization (PPO), consumer driven health plan (CDHP)), and location (see Table A-3-1). Unfortunately race/ethnicity is not available in the commercial claims. With multiple years of data, an individual may appear more than once in the sample so I cluster the standard errors by individual. As a sensitivity analysis I also run all regressions using just the first observed mammogram in the data. In addition to the baseline models, I also separate the false-positive variable into whether it was the first false-positive or second or more; I separate the false-positive variable into whether or not a biopsy was performed; and I separate the false-positive variable into the length of time it takes to resolve (within a week vs. longer and within a month vs. longer).

3.3 Data

All analyses are based on two sets of claims data—the 2003-2007 Truven Health MarketScan Medicaid Multistate Database and the 2003-2004 Truven Health MarketScan Commercial Claims and Encounters Database. The Medicaid database includes administrative claims data for approximately 7 million Medicaid recipients from 8 geographically diverse, but not identifiable states. The data are designed to be nationally representative but given the wide variation in state Medicaid programs, the representativeness may be somewhat limited. The commercial claims database collects claims and enrollment data from over 13 million commercially insured beneficiaries and their dependents. The data include employer and health plan data from a variety of types of health plans including fee-for-service, preferred provider organizations, health maintenance organizations, and others (Adamson, Chang, and Hansen, 2006). The Marketscan commercial sample is primarily composed of self-insured, medium, and large employers. Compared to the nationally representative sample of commercial health

insurance enrollees from the Medical Expenditure Panel Survey, the Marketscan commercial enrollees are generally similar demographically except for being over-representative of the southern region (Aizcorbe et al., 2012). While the similarity demonstrates the Marketscan data are generally representative of the population with employer sponsored health insurance, one cautionary note in terms of generalizability is that this is a subset of the overall population and even the population with private health insurance.

Both the Medicaid and the commercial claims data include inpatient, outpatient, and prescription drug claims in addition to enrollment and plan characteristic data as well as a limited number of demographic variables that vary slightly between the two data sets (Adamson, Chang, and Hansen, 2006). In particular, as mentioned above, the Medicaid claims data include race and ethnicity information, while the commercial claims contain geographic information. The primary advantage of the Marketscan claims data for these analyses is they are the only data set with both a large, representative population and information on all types of medical care utilization. Although Medicare claims would be another possible source of data, there are a few limitations. The first limitation is that the rate of mammography false-positive declines significantly with age (Elmore et al., 1998). The second limitation is that the Medicare population likely has a much longer history of screening and therefore more likely to have previously had a false-positive mammogram so may be less likely initiate depression or anxiety medication following a false-positive—it is estimated that between 1/3 and 1/2 of women will experience a false-positive over 8 to 10 years of screening (Elmore et al., 1998; Hubbard et al., 2011; Jacobsen et al., 2015). Finally, another advantage to using Medicaid and commercial claims therefore allows a comparison of the effect of a false-positive mammogram on depression or anxiety medication

initiation across insurance types for a similar age group. However, it would be important to see if the results hold for the Medicare population in future research.

3.4 Results

I first provide summary statistics by result of the screening mammogram—true-negative, true-positive, and false-positive. As both Table 3-1 and Table 3-2 show, there are limited differences between the three screening outcome populations. As Table 3-1 shows, there is some evidence that the Medicaid false-positive population is somewhat younger but that is consistent with the prior literature (Christiansen et al., 2000). The Medicaid false-positive population also appears to be slightly less likely to be eligible due to disability, although there is no obvious a priori reason why this might affect the results[26]. On the commercially insured side the three populations look remarkably similar.

In the baseline regressions in Table 3-3 and Table 3-4, I find significant increases in depression or anxiety medication initiation following a false-positive mammogram in both for the Medicaid and commercially insured populations. On the Medicaid side I find that a false-positive leads to approximately a 0.5 percentage point increase in the probability of initiation in the month following the false-positive up to a 1.5 percentage point increase by month 12. The increase is somewhat larger in the commercially insured than the Medicaid insured population, possibly due to better access to care in the commercially insured population (Berk and Schur, 1998 ; Kellerman and Weinick, 2012). The effect increases from an approximate 1 percentage point increase in the first month to a 4-6 percentage point increase by month 11. In both the

[26] Even if the disability population is more likely to be taking depression or anxiety medication at baseline, this would just mean they are more likely to be excluded from the analysis sample, meaning there should be limited effect on initiation following the screening mammogram itself.

Medicaid and commercially insured population most of the effects occurs in the first 7 to 9 months following the false-positive mammogram.

To put these effects in context, I find an approximately 4- to 5-fold larger increase in the effect of a true positive mammogram on depression or anxiety medication initiation. The true positive effect is consistent with other estimates from the literature that show a breast cancer diagnosis leads to significant increases in depression and anxiety (Burgess et al., 2005; Reich, Lesur, Perdrizet-Chevall, 2007). Although there is no good a priori estimate of how the effect of a false-positive on medication initiation should compare to the true-positive effect, it appears reasonable that the effect is higher than for a true-negative result but significantly lower than for a true-positive result.

Certainly, one concern with the analyses is the restriction of continuous enrollment and how attrition might affect the results. Yet Table 3-3 and Table 3-4 show this may not be a significant problem. I find the results do not change qualitatively with the differing continuous enrollment restrictions. Furthermore, when the analysis is restricted to 6 or 9 months continuous enrollment after the mammogram I see little change in the 7-11/12 or 10-11/12 month rates of initiation (i.e. the period where some of the sample has been attrited). If differential attrition were a significant problem I would expect to see much larger differences. For example, the 7-12 month results should look very different in the sample where I restrict to those continuously enrolled for 6 months compared to 9 or 12 months. Since they do not, it suggests that attrition may be less of an issue.

Another potential concern is that an individual may be observed more than once in the data and these multiply observed individuals may be differentially affecting the results. This is a slightly larger concern in the Medicaid data because of the greater number of years of data.

However as Table 3-5 and Table 3-6 show the results change very little, and if anything get slightly larger on the Medicaid side, when restricting the analyses to only the first mammogram observed.

To build on the baseline analyses, I also try to examine some factors that predict greater depression or anxiety medication initiation. I first examine if the effects are larger when the false-positive is a woman's first experience with a false-positive. If a woman has previously experienced a false-positive, the negative psychological consequences might be lower for subsequent false-positives as a woman has a better idea of what to expect and may better understand that the additional testing may ultimately rule out breast cancer. Interestingly, while this seems to be the case on the Medicaid side, there is less evidence of this on the commercially insured side.

As Table 3-7 shows, the only positive and significant increases in depression or anxiety medication initiation are for the populations experiencing a first false-positive or a true-positive. However, in the commercially insured population both the first and second false-positives seem to be associated with fairly similar sized and significant increases in medication initiation (see Table 3-8). The result is somewhat more surprising given the sample restriction that an individual cannot have previously taken depression or anxiety medication. This means that in the commercially insured population, some women initiate depression or anxiety medication following the second false-positive but not the first[27]. Given that many women believe they are at higher risk following a false-positive (Aro et al., 2000; Brett et al., 2005), it is certainly possible that anxiety increases after a second false-positive because the first time may have seemed like an anomaly while the second is additional evidence of high risk. One possibility for

[27] Given that there are only two years of commercial claims data, there are relatively few women observed to have multiple false-positive mammograms. Although the number varies depending on the specific sample restrictions, there are only at most a couple of hundred women in the commercial claims with multiple false-positive mammograms.

why the pattern might be seen in the commercially insured but not the Medicaid insured is that the Medicaid population may have lower trust in the health care system (Boulware et al., 2003; Armstrong et al., 2007) and view a subsequent abnormal result as another mistake while the commercially insured population might view it as a stronger signal that there may truly be a suspicious growth.

In addition to the number of false-positives experienced, another important factor that could lead to greater medication initiation is the invasiveness of the follow-up procedures. Splitting the false-positive variable into those that include a biopsy and those that do not, I again find different results by insurance type. On the Medicaid side in Table 3-9, I see fairly comparable effects for both biopsy and non-biopsy false-positives with each group having higher rates of initiation depending on the number of months of follow-up. However, in the commercially insured analyses, in Table 3-10, I see significantly higher rates of depression or anxiety medication initiation in the false-positive population that undergoes a breast biopsy. The rates are nearly 2 to 3 times higher for the population that undergoes a breast biopsy, with the results remaining fairly consistent at each month of follow-up. Again this could be related to different expectations or interpretations of the final results for the two different populations.

Another potential risk factor for worse psychological consequences is the length of follow-up. Interestingly, the results once again vary by insurance status. On the Medicaid side, there is not much difference in the effect if the false-positive is resolved within a week (see Table 3-11) compared to longer; and if anything the population whose false-positive is resolved in less than a month appears to have a slightly higher initiation rate than the population whose resolution is at least a month (see Table 3-13). However, I see the more commonly predicted pattern of longer resolution time leading to greater depression or anxiety medication initiation in

the commercially insured population. On the commercially insured side, in both cases—using a one week cut-point or a one month cut-point—the group with the longer false-positive resolution time has almost twice the probability of initiating depression or anxiety medication (see Table 3-12 and Table 3-14). One possible reason for the discrepancy between the Medicaid and commercially insured populations is that a greater delay in resolution may unfortunately not be as unfamiliar for the Medicaid insured population (Medicaid Access Study Group, 1994; Bisgaier and Rhodes, 2011). Being more used to delayed care may mean less anxiety about delayed results for the Medicaid population.

While a false-positive appears to have a significant effect on the extensive margin of increasing the probability of initiating depression or anxiety medication, I also examine the effect on the intensive margin. I estimate the effect of a false-positive or true-positive on the number of fills at 6, 9, and 11/12 months as well as the total amount spent on depression or anxiety medication at 6, 9, 11/12 months. I see very little effect of a false-positive on either the number of fills or the total amount spent on medication for the Medicaid population in Table 3-15. On the commercially insured side, I see a modest increase in the number of fills and a modest $15-20 increase in amount spent as a result of a false-positive (see Table 3-16). For both the Medicaid and commercially insured population, most of the effect appears to be on the extensive margin, suggesting more short-lived depression or anxiety.

As another sensitivity analysis, I estimate the baseline models using the alternative definition of depression or anxiety medications (i.e. “tranquilizers/anti-psychotics” and “barbiturates”). While the medications are psychiatric medications and some may be prescribed for depression or anxiety, there are many other unrelated psychiatric conditions for which these medications might be prescribed. Therefore, while

it is possible that I will see greater rates of initiation among women who experience a false-positive or true-positive, the effect should be smaller than in the baseline estimates. As

Table **A-3-2** and Table A-3-3 show, I see much smaller and mostly negligible effects. Although some of the effects are statistically significant they are all several orders of magnitude smaller than in the baseline regressions. Therefore, although a false-positive may lead to an increase in the initiation of these alternative medications, the effects are likely economically small.

As a final analysis, I re-run the baseline analyses but restricted to the sample of women who have a prior claim for a depression or anxiety medication but do not have a claim in the prior 6 months. However, I see little effect of a false-positive on increasing the likelihood of re-initiating depression or anxiety medication (see Table A-3-4). There is also little effect of a true-positive on re-initiation, so this may be a small and unusually selected population. Given the small sample, it is not surprising that I find little effect in this analysis so ultimately I do not focus on those results.

3.5 Discussion

This study provides the first evidence that the adverse psychological effects surrounding a false-positive mammogram actually rise to the clinical level of significantly increasing the likelihood of initiating depression or anxiety medication. This is important both in terms of demonstrating a strong negative psychological effect from a false-positive as well as increasing the costs associated with a false-positive mammogram and therefore mammography more generally. I find significantly larger effects of a false-positive on medication initiation in the commercially insured population compared to the Medicaid insured but this may in part be due

to the much higher baseline rates of depression or anxiety medication utilization in the Medicaid population. In my sample, approximately 61% of the Medicaid sample has a claim for depression or anxiety medication prior to the index mammogram, in part a reason why the Medicaid sample is even smaller than it could be. On the other hand, only about 4% of the commercially insured population has a depression or anxiety medication claim prior to the screening mammogram.

The results also suggest that there are several false-positive populations most at risk of experiencing depression or anxiety that leads to initiation of these medications. For the population with Medicaid insurance, the only factor that seems to lead to increased risk of depression or anxiety medication initiation is if the false-positive is the first one experienced. For those with private insurance, the more invasive the procedure (i.e. those who undergo a breast biopsy) and those who have a longer time to resolution are more likely to initiate depression or anxiety medication. This suggests that these subpopulations may be particularly in need of good follow-up care following a false-positive to ensure they are coping effectively and also to mitigate negative psychological consequences that may make these women less likely to screen in the future, an effect that also warrants additional research and attention.

Extrapolating from the results of this study, we can estimate the total number of women and the total cost of depression or anxiety medication initiation as a result of a false-positive mammogram. Given the high baseline rates of depression or anxiety medication utilization in the Medicaid population the total numbers are relatively small so I focus primarily on the commercially insured. There are 43.6 million women between the ages of 40 and 64 (Ong and Mendl, 2015), of whom 70.4% have private health insurance (Cohen and Martinez, 2014). The private health insurance mammography rate is 75.6% (National Center for Health Statistics, 2013) and I find an 11% false-positive rate, which is nearly identical to the one found by Ong

and Mendl (2015). This means approximately 2.5 million with private insurance experience a false-positive mammogram. I then find an approximate 5 percentage point increase in the probability of initiating depression or anxiety medication within 12 months of the mammogram, leading to a total estimate of nearly 128,000 women initiating medication as a result of a false-positive mammogram. While overall this is a relatively small percentage of women undergoing a mammogram, this is a serious adverse effect and likely significantly understates the total number of women who experience significant increases in anxiety or depression that does not rise to the level of initiating medication use. To further get a sense of the magnitude, about the same number of women between the ages of 40 and 64 will be diagnosed with breast cancer as will initiate depression or anxiety medication as a result of a false-positive (National Cancer Institute, 2015).

In the commercial claims data I find on average women with a false-positive have 2.67 fills at a total cost of $134 leading to a total cost of $17 million. However, this total cost estimate just includes women initiating depression or anxiety medication. A population particularly at risk for adverse psychological consequences would be women currently taking depression or anxiety medications, who may extend use as a result of experiencing a false-positive. In fact, I find women who have a previous depression or anxiety medication claim and then experience a false-positive mammogram on average spend $20 more in the Medicaid population and $67 more in the commercially insured population. In addition women may seek non-pharmaceutical treatment. Therefore this estimate is likely a significant underestimate of the financial consequences of the adverse psychological effects of a false-positive mammogram. Also maybe more significant than the overall cost is the 128,000 women who have such an adverse

experience that they initiate depression or anxiety medication, which may also indicate an additional risk that they may be reluctant to screen in the future.

More generally, the results suggest that there can be serious adverse psychological consequences as a result of a false-positive mammogram. Although most women tend to report an understanding that there is a risk of a false-positive, they tend to underestimate the probability (Schwartz et al., 2000) and have varying levels of tolerance for overdetection (Van den Bruel et al., 2015). In spite of this, based on survey responses, the risk of false-positive does not appear to affect of stated enthusiasm for mammography screening (Schwartz et al., 2004). However, it is important to both fully understand and communicate the potential harms of screening to women so they can make optimal individual screening decisions. Furthermore, the results further highlight that there may be particular value in best targeting screening for women based on both risk of breast cancer and preferences for screening (Pace and Keating, 2014). This is similar to the growing call for more "precision medicine" where screening can be best targeted at high risk women (Collins and Varmus, 2015), thereby potentially avoiding adverse psychological consequences in particular for low women at low risk of breast cancer. This is certainly not to say that these results suggest women should not undergo mammography, but rather that the full set of possible harms, such as false-positives, are explained. Furthermore, once a false-positive has been resolved, it is important to ensure that follow-up care is sufficient to both identify and treat women who may be suffering from clinical depression or anxiety. In particular, the results also help identify population who may be most at risk of clinical depression or anxiety so that follow-up care can be appropriately targeted at these women. This care might include further explaining the reasons for the false-positive, what future risk of cancer might be (especially when there is no elevated risk), and what additional follow-up steps might need to be taken.

3.6 Limitations

One of the major limitations for the analyses within the Medicaid population is the generalizability of the results even within the Medicaid population. The research design requires the Medicaid population to be continuously enrolled for a minimum of 15 months but more generally for over 2 years. However, there is a well-documented high rate of churn in the Medicaid population (Centers for Medicare and Medicaid Services, 2000; Short and Graefe, 2003; Duggan, 2004). This likely means the Medicaid population in this study is not representative of the overall Medicaid sample. The study sample is likely sicker and more disabled—e.g. as Table 3-1 shows the longer the population is continuously enrolled the greater the proportion of individuals who are eligible for Medicaid due to disability.

A related limitation is that the requirement of continuous enrollment and limited years of data mean I cannot be sure an individual has never previously taken depression or anxiety medication. In an alternate analysis I show that there is little effect of a false-positive on depression or anxiety medication utilization among women who have taken the medication previously but not in the prior 6 months, although there are obvious sample limitations to this. But more generally, this limitation should not be too much of a problem because a woman restarting depression or anxiety medication following a false-positive more than a year after previously taking it is an important effect of a false-positive.

Another limitation is that this study only examines the effect of a false-positive mammogram on psychiatric medications, but there could also be effects on counseling or other types of non-pharmaceutical psychological care. In addition this study only captures the effect on depression and anxiety that rises to the clinical level of medication utilization. Certainly there could remain serious and important psychological consequences that do not rise to the level of

requiring medication as previous studies using questionnaires have shown (Brett et al., 2005; Brewer, Salz, and Lillie, 2007; Hafslund and Nortvedt, 2009; Salz, Richman, and Brewer, 2010; Bond et al., 2013).

Finally, I am limited by not being able to observe the actual results of the mammogram itself. Although as previously demonstrated, the claims-based approach to identifying false-positives has been validated (Hubbard et al., 2014). Furthermore, the main limitation appears to be that some women who experience an abnormal result may not receive additional follow-up testing. To the extent that these women are more likely to initiate depression or anxiety medication but are misclassified as true-negatives in the analyses, this is only likely to bias my estimates towards finding no effect.

3.7 Conclusions

Although previous studies have shown some evidence that a false-positive mammogram may increase depression or anxiety, this is the first study to show that this rises to the clinical level of significantly increasing the probability of initiating depression or anxiety medication utilization. I also provide evidence that the commercially insured populations most at risk for initiating depression or anxiety medication are those that undergo more invasive false-positives (i.e. those involving a breast biopsy) and those with a longer time to resolution, whereas on the Medicaid side it is women experiencing their first false-positive. This suggests these are populations that should be more closely followed to ensure that they are receive appropriate follow-up care including monitoring for adverse psychological effects. More generally, the results also further add to the list of adverse effects that should be considered when determining optimal screening frequency and timing.

3.8 References

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Table 3-1 Summary statistics by mammogram outcome for the Medicaid population

	Enrolled 6 months post			Enrolled 9 months post			Enrolled 12 months post		
	True Negative	True Positive	False Positive	True Negative	True Positive	False Positive	True Negative	True Positive	False Positive
Age	51.61	53.53	50.63	51.60	53.42	50.72	51.60	53.41	50.78
	(6.99)	(6.81)	(6.87)	(6.9)	(6.7)	(6.84)	(6.78)	(6.62)	(6.75)
White	0.31	0.31	0.33	0.30	0.30	0.33	0.30	0.31	0.33
	(0.46)	(0.46)	(0.47)	(0.46)	(0.46)	(0.47)	(0.46)	(0.46)	(0.47)
Black	0.47	0.51	0.43	0.47	0.53	0.45	0.49	0.53	0.46
	(0.5)	(0.5)	(0.5)	(0.5)	(0.5)	(0.5)	(0.5)	(0.5)	(0.5)
Hispanic	0.02	0.01	0.01	0.02	0.01	0.01	0.02	0.00	0.01
	(0.14)	(0.09)	(0.12)	(0.13)	(0.08)	(0.11)	(0.14)	(0.07)	(0.11)
Other race	0.21	0.17	0.22	0.20	0.16	0.21	0.19	0.15	0.19
	(0.41)	(0.37)	(0.42)	(0.4)	(0.37)	(0.41)	(0.4)	(0.36)	(0.4)
Reason for Medicaid eligibility - blind or disabled	0.59	0.70	0.55	0.62	0.70	0.58	0.65	0.72	0.61
	(0.49)	(0.46)	(0.5)	(0.49)	(0.46)	(0.49)	(0.48)	(0.45)	(0.49)
Reason for Medicaid eligibility - adult not based on unemployment status	0.35	0.27	0.37	0.33	0.26	0.35	0.31	0.25	0.33
	(0.48)	(0.44)	(0.48)	(0.47)	(0.44)	(0.48)	(0.46)	(0.43)	(0.47)
Reason for Medicaid eligibility - unknown	0.06	0.04	0.07	0.05	0.04	0.06	0.05	0.03	0.06
	(0.23)	(0.19)	(0.26)	(0.22)	(0.19)	(0.25)	(0.21)	(0.17)	(0.23)
Reason for Medicaid eligibility - unemployed adult	0.00	-	-	0.00	-	-	0.00	-	-
	(0.01)	-	-	(0.01)	-	-	(0.01)	-	-
Reason for Medicaid eligibility - NBCCEDP	0.00	-	0.00	0.00	-	0.00	0.00	-	0.00
	(0.03)	-	(0.01)	(0.03)	-	(0.02)	(0.02)	-	(0.02)
Capitated Medicaid plan	0.52	0.51	0.52	0.52	0.51	0.52	0.51	0.50	0.51
	(0.5)	(0.5)	(0.5)	(0.5)	(0.5)	(0.5)	(0.5)	(0.5)	(0.5)
Comprehensive Medicaid plan	0.48	0.47	0.47	0.48	0.48	0.48	0.49	0.48	0.49
	(0.5)	(0.5)	(0.5)	(0.5)	(0.5)	(0.5)	(0.5)	(0.5)	(0.5)
Initiate depression/anxiety meds within 3 months	0.04	0.08	0.04	0.04	0.08	0.04	0.04	0.08	0.05
	(0.19)	(0.27)	(0.2)	(0.19)	(0.27)	(0.21)	(0.19)	(0.28)	(0.21)
Initiate depression/anxiety meds within 6 months	0.07	0.16	0.08	0.07	0.16	0.08	0.07	0.16	0.08
	(0.25)	(0.37)	(0.27)	(0.26)	(0.37)	(0.27)	(0.26)	(0.37)	(0.27)
Initiate depression/anxiety meds within 9 months	0.09	0.19	0.11	0.10	0.19	0.11	0.10	0.19	0.11
	(0.29)	(0.39)	(0.31)	(0.3)	(0.39)	(0.32)	(0.3)	(0.39)	(0.32)
Initiate depression/anxiety meds within 12 months	0.11	0.22	0.13	0.12	0.22	0.14	0.13	0.22	0.14
	(0.32)	(0.41)	(0.33)	(0.33)	(0.42)	(0.34)	(0.33)	(0.41)	(0.35)
N	39,330	528	4,968	33,082	480	4,182	27,535	436	3,507

Mean (SD) All means are for individuals enrolled for 12 months continuously prior to the index mammogram.

Table 3-2 Summary statistics by mammogram outcome for the commercially insured population

	Enrolled 6 months post			Enrolled 9 months post			Enrolled 11 months post		
	True Negative	True Positive	False Positive	True Negative	True Positive	False Positive	True Negative	True Positive	False Positive
Age	53.13	55.18	52.27	53.08	55.14	52.23	53.13	55.36	52.33
	(6.55)	(6.23)	(6.63)	(6.55)	(5.93)	(6.65)	(6.54)	(5.88)	(6.62)
Insurance - comprehensive	0.18	0.19	0.16	0.18	0.18	0.16	0.18	0.16	0.16
	(0.39)	(0.39)	(0.37)	(0.39)	(0.38)	(0.37)	(0.39)	(0.36)	(0.37)
Insurance - EPO	<0.001	-	<0.001	<0.001	-	0.00	0.00	-	<0.001
	(0.03)	-	(0.03)	(0.03)	-	(0.04)	(0.03)	-	(0.03)
Insurance- HMO	0.19	0.19	0.18	0.19	0.18	0.18	0.19	0.20	0.18
	(0.39)	(0.39)	(0.38)	(0.39)	(0.38)	(0.38)	(0.4)	(0.4)	(0.38)
Insurance - POS	0.14	0.14	0.15	0.14	0.14	0.16	0.15	0.17	0.16
	(0.35)	(0.35)	(0.36)	(0.35)	(0.35)	(0.36)	(0.35)	(0.37)	(0.37)
Insurance - POS capitated	0.02	0.02	0.03	0.02	0.02	0.03	0.02	0.03	0.03
	(0.15)	(0.14)	(0.16)	(0.15)	(0.15)	(0.16)	(0.15)	(0.16)	(0.16)
Insurance - PPO	0.46	0.46	0.47	0.45	0.47	0.47	0.45	0.44	0.47
	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)
Insurance - CDHP	<0.001	<0.001	<0.001	<0.001	<0.001	<0.001	<0.001	-	<0.001
	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	-	(0.02)
Region - Northeast	0.08	0.08	0.09	0.08	0.07	0.09	0.08	0.06	0.08
	(0.27)	(0.27)	(0.28)	(0.27)	(0.26)	(0.28)	(0.27)	(0.24)	(0.26)
Region - North Central	0.32	0.31	0.30	0.32	0.32	0.31	0.33	0.32	0.30
	(0.47)	(0.46)	(0.46)	(0.47)	(0.47)	(0.46)	(0.47)	(0.47)	(0.46)
Region - South	0.39	0.37	0.42	0.38	0.37	0.41	0.38	0.34	0.43
	(0.49)	(0.48)	(0.49)	(0.49)	(0.48)	(0.49)	(0.49)	(0.48)	(0.5)
Region - West	0.21	0.24	0.19	0.21	0.24	0.19	0.20	0.27	0.19
	(0.41)	(0.43)	(0.39)	(0.41)	(0.43)	(0.39)	(0.4)	(0.44)	(0.39)
Region - Unknown	0.01	<0.001	0.01	0.01	<0.001	<0.001	0.01	<0.001	<0.001
	(0.08)	(0.06)	(0.07)	(0.07)	(0.05)	(0.07)	(0.08)	(0.07)	(0.06)
Initiate depression/anxiety meds within 3 months	0.00	0.11	0.02	0.00	0.12	0.02	0.00	0.11	0.02
	(0.04)	(0.32)	(0.15)	(0.04)	(0.32)	(0.16)	(0.04)	(0.32)	(0.15)
Initiate depression/anxiety meds within 6 months	0.00	0.18	0.04	0.00	0.18	0.04	0.00	0.16	0.04
	(0.06)	(0.38)	(0.19)	(0.06)	(0.38)	(0.19)	(0.05)	(0.36)	(0.2)
Initiate depression/anxiety meds within 9 months	0.00	0.21	0.05	0.00	0.21	0.05	0.00	0.20	0.05
	(0.07)	(0.4)	(0.21)	(0.07)	(0.41)	(0.22)	(0.07)	(0.4)	(0.23)
Initiate depression/anxiety meds within 11 months	0.00	0.21	0.05	0.01	0.23	0.06	0.01	0.21	0.07
	(0.07)	(0.41)	(0.22)	(0.07)	(0.42)	(0.24)	(0.07)	(0.41)	(0.25)
N	229,002	1,262	20,863	112,292	671	10,291	36,082	235	3,260

Mean (SD)

All means are for individuals enrolled for 12 months continuously prior to the index mammogram.

Table 3-3 Baseline regression estimates for the Medicaid population

	1	2	3	4	5	6	7	8	9	10	11	12	N
Enrolled- 12+ months before and 6+ months after index mammogram													
False-Positive	0.004	0.006*	0.004	0.006	0.006	0.008	0.009*	0.011*	0.014**	0.015**	0.012*	0.013**	44,826
	(0.002)	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)	(0.005)	
True-Positive	0.021**	0.030**	0.044***	0.050***	0.069***	0.093***	0.094***	0.097***	0.099***	0.105***	0.105***	0.107***	
	(0.008)	(0.010)	(0.012)	(0.013)	(0.014)	(0.016)	(0.016)	(0.017)	(0.017)	(0.018)	(0.018)	(0.018)	
Enrolled- 12+ months before and 9+ months after index mammogram													
False-Positive	0.005*	0.007*	0.006	0.008*	0.008*	0.009*	0.010*	0.013**	0.016**	0.017**	0.014*	0.015**	37,744
	(0.002)	(0.003)	(0.003)	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.006)	
True-Positive	0.022**	0.034**	0.045***	0.053***	0.069***	0.090***	0.092***	0.096***	0.097***	0.104***	0.103***	0.103***	
	(0.008)	(0.011)	(0.012)	(0.014)	(0.015)	(0.017)	(0.017)	(0.018)	(0.018)	(0.019)	(0.019)	(0.019)	
Enrolled- 12+ months before and 12+ months after index mammogram													
False-Positive	0.006*	0.007*	0.006	0.006	0.006	0.006	0.008	0.011*	0.013*	0.014*	0.01	0.011	31,478
	(0.002)	(0.003)	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)	(0.005)	(0.006)	(0.006)	(0.006)	(0.006)	
True-Positive	0.019*	0.032**	0.045***	0.054***	0.069***	0.088***	0.088***	0.091***	0.091***	0.095***	0.094***	0.094***	
	(0.008)	(0.011)	(0.013)	(0.015)	(0.016)	(0.018)	(0.018)	(0.018)	(0.019)	(0.019)	(0.019)	(0.020)	
Enrolled- 9+ months before and 6+ months after index mammogram													
False-Positive	0.006**	0.008**	0.007*	0.008*	0.007*	0.009*	0.010*	0.013**	0.015***	0.016***	0.014**	0.015**	53,207
	(0.002)	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)	
True-Positive	0.021**	0.034***	0.047***	0.050***	0.062***	0.083***	0.081***	0.087***	0.089***	0.099***	0.097***	0.099***	
	(0.007)	(0.009)	(0.011)	(0.012)	(0.013)	(0.014)	(0.015)	(0.015)	(0.015)	(0.016)	(0.016)	(0.016)	
Enrolled- 9+ months before and 9+ months after index mammogram													
False-Positive	0.007***	0.009***	0.009**	0.010**	0.009*	0.010*	0.011*	0.014**	0.016***	0.017***	0.015**	0.016**	45,187
	(0.002)	(0.003)	(0.003)	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	
True-Positive	0.022**	0.036***	0.048***	0.052***	0.062***	0.081***	0.079***	0.085***	0.087***	0.097***	0.094***	0.093***	
	(0.008)	(0.010)	(0.012)	(0.013)	(0.014)	(0.015)	(0.015)	(0.016)	(0.016)	(0.017)	(0.017)	(0.017)	
Enrolled- 9+ months before and 12+ months after index mammogram													
False-Positive	0.008**	0.010**	0.009*	0.008	0.006	0.007	0.008	0.011*	0.014*	0.014*	0.011	0.012*	38,160
	(0.002)	(0.003)	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.006)	(0.006)	
True-Positive	0.019*	0.035***	0.049***	0.054***	0.062***	0.078***	0.075***	0.080***	0.079***	0.088***	0.085***	0.083***	
	(0.008)	(0.010)	(0.012)	(0.013)	(0.014)	(0.015)	(0.016)	(0.016)	(0.017)	(0.017)	(0.017)	(0.018)	

(Robust standard errors clustered by individual)
* p<0.10, ** p<0.05, *** p<0.01
All regressions also control for age, race/ethnicity, reason for Medicaid eligibility, capitation, whether plan is comprehensive.

Table 3-4 Baseline regression estimates for the commercially insured population

	Enrolled- 12+ months before and 6+ months after index mammogram											
	1	2	3	4	5	6	7	8	9	10	11	N
False-Positive	0.009*** (0.001)	0.016*** (0.001)	0.021*** (0.001)	0.025*** (0.001)	0.028*** (0.001)	0.032*** (0.001)	0.036*** (0.001)	0.039*** (0.001)	0.042*** (0.001)	0.044*** (0.001)	0.045*** (0.002)	251,127
True-Positive	0.042*** (0.006)	0.084*** (0.008)	0.111*** (0.009)	0.142*** (0.010)	0.164*** (0.011)	0.177*** (0.011)	0.190*** (0.011)	0.197*** (0.011)	0.202*** (0.011)	0.207*** (0.011)	0.209*** (0.012)	
	Enrolled- 12+ months before and 9+ months after index mammogram											
	1	2	3	4	5	6	7	8	9	10	11	N
False-Positive	0.009*** (0.001)	0.017*** (0.001)	0.023*** (0.002)	0.027*** (0.002)	0.031*** (0.002)	0.035*** (0.002)	0.040*** (0.002)	0.043*** (0.002)	0.048*** (0.002)	0.052*** (0.002)	0.054*** (0.002)	123,254
True-Positive	0.047*** (0.008)	0.088*** (0.011)	0.116*** (0.012)	0.148*** (0.014)	0.164*** (0.014)	0.176*** (0.015)	0.192*** (0.015)	0.200*** (0.016)	0.209*** (0.016)	0.219*** (0.016)	0.222*** (0.016)	
	Enrolled- 12+ months before and 11+ months after index mammogram											
	1	2	3	4	5	6	7	8	9	10	11	N
False-Positive	0.009*** (0.002)	0.017*** (0.002)	0.022*** (0.003)	0.029*** (0.003)	0.033*** (0.003)	0.037*** (0.003)	0.042*** (0.004)	0.046*** (0.004)	0.049*** (0.004)	0.056*** (0.004)	0.060*** (0.004)	39,577
True-Positive	0.046*** (0.014)	0.097*** (0.019)	0.114*** (0.021)	0.126*** (0.022)	0.142*** (0.023)	0.155*** (0.024)	0.175*** (0.025)	0.188*** (0.026)	0.196*** (0.026)	0.204*** (0.026)	0.208*** (0.027)	
	Enrolled- 9+ months before and 6+ months after index mammogram											
	1	2	3	4	5	6	7	8	9	10	11	N
False-Positive	0.009*** (0.001)	0.016*** (0.001)	0.022*** (0.001)	0.026*** (0.001)	0.030*** (0.001)	0.035*** (0.001)	0.039*** (0.001)	0.042*** (0.001)	0.045*** (0.001)	0.048*** (0.001)	0.050*** (0.001)	403,749
True-Positive	0.039*** (0.004)	0.073*** (0.005)	0.097*** (0.006)	0.123*** (0.007)	0.139*** (0.007)	0.151*** (0.007)	0.164*** (0.007)	0.171*** (0.008)	0.179*** (0.008)	0.184*** (0.008)	0.192*** (0.008)	
	Enrolled- 9+ months before and 9+ months after index mammogram											
	1	2	3	4	5	6	7	8	9	10	11	N
False-Positive	0.009*** (0.001)	0.017*** (0.001)	0.023*** (0.001)	0.027*** (0.001)	0.032*** (0.001)	0.037*** (0.001)	0.041*** (0.001)	0.045*** (0.001)	0.049*** (0.002)	0.053*** (0.002)	0.057*** (0.002)	270,423
True-Positive	0.039*** (0.005)	0.070*** (0.006)	0.094*** (0.007)	0.118*** (0.007)	0.130*** (0.008)	0.142*** (0.008)	0.156*** (0.008)	0.163*** (0.009)	0.174*** (0.009)	0.181*** (0.009)	0.190*** (0.009)	
	Enrolled- 9+ months before and 11+ months after index mammogram											
	1	2	3	4	5	6	7	8	9	10	11	N
False-Positive	0.010*** (0.001)	0.016*** (0.001)	0.022*** (0.001)	0.028*** (0.001)	0.033*** (0.002)	0.038*** (0.002)	0.042*** (0.002)	0.046*** (0.002)	0.050*** (0.002)	0.054*** (0.002)	0.059*** (0.002)	184,066
True-Positive	0.035*** (0.005)	0.065*** (0.007)	0.085*** (0.007)	0.104*** (0.008)	0.115*** (0.009)	0.126*** (0.009)	0.141*** (0.009)	0.148*** (0.009)	0.159*** (0.010)	0.165*** (0.010)	0.175*** (0.010)	

(Robust standard errors clustered by individual)
* p<0.10, ** p<0.05, *** p<0.01
All regressions also control for age, insurance plan type, and location of residence.

Table 3-5 Baseline regression estimates for the Medicaid population restricted to first observed mammogram

	1	2	3	4	5	6	7	8	9	10	11	12	N
Enrolled- 12+ months before and 6+ months after index mammogram													
False-Positive	0.005	0.008*	0.006	0.004	0.004	0.007	0.008	0.012*	0.015*	0.015*	0.012	0.012	23,092
	(0.003)	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)	(0.006)	(0.006)	(0.006)	(0.006)	(0.007)	(0.007)	
True-Positive	0.024*	0.041**	0.051**	0.052**	0.062**	0.091***	0.088***	0.097***	0.095***	0.104***	0.100***	0.106***	
	(0.012)	(0.015)	(0.017)	(0.018)	(0.020)	(0.022)	(0.022)	(0.023)	(0.023)	(0.024)	(0.024)	(0.025)	
Enrolled- 12+ months before and 9+ months after index mammogram													
False-Positive	0.008*	0.011*	0.010*	0.008	0.008	0.01	0.011	0.017*	0.019**	0.019**	0.015*	0.015*	19,741
	(0.003)	(0.004)	(0.005)	(0.005)	(0.006)	(0.006)	(0.006)	(0.007)	(0.007)	(0.007)	(0.007)	(0.008)	
True-Positive	0.023*	0.043**	0.047**	0.048**	0.057**	0.082***	0.079***	0.089***	0.086***	0.095***	0.091***	0.093***	
	(0.012)	(0.016)	(0.018)	(0.019)	(0.020)	(0.022)	(0.023)	(0.024)	(0.024)	(0.025)	(0.025)	(0.025)	
Enrolled- 12+ months before and 12+ months after index mammogram													
False-Positive	0.009*	0.011*	0.009	0.004	0.004	0.005	0.007	0.012	0.014	0.014	0.01	0.009	16,548
	(0.004)	(0.005)	(0.005)	(0.006)	(0.006)	(0.007)	(0.007)	(0.007)	(0.008)	(0.008)	(0.008)	(0.008)	
True-Positive	0.018	0.040*	0.046*	0.048*	0.050*	0.070**	0.063**	0.071**	0.068**	0.078**	0.073**	0.075**	
	(0.012)	(0.016)	(0.018)	(0.020)	(0.021)	(0.023)	(0.023)	(0.024)	(0.025)	(0.026)	(0.026)	(0.026)	
Enrolled- 9+ months before and 6+ months after index mammogram													
False-Positive	0.008**	0.010**	0.008*	0.007	0.005	0.008	0.008	0.012*	0.015**	0.015*	0.012*	0.013*	31,207
	(0.003)	(0.003)	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)	(0.005)	(0.006)	(0.006)	(0.006)	(0.006)	
True-Positive	0.022*	0.042**	0.052***	0.050**	0.052**	0.075***	0.068***	0.079***	0.079***	0.093***	0.088***	0.090***	
	(0.010)	(0.013)	(0.015)	(0.015)	(0.016)	(0.018)	(0.018)	(0.019)	(0.019)	(0.020)	(0.020)	(0.021)	
Enrolled- 9+ months before and 9+ months after index mammogram													
False-Positive	0.010***	0.013***	0.012**	0.010*	0.008	0.01	0.01	0.015*	0.017**	0.017**	0.014*	0.014*	26,974
	(0.003)	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.007)	
True-Positive	0.022*	0.042**	0.049**	0.047**	0.048**	0.068***	0.062***	0.072***	0.072***	0.084***	0.078***	0.078***	
	(0.010)	(0.013)	(0.015)	(0.016)	(0.017)	(0.018)	(0.019)	(0.020)	(0.020)	(0.021)	(0.021)	(0.021)	
Enrolled- 9+ months before and 12+ months after index mammogram													
False-Positive	0.011***	0.013**	0.011*	0.005	0.003	0.005	0.005	0.01	0.012	0.012	0.009	0.009	23,054
	(0.003)	(0.004)	(0.005)	(0.005)	(0.005)	(0.006)	(0.006)	(0.006)	(0.007)	(0.007)	(0.007)	(0.007)	
True-Positive	0.019	0.042**	0.050**	0.048**	0.045*	0.059**	0.050**	0.059**	0.057**	0.070**	0.064**	0.063**	
	(0.010)	(0.014)	(0.016)	(0.017)	(0.017)	(0.019)	(0.019)	(0.020)	(0.020)	(0.021)	(0.021)	(0.022)	

(Robust standard errors)
* p<0.10, ** p<0.05, *** p<0.01
All regressions also control for age, race/ethnicity, reason for Medicaid eligibility, capitation, whether plan is comprehensive.

Table 3-6 Baseline regression estimates for the commercially insured population restricted to first observed mammogram

	Enrolled- 12+ months before and 6+ months after index mammogram											
	1	2	3	4	5	6	7	8	9	10	11	N
False-Positive	0.009***	0.018***	0.023***	0.027***	0.031***	0.036***	0.040***	0.043***	0.046***	0.048***	0.049***	150,012
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	
True-Positive	0.040***	0.083***	0.105***	0.137***	0.162***	0.176***	0.193***	0.203***	0.206***	0.209***	0.211***	
	(0.007)	(0.010)	(0.011)	(0.012)	(0.013)	(0.013)	(0.014)	(0.014)	(0.014)	(0.014)	(0.014)	
	Enrolled- 12+ months before and 9+ months after index mammogram											
	1	2	3	4	5	6	7	8	9	10	11	N
False-Positive	0.010***	0.018***	0.024***	0.030***	0.034***	0.039***	0.044***	0.047***	0.052***	0.056***	0.058***	85,200
	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)	
True-Positive	0.038***	0.082***	0.106***	0.144***	0.163***	0.175***	0.193***	0.203***	0.208***	0.214***	0.216***	
	(0.009)	(0.012)	(0.014)	(0.016)	(0.017)	(0.017)	(0.018)	(0.018)	(0.018)	(0.019)	(0.019)	
	Enrolled- 12+ months before and 11+ months after index mammogram											
	1	2	3	4	5	6	7	8	9	10	11	N
False-Positive	0.008***	0.017***	0.022***	0.030***	0.033***	0.038***	0.042***	0.045***	0.049***	0.054***	0.059***	32,473
	(0.002)	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)	
True-Positive	0.040**	0.085***	0.105***	0.120***	0.135***	0.149***	0.174***	0.189***	0.193***	0.203***	0.208***	
	(0.014)	(0.020)	(0.022)	(0.023)	(0.024)	(0.025)	(0.027)	(0.028)	(0.028)	(0.029)	(0.029)	
	Enrolled- 9+ months before and 6+ months after index mammogram											
	1	2	3	4	5	6	7	8	9	10	11	N
False-Positive	0.010***	0.017***	0.023***	0.027***	0.032***	0.037***	0.041***	0.045***	0.048***	0.051***	0.054***	302,366
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	
True-Positive	0.038***	0.070***	0.092***	0.117***	0.132***	0.146***	0.160***	0.168***	0.176***	0.180***	0.189***	
	(0.004)	(0.006)	(0.006)	(0.007)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.009)	(0.009)	
	Enrolled- 9+ months before and 9+ months after index mammogram											
	1	2	3	4	5	6	7	8	9	10	11	N
False-Positive	0.010***	0.017***	0.023***	0.028***	0.033***	0.038***	0.043***	0.047***	0.051***	0.055***	0.058***	232,285
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	
True-Positive	0.036***	0.067***	0.088***	0.113***	0.126***	0.138***	0.153***	0.160***	0.170***	0.176***	0.185***	
	(0.005)	(0.006)	(0.007)	(0.008)	(0.008)	(0.008)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	
	Enrolled- 9+ months before and 11+ months after index mammogram											
	1	2	3	4	5	6	7	8	9	10	11	N
False-Positive	0.010***	0.016***	0.022***	0.028***	0.032***	0.038***	0.042***	0.046***	0.049***	0.054***	0.059***	176,953
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	
True-Positive	0.034***	0.062***	0.083***	0.102***	0.113***	0.125***	0.140***	0.147***	0.158***	0.163***	0.175***	
	(0.005)	(0.007)	(0.007)	(0.008)	(0.009)	(0.009)	(0.009)	(0.010)	(0.010)	(0.010)	(0.010)	

(Robust standard errors)
* p<0.10, ** p<0.05, *** p<0.01
All regressions also control for age, insurance plan type, and location of residence.

Table 3-7 Regression estimates for the Medicaid population by the number of false-positives

	1	2	3	4	5	6	7	8	9	10	11	12	N
Enrolled- 12+ months before and 6+ months after index mammogram													
False-Positive (1st)	0.005*	0.006*	0.004	0.006	0.006	0.008	0.009*	0.012**	0.015**	0.017***	0.014**	0.015**	44,826
	(0.002)	(0.003)	(0.003)	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	
False-Positive (2+)	-0.011***	0.003	0.004	0.007	0.009	0.004	-0.001	-0.003	-0.005	-0.009	-0.012	-0.004	
	(0.003)	(0.009)	(0.011)	(0.012)	(0.013)	(0.014)	(0.014)	(0.015)	(0.015)	(0.015)	(0.016)	(0.017)	
True-Positive	0.021**	0.030**	0.044***	0.050***	0.069***	0.093***	0.094***	0.097***	0.099***	0.105***	0.105***	0.107***	
	(0.008)	(0.010)	(0.012)	(0.013)	(0.014)	(0.016)	(0.016)	(0.017)	(0.017)	(0.018)	(0.018)	(0.018)	
Enrolled- 12+ months before and 9+ months after index mammogram													
	1	2	3	4	5	6	7	8	9	10	11	12	N
False-Positive (1st)	0.006*	0.007*	0.007	0.008*	0.008	0.009*	0.011*	0.014**	0.017**	0.019***	0.016**	0.016**	37,744
	(0.002)	(0.003)	(0.003)	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)	(0.005)	(0.006)	(0.006)	(0.006)	
False-Positive (2+)	-0.014***	0.002	0.004	0.011	0.011	0.007	0.002	0	-0.002	-0.006	-0.01	-0.001	
	(0.001)	(0.010)	(0.012)	(0.014)	(0.015)	(0.016)	(0.016)	(0.017)	(0.017)	(0.018)	(0.018)	(0.019)	
True-Positive	0.022**	0.034**	0.045***	0.053***	0.069***	0.090***	0.092***	0.096***	0.097***	0.104***	0.103***	0.103***	
	(0.008)	(0.011)	(0.012)	(0.014)	(0.015)	(0.017)	(0.017)	(0.018)	(0.018)	(0.019)	(0.019)	(0.019)	
Enrolled- 12+ months before and 12+ months after index mammogram													
	1	2	3	4	5	6	7	8	9	10	11	12	N
False-Positive (1st)	0.007**	0.008*	0.006	0.005	0.005	0.006	0.008	0.012*	0.014*	0.016**	0.012	0.012	31,478
	(0.003)	(0.003)	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	
False-Positive (2+)	-0.014***	0.004	0.009	0.018	0.016	0.01	0.005	0.005	0.003	-0.006	-0.009	0.002	
	(0.001)	(0.011)	(0.014)	(0.017)	(0.018)	(0.018)	(0.018)	(0.019)	(0.020)	(0.020)	(0.020)	(0.022)	
True-Positive	0.019*	0.032**	0.045***	0.054***	0.069***	0.088***	0.088***	0.091***	0.091***	0.095***	0.094***	0.094***	
	(0.008)	(0.011)	(0.013)	(0.015)	(0.016)	(0.018)	(0.018)	(0.018)	(0.019)	(0.019)	(0.019)	(0.020)	
Enrolled- 9+ months before and 6+ months after index mammogram													
	1	2	3	4	5	6	7	8	9	10	11	12	N
False-Positive (1st)	0.007***	0.008**	0.007*	0.008*	0.008*	0.010*	0.011**	0.014**	0.017***	0.018***	0.016***	0.017***	53,207
	(0.002)	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)	(0.005)	

False-Positive (2+)	-0.009*	0.004	0.003	0.006	0.006	0.001	-0.005	-0.008	-0.01	-0.015	-0.018	-0.012	
	(0.004)	(0.009)	(0.011)	(0.013)	(0.014)	(0.014)	(0.014)	(0.015)	(0.015)	(0.016)	(0.016)	(0.017)	
True-Positive	0.021**	0.034***	0.047***	0.050***	0.062***	0.083***	0.081***	0.087***	0.089***	0.099***	0.097***	0.099***	
	(0.007)	(0.009)	(0.011)	(0.012)	(0.013)	(0.014)	(0.015)	(0.015)	(0.015)	(0.016)	(0.016)	(0.016)	
	Enrolled- 9+ months before and 9+ months after index mammogram												
	1	2	3	4	5	6	7	8	9	10	11	12	N
False-Positive (1^{st})	0.009***	0.010***	0.010**	0.010**	0.009*	0.011*	0.011*	0.015**	0.018***	0.019***	0.017**	0.018***	45,187
	(0.002)	(0.003)	(0.003)	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	
False-Positive (2+)	-0.011**	0.003	0.004	0.009	0.008	0.003	-0.003	-0.005	-0.008	-0.013	-0.017	-0.009	
	(0.004)	(0.010)	(0.012)	(0.014)	(0.015)	(0.016)	(0.016)	(0.017)	(0.018)	(0.018)	(0.018)	(0.019)	
True-Positive	0.022**	0.036***	0.048***	0.052***	0.062***	0.081***	0.079***	0.085***	0.087***	0.097***	0.094***	0.093***	
	(0.008)	(0.010)	(0.012)	(0.013)	(0.014)	(0.015)	(0.015)	(0.016)	(0.016)	(0.017)	(0.017)	(0.017)	
	Enrolled- 9+ months before and 12+ months after index mammogram												
	1	2	3	4	5	6	7	8	9	10	11	12	N
False-Positive (1^{st})	0.009***	0.010**	0.009*	0.007	0.006	0.007	0.008	0.012*	0.014**	0.015**	0.012*	0.013*	38,160
	(0.003)	(0.003)	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)	(0.005)	(0.006)	(0.006)	(0.006)	
False-Positive (2+)	-0.011*	0.005	0.009	0.017	0.014	0.006	0	0	-0.002	-0.012	-0.016	-0.005	
	(0.004)	(0.012)	(0.014)	(0.017)	(0.018)	(0.018)	(0.019)	(0.020)	(0.020)	(0.020)	(0.021)	(0.022)	
True-Positive	0.019*	0.035***	0.049***	0.054***	0.062***	0.078***	0.075***	0.080***	0.079***	0.088***	0.085***	0.083***	
	(0.008)	(0.010)	(0.012)	(0.013)	(0.014)	(0.015)	(0.016)	(0.016)	(0.017)	(0.017)	(0.017)	(0.018)	

(Robust standard errors clustered by individual)
* p<0.10, ** p<0.05, *** p<0.01
All regressions also control for age, race/ethnicity, reason for Medicaid eligibility, capitation, whether plan is comprehensive.

Table 3-8 Regression estimates for the commercially insured population by the number of false-positives

	1	2	3	4	5	6	7	8	9	10	11	N
Enrolled- 12+ months before and 6+ months after index mammogram												
False-Positive (1st)	0.009***	0.016***	0.021***	0.025***	0.028***	0.032***	0.036***	0.039***	0.042***	0.044***	0.044***	251,127
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	
False-Positive (2+)	0.012**	0.019***	0.024***	0.028***	0.034***	0.038***	0.041***	0.050***	0.052***	0.054***	0.054***	
	(0.004)	(0.005)	(0.006)	(0.007)	(0.007)	(0.008)	(0.008)	(0.009)	(0.009)	(0.009)	(0.009)	
True-Positive	0.042***	0.084***	0.111***	0.142***	0.164***	0.177***	0.190***	0.197***	0.202***	0.207***	0.209***	
	(0.006)	(0.008)	(0.009)	(0.010)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)	(0.012)	
Enrolled- 12+ months before and 9+ months after index mammogram												
	1	2	3	4	5	6	7	8	9	10	11	N
False-Positive (1st)	0.009***	0.017***	0.023***	0.027***	0.031***	0.035***	0.040***	0.043***	0.048***	0.052***	0.054***	123,254
	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	
False-Positive (2+)	0.009	0.018	0.031*	0.031*	0.040**	0.039**	0.043**	0.047**	0.052**	0.056***	0.056***	
	(0.007)	(0.009)	(0.012)	(0.012)	(0.014)	(0.014)	(0.015)	(0.015)	(0.016)	(0.016)	(0.016)	
True-Positive	0.047***	0.088***	0.116***	0.148***	0.164***	0.176***	0.192***	0.200***	0.209***	0.219***	0.222***	
	(0.008)	(0.011)	(0.012)	(0.014)	(0.014)	(0.015)	(0.015)	(0.016)	(0.016)	(0.016)	(0.016)	
Enrolled- 12+ months before and 11+ months after index mammogram												
	1	2	3	4	5	6	7	8	9	10	11	N
False-Positive (1st)	0.009***	0.017***	0.022***	0.029***	0.033***	0.037***	0.042***	0.045***	0.049***	0.055***	0.060***	39,577
	(0.002)	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	
False-Positive (2+)	-0.001*	0.032	0.032	0.032	0.064	0.064	0.097	0.096	0.096	0.095	0.095	
	(0.000)	(0.033)	(0.033)	(0.033)	(0.045)	(0.045)	(0.055)	(0.055)	(0.055)	(0.055)	(0.055)	
True-Positive	0.046***	0.097***	0.114***	0.126***	0.142***	0.155***	0.175***	0.188***	0.196***	0.204***	0.208***	
	(0.014)	(0.019)	(0.021)	(0.022)	(0.023)	(0.024)	(0.025)	(0.026)	(0.026)	(0.026)	(0.027)	
Enrolled- 9+ months before and 6+ months after index mammogram												
	1	2	3	4	5	6	7	8	9	10	11	N
False-Positive (1st)	0.009***	0.016***	0.022***	0.026***	0.030***	0.035***	0.039***	0.042***	0.045***	0.048***	0.050***	403,749
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	

False-Positive (2+)	0.011**	0.019***	0.024***	0.028***	0.034***	0.038***	0.041***	0.049***	0.052***	0.053***	0.053***	
	(0.004)	(0.005)	(0.006)	(0.007)	(0.007)	(0.008)	(0.008)	(0.009)	(0.009)	(0.009)	(0.009)	
True-Positive	0.039***	0.073***	0.097***	0.123***	0.139***	0.151***	0.164***	0.171***	0.179***	0.184***	0.192***	
	(0.004)	(0.005)	(0.006)	(0.007)	(0.007)	(0.007)	(0.007)	(0.008)	(0.008)	(0.008)	(0.008)	
	Enrolled- 9+ months before and 9+ months after index mammogram											
	1	2	3	4	5	6	7	8	9	10	11	N
False-Positive (1^{st})	0.009***	0.017***	0.022***	0.027***	0.032***	0.037***	0.041***	0.045***	0.049***	0.053***	0.057***	270,423
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	
False-Positive (2+)	0.009	0.017	0.031*	0.030*	0.039**	0.039**	0.043**	0.047**	0.051**	0.055***	0.055***	
	(0.007)	(0.009)	(0.012)	(0.012)	(0.014)	(0.014)	(0.014)	(0.015)	(0.016)	(0.016)	(0.016)	
True-Positive	0.039***	0.070***	0.094***	0.118***	0.130***	0.142***	0.156***	0.163***	0.174***	0.181***	0.190***	
	(0.005)	(0.006)	(0.007)	(0.007)	(0.008)	(0.008)	(0.008)	(0.009)	(0.009)	(0.009)	(0.009)	
	Enrolled- 9+ months before and 11+ months after index mammogram											
	1	2	3	4	5	6	7	8	9	10	11	N
False-Positive (1^{st})	0.010***	0.016***	0.022***	0.028***	0.032***	0.038***	0.042***	0.046***	0.049***	0.054***	0.059***	184,066
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	
False-Positive (2+)	-0.001***	0.032	0.031	0.031	0.063	0.063	0.096	0.095	0.095	0.094	0.093	
	(0.000)	(0.033)	(0.033)	(0.033)	(0.046)	(0.046)	(0.055)	(0.055)	(0.055)	(0.055)	(0.055)	
True-Positive	0.035***	0.065***	0.085***	0.104***	0.115***	0.126***	0.141***	0.148***	0.159***	0.165***	0.175***	
	(0.005)	(0.007)	(0.007)	(0.008)	(0.009)	(0.009)	(0.009)	(0.009)	(0.010)	(0.010)	(0.010)	

(Robust standard errors clustered by individual)
* p<0.10, ** p<0.05, *** p<0.01
All regressions also control for age, insurance plan type, and location of residence.

Table 3-9 Regression estimates for the Medicaid population by use of biopsy

	Enrolled- 12+ months before and 6+ months after index mammogram												
	1	2	3	4	5	6	7	8	9	10	11	12	N
False-Positive (B)	-0.001 (0.008)	0.017 (0.014)	0.014 (0.015)	0.012 (0.016)	0.005 (0.016)	0.004 (0.017)	0.008 (0.019)	0.009 (0.019)	0.019 (0.021)	0.012 (0.021)	0.006 (0.021)	0.003 (0.021)	44,826
False-Positive (NB)	0.004 (0.002)	0.005 (0.003)	0.004 (0.003)	0.006 (0.003)	0.006 (0.004)	0.008 (0.004)	0.009* (0.004)	0.011* (0.005)	0.013** (0.005)	0.015** (0.005)	0.013* (0.005)	0.014** (0.005)	
True-Positive	0.021** (0.008)	0.030** (0.010)	0.044*** (0.012)	0.050*** (0.013)	0.069*** (0.014)	0.093*** (0.016)	0.094*** (0.016)	0.097*** (0.017)	0.099*** (0.017)	0.105*** (0.018)	0.105*** (0.018)	0.107*** (0.018)	
	Enrolled- 12+ months before and 9+ months after index mammogram												
	1	2	3	4	5	6	7	8	9	10	11	12	N
False-Positive (B)	-0.004 (0.007)	0.01 (0.014)	0.008 (0.015)	0.007 (0.017)	0.001 (0.018)	0.001 (0.019)	0.008 (0.021)	0.01 (0.022)	0.022 (0.024)	0.013 (0.024)	0.006 (0.024)	0.003 (0.024)	37,744
False-Positive (NB)	0.005* (0.002)	0.007* (0.003)	0.006 (0.003)	0.008* (0.004)	0.008* (0.004)	0.009* (0.005)	0.010* (0.005)	0.014** (0.005)	0.015** (0.005)	0.018** (0.005)	0.014** (0.006)	0.016** (0.006)	
True-Positive	0.022** (0.008)	0.034** (0.011)	0.045*** (0.012)	0.053*** (0.014)	0.069*** (0.015)	0.090*** (0.017)	0.092*** (0.017)	0.096*** (0.018)	0.097*** (0.018)	0.104*** (0.019)	0.103*** (0.019)	0.103*** (0.019)	
	Enrolled- 12+ months before and 12+ months after index mammogram												
	1	2	3	4	5	6	7	8	9	10	11	12	N
False-Positive (B)	-0.003 (0.009)	0.009 (0.015)	0.008 (0.017)	0.009 (0.019)	0.004 (0.019)	0.005 (0.021)	0.014 (0.023)	0.011 (0.024)	0.014 (0.025)	0.005 (0.025)	-0.003 (0.025)	-0.007 (0.025)	31,478
False-Positive (NB)	0.006* (0.003)	0.007* (0.003)	0.006 (0.004)	0.006 (0.004)	0.006 (0.005)	0.007 (0.005)	0.008 (0.005)	0.011* (0.006)	0.013* (0.006)	0.015* (0.006)	0.011 (0.006)	0.012 (0.006)	
True-Positive	0.019* (0.008)	0.032** (0.011)	0.045*** (0.013)	0.054*** (0.015)	0.069*** (0.016)	0.088*** (0.018)	0.088*** (0.018)	0.091*** (0.018)	0.091*** (0.019)	0.095*** (0.019)	0.094*** (0.019)	0.094*** (0.020)	
	Enrolled- 9+ months before and 6+ months after index mammogram												
	1	2	3	4	5	6	7	8	9	10	11	12	N
False-Positive (B)	0.002 (0.008)	0.018 (0.013)	0.016 (0.014)	0.015 (0.015)	0.011 (0.016)	0.011 (0.017)	0.016 (0.018)	0.015 (0.019)	0.021 (0.020)	0.013 (0.020)	0.014 (0.020)	0.017 (0.021)	53,207

False-Positive (NB)	0.006** (0.002)	0.007** (0.003)	0.007* (0.003)	0.008* (0.003)	0.007* (0.004)	0.009* (0.004)	0.009* (0.004)	0.013** (0.004)	0.015*** (0.004)	0.016*** (0.005)	0.014** (0.005)	0.015** (0.005)	
True-Positive	0.021** (0.007)	0.034*** (0.009)	0.047*** (0.011)	0.050*** (0.012)	0.062*** (0.013)	0.083*** (0.014)	0.081*** (0.015)	0.087*** (0.015)	0.089*** (0.015)	0.099*** (0.016)	0.097*** (0.016)	0.099*** (0.016)	
					Enrolled- 9+ months before and 9+ months after index mammogram								
	1	2	3	4	5	6	7	8	9	10	11	12	N
False-Positive (B)	0.001 (0.009)	0.014 (0.013)	0.013 (0.015)	0.009 (0.016)	0.006 (0.017)	0.007 (0.018)	0.015 (0.020)	0.014 (0.021)	0.022 (0.022)	0.013 (0.022)	0.014 (0.023)	0.017 (0.023)	45,187
False-Positive (NB)	0.008*** (0.002)	0.009** (0.003)	0.009** (0.003)	0.010** (0.004)	0.009* (0.004)	0.010* (0.004)	0.010* (0.004)	0.014** (0.005)	0.016** (0.005)	0.017*** (0.005)	0.015** (0.005)	0.016** (0.005)	
True-Positive	0.022** (0.008)	0.036*** (0.010)	0.048*** (0.012)	0.052*** (0.013)	0.062*** (0.014)	0.081*** (0.015)	0.079*** (0.015)	0.085*** (0.016)	0.087*** (0.016)	0.097*** (0.017)	0.094*** (0.017)	0.093*** (0.017)	
					Enrolled- 9+ months before and 12+ months after index mammogram								
	1	2	3	4	5	6	7	8	9	10	11	12	N
False-Positive (B)	0.003 (0.010)	0.014 (0.014)	0.014 (0.016)	0.011 (0.017)	0.009 (0.018)	0.012 (0.020)	0.021 (0.022)	0.017 (0.022)	0.016 (0.023)	0.006 (0.023)	0.008 (0.024)	0.011 (0.025)	38,160
False-Positive (NB)	0.008*** (0.003)	0.009** (0.003)	0.008* (0.004)	0.007 (0.004)	0.006 (0.004)	0.007 (0.005)	0.007 (0.005)	0.011* (0.005)	0.013* (0.005)	0.014* (0.006)	0.011 (0.006)	0.012* (0.006)	
True-Positive	0.019* (0.008)	0.035*** (0.010)	0.049*** (0.012)	0.054*** (0.013)	0.062*** (0.014)	0.078*** (0.015)	0.075*** (0.016)	0.080*** (0.016)	0.079*** (0.017)	0.088*** (0.017)	0.085*** (0.017)	0.083*** (0.018)	

(Robust standard errors clustered by individual)
* p<0.10, ** p<0.05, *** p<0.01
All regressions also control for age, race/ethnicity, reason for Medicaid eligibility, capitation, whether plan is comprehensive.

(B) biopsy (NB) no biopsy

Table 3-10 Regression estimates for the commercially insured population by use of biopsy

	1	2	3	4	5	6	7	8	9	10	11	N
Enrolled- 12+ months before and 6+ months after index mammogram												
False-Positive (B)	0.031***	0.055***	0.065***	0.073***	0.081***	0.086***	0.091***	0.097***	0.105***	0.108***	0.112***	251,127
	(0.005)	(0.007)	(0.007)	(0.008)	(0.008)	(0.008)	(0.008)	(0.009)	(0.009)	(0.009)	(0.009)	
False-Positive (NB)	0.007***	0.014***	0.018***	0.022***	0.025***	0.029***	0.033***	0.036***	0.038***	0.040***	0.041***	
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	
True-Positive	0.042***	0.084***	0.111***	0.142***	0.164***	0.177***	0.190***	0.197***	0.202***	0.207***	0.209***	
	(0.006)	(0.008)	(0.009)	(0.010)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)	(0.012)	
Enrolled- 12+ months before and 9+ months after index mammogram												
	1	2	3	4	5	6	7	8	9	10	11	N
False-Positive (B)	0.032***	0.053***	0.061***	0.072***	0.080***	0.083***	0.087***	0.097***	0.111***	0.117***	0.124***	123,254
	(0.007)	(0.009)	(0.010)	(0.010)	(0.011)	(0.011)	(0.011)	(0.012)	(0.013)	(0.013)	(0.013)	
False-Positive (NB)	0.007***	0.015***	0.020***	0.024***	0.028***	0.032***	0.037***	0.040***	0.044***	0.048***	0.049***	
	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	
True-Positive	0.047***	0.088***	0.116***	0.148***	0.164***	0.176***	0.192***	0.200***	0.209***	0.219***	0.222***	
	(0.008)	(0.011)	(0.012)	(0.014)	(0.014)	(0.015)	(0.015)	(0.016)	(0.016)	(0.016)	(0.016)	
Enrolled- 12+ months before and 11+ months after index mammogram												
	1	2	3	4	5	6	7	8	9	10	11	N
False-Positive (B)	0.009	0.013	0.022*	0.042**	0.061***	0.066***	0.070***	0.084***	0.104***	0.113***	0.137***	39,577
	(0.007)	(0.009)	(0.011)	(0.014)	(0.017)	(0.018)	(0.018)	(0.020)	(0.022)	(0.023)	(0.025)	
False-Positive (NB)	0.009***	0.017***	0.022***	0.028***	0.032***	0.035***	0.040***	0.043***	0.046***	0.052***	0.055***	
	(0.002)	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	
True-Positive	0.046***	0.097***	0.114***	0.126***	0.142***	0.155***	0.175***	0.188***	0.196***	0.204***	0.208***	
	(0.014)	(0.019)	(0.021)	(0.022)	(0.023)	(0.024)	(0.025)	(0.026)	(0.026)	(0.026)	(0.027)	
Enrolled- 9+ months before and 6+ months after index mammogram												
	1	2	3	4	5	6	7	8	9	10	11	N
False-Positive (B)	0.034***	0.059***	0.069***	0.077***	0.088***	0.095***	0.101***	0.110***	0.116***	0.121***	0.125***	403,749
	(0.004)	(0.005)	(0.006)	(0.006)	(0.006)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.008)	

False-Positive (NB)	0.008*** (0.001)	0.014*** (0.001)	0.019*** (0.001)	0.023*** (0.001)	0.026*** (0.001)	0.031*** (0.001)	0.035*** (0.001)	0.038*** (0.001)	0.041*** (0.001)	0.043*** (0.001)	0.045*** (0.001)	
True-Positive	0.039*** (0.004)	0.073*** (0.005)	0.097*** (0.006)	0.123*** (0.007)	0.139*** (0.007)	0.151*** (0.007)	0.164*** (0.007)	0.171*** (0.008)	0.179*** (0.008)	0.184*** (0.008)	0.192*** (0.008)	
	Enrolled- 9+ months before and 9+ months after index mammogram											
	1	2	3	4	5	6	7	8	9	10	11	N
False-Positive (B)	0.036*** (0.005)	0.060*** (0.006)	0.070*** (0.007)	0.079*** (0.007)	0.089*** (0.008)	0.096*** (0.008)	0.101*** (0.008)	0.114*** (0.009)	0.122*** (0.009)	0.129*** (0.009)	0.135*** (0.009)	270,423
False-Positive (NB)	0.008*** (0.001)	0.014*** (0.001)	0.019*** (0.001)	0.024*** (0.001)	0.028*** (0.001)	0.033*** (0.001)	0.037*** (0.001)	0.040*** (0.001)	0.044*** (0.002)	0.048*** (0.002)	0.051*** (0.002)	
True-Positive	0.039*** (0.005)	0.070*** (0.006)	0.094*** (0.007)	0.118*** (0.007)	0.130*** (0.008)	0.142*** (0.008)	0.156*** (0.008)	0.163*** (0.009)	0.174*** (0.009)	0.181*** (0.009)	0.190*** (0.009)	
	Enrolled- 9+ months before and 11+ months after index mammogram											
	1	2	3	4	5	6	7	8	9	10	11	N
False-Positive (B)	0.033*** (0.006)	0.054*** (0.008)	0.065*** (0.008)	0.076*** (0.009)	0.089*** (0.010)	0.098*** (0.010)	0.104*** (0.010)	0.119*** (0.011)	0.126*** (0.011)	0.133*** (0.011)	0.143*** (0.012)	184,066
False-Positive (NB)	0.008*** (0.001)	0.014*** (0.001)	0.019*** (0.001)	0.025*** (0.001)	0.029*** (0.001)	0.034*** (0.002)	0.038*** (0.002)	0.041*** (0.002)	0.045*** (0.002)	0.049*** (0.002)	0.053*** (0.002)	
True-Positive	0.035*** (0.005)	0.065*** (0.007)	0.085*** (0.007)	0.104*** (0.008)	0.115*** (0.009)	0.126*** (0.009)	0.141*** (0.009)	0.148*** (0.009)	0.159*** (0.010)	0.165*** (0.010)	0.175*** (0.010)	

(Robust standard errors clustered by individual)
* p<0.10, ** p<0.05, *** p<0.01
All regressions also control for age, insurance plan type, and location of residence.

(B) biopsy (NB) no biopsy

Table 3-11 Regression estimates for the Medicaid population by length of follow-up (1 week)

	Enrolled- 12+ months before and 6+ months after index mammogram												
	1	2	3	4	5	6	7	8	9	10	11	12	N
False-Positive (≥1 week)	0.004	0.006*	0.005	0.006	0.007	0.007	0.008	0.009*	0.012*	0.014**	0.011*	0.012*	44,826
	(0.002)	(0.003)	(0.003)	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	
False-Positive (<1 week)	0.001	0.002	0.003	0.006	0.004	0.011	0.014	0.025	0.029*	0.026	0.027	0.028	
	(0.005)	(0.007)	(0.008)	(0.010)	(0.010)	(0.012)	(0.012)	(0.013)	(0.014)	(0.014)	(0.014)	(0.015)	
True-Positive	0.021**	0.030**	0.044***	0.050***	0.069***	0.093***	0.094***	0.097***	0.099***	0.105***	0.105***	0.107***	
	(0.008)	(0.010)	(0.012)	(0.013)	(0.014)	(0.016)	(0.016)	(0.017)	(0.017)	(0.018)	(0.018)	(0.018)	
	Enrolled- 12+ months before and 9+ months after index mammogram												
	1	2	3	4	5	6	7	8	9	10	11	12	N
False-Positive (≥1 week)	0.005*	0.008*	0.007	0.008*	0.008	0.008	0.009	0.011*	0.013*	0.015**	0.011*	0.012*	37,744
	(0.002)	(0.003)	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)	(0.005)	(0.006)	(0.006)	(0.006)	
False-Positive (<1 week)	0.002	0.002	0.005	0.008	0.007	0.013	0.019	0.033*	0.038*	0.034*	0.036*	0.037*	
	(0.006)	(0.008)	(0.010)	(0.011)	(0.012)	(0.013)	(0.014)	(0.015)	(0.016)	(0.016)	(0.017)	(0.017)	
True-Positive	0.022**	0.034**	0.045***	0.053***	0.069***	0.090***	0.092***	0.096***	0.097***	0.104***	0.103***	0.103***	
	(0.008)	(0.011)	(0.012)	(0.014)	(0.015)	(0.017)	(0.017)	(0.018)	(0.018)	(0.019)	(0.019)	(0.019)	
	Enrolled- 12+ months before and 12+ months after index mammogram												
	1	2	3	4	5	6	7	8	9	10	11	12	N
False-Positive (≥1 week)	0.006*	0.009*	0.007	0.007	0.007	0.007	0.008	0.01	0.012*	0.013*	0.009	0.01	31,478
	(0.003)	(0.003)	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)	(0.006)	(0.006)	(0.006)	(0.006)	(0.007)	
False-Positive (<1 week)	0.002	-0.001	-0.001	-0.001	-0.002	-0.001	0.007	0.021	0.026	0.021	0.018	0.021	
	(0.007)	(0.008)	(0.010)	(0.011)	(0.012)	(0.013)	(0.014)	(0.016)	(0.017)	(0.017)	(0.017)	(0.018)	
True-Positive	0.019*	0.032**	0.045***	0.054***	0.069***	0.088***	0.088***	0.091***	0.091***	0.095***	0.094***	0.094***	
	(0.008)	(0.011)	(0.013)	(0.015)	(0.016)	(0.018)	(0.018)	(0.018)	(0.019)	(0.019)	(0.019)	(0.020)	

Enrolled- 9+ months before and 6+ months after index mammogram

	1	2	3	4	5	6	7	8	9	10	11	12	N
False-Positive (≥1 week)	0.006**	0.008**	0.007*	0.008*	0.008*	0.009*	0.009*	0.011*	0.014**	0.015**	0.012*	0.014**	53,207
	(0.002)	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)	(0.005)	
False-Positive (<1 week)	0.003	0.005	0.006	0.007	0.006	0.012	0.014	0.025	0.028*	0.027*	0.028*	0.029*	
	(0.006)	(0.007)	(0.009)	(0.010)	(0.010)	(0.011)	(0.012)	(0.013)	(0.013)	(0.014)	(0.014)	(0.014)	
True-Positive	0.021**	0.034***	0.047***	0.050***	0.062***	0.083***	0.081***	0.087***	0.089***	0.099***	0.097***	0.099***	
	(0.007)	(0.009)	(0.011)	(0.012)	(0.013)	(0.014)	(0.015)	(0.015)	(0.015)	(0.016)	(0.016)	(0.016)	

Enrolled- 9+ months before and 9+ months after index mammogram

	1	2	3	4	5	6	7	8	9	10	11	12	N
False-Positive (≥1 week)	0.008**	0.010**	0.009**	0.010**	0.009*	0.009*	0.009*	0.011*	0.013**	0.015**	0.011*	0.013*	45,187
	(0.002)	(0.003)	(0.003)	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	
False-Positive (<1 week)	0.006	0.007	0.01	0.012	0.013	0.019	0.022	0.037*	0.042**	0.040*	0.042**	0.043**	
	(0.006)	(0.008)	(0.010)	(0.011)	(0.012)	(0.013)	(0.014)	(0.015)	(0.015)	(0.016)	(0.016)	(0.017)	
True-Positive	0.022**	0.036***	0.048***	0.052***	0.062***	0.081***	0.079***	0.085***	0.087***	0.097***	0.094***	0.093***	
	(0.008)	(0.010)	(0.012)	(0.013)	(0.014)	(0.015)	(0.015)	(0.016)	(0.016)	(0.017)	(0.017)	(0.017)	

Enrolled- 9+ months before and 12+ months after index mammogram

	1	2	3	4	5	6	7	8	9	10	11	12	N
False-Positive (≥1 week)	0.008**	0.010**	0.009*	0.008*	0.007	0.007	0.007	0.01	0.012*	0.012*	0.009	0.01	38,160
	(0.003)	(0.003)	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)	(0.006)	(0.006)	(0.006)	(0.006)	
False-Positive (<1 week)	0.005	0.004	0.005	0.002	0.003	0.006	0.011	0.025	0.03	0.028	0.026	0.028	
	(0.007)	(0.009)	(0.010)	(0.011)	(0.012)	(0.013)	(0.014)	(0.016)	(0.016)	(0.017)	(0.017)	(0.018)	
True-Positive	0.019*	0.035***	0.049***	0.054***	0.062***	0.078***	0.075***	0.080***	0.079***	0.088***	0.085***	0.083***	
	(0.008)	(0.010)	(0.012)	(0.013)	(0.014)	(0.015)	(0.016)	(0.016)	(0.017)	(0.017)	(0.017)	(0.018)	

(Robust standard errors clustered by individual)
* p<0.10, ** p<0.05, *** p<0.01
All regressions also control for age, race/ethnicity, reason for Medicaid eligibility, capitation, whether plan is comprehensive.

Table 3-12 Regression estimates for the commercially insured population by length of follow-up (1 week)

Enrolled- 12+ months before and 6+ months after index mammogram												
	1	2	3	4	5	6	7	8	9	10	11	N
False-Positive (≥1 week)	0.010*** (0.001)	0.018*** (0.001)	0.023*** (0.001)	0.027*** (0.001)	0.031*** (0.001)	0.036*** (0.001)	0.040*** (0.002)	0.043*** (0.002)	0.046*** (0.002)	0.048*** (0.002)	0.049*** (0.002)	251,177
False-Positive (<1 week)	0.004*** (0.001)	0.008*** (0.002)	0.011*** (0.002)	0.013*** (0.002)	0.014*** (0.002)	0.016*** (0.002)	0.020*** (0.002)	0.021*** (0.003)	0.023*** (0.003)	0.025*** (0.003)	0.025*** (0.003)	
True-Positive	0.042*** (0.006)	0.083*** (0.008)	0.110*** (0.009)	0.141*** (0.010)	0.163*** (0.010)	0.176*** (0.011)	0.189*** (0.011)	0.196*** (0.011)	0.201*** (0.011)	0.206*** (0.011)	0.208*** (0.011)	
Enrolled- 12+ months before and 9+ months after index mammogram												
	1	2	3	4	5	6	7	8	9	10	11	N
False-Positive (≥1 week)	0.010*** (0.001)	0.019*** (0.002)	0.025*** (0.002)	0.029*** (0.002)	0.034*** (0.002)	0.039*** (0.002)	0.043*** (0.002)	0.048*** (0.002)	0.053*** (0.003)	0.057*** (0.003)	0.059*** (0.003)	123,280
False-Positive (<1 week)	0.005** (0.002)	0.009*** (0.002)	0.013*** (0.003)	0.017*** (0.003)	0.018*** (0.003)	0.019*** (0.003)	0.023*** (0.004)	0.024*** (0.004)	0.028*** (0.004)	0.031*** (0.004)	0.031*** (0.004)	
True-Positive	0.047*** (0.008)	0.088*** (0.011)	0.116*** (0.012)	0.148*** (0.014)	0.164*** (0.014)	0.175*** (0.015)	0.191*** (0.015)	0.199*** (0.016)	0.208*** (0.016)	0.218*** (0.016)	0.221*** (0.016)	
Enrolled- 12+ months before and 11+ months after index mammogram												
	1	2	3	4	5	6	7	8	9	10	11	N
False-Positive (≥1 week)	0.010*** (0.002)	0.018*** (0.003)	0.023*** (0.003)	0.030*** (0.003)	0.035*** (0.004)	0.040*** (0.004)	0.045*** (0.004)	0.049*** (0.004)	0.052*** (0.004)	0.059*** (0.005)	0.064*** (0.005)	39,579
False-Positive (<1 week)	0.004 (0.003)	0.012* (0.005)	0.017** (0.006)	0.025*** (0.007)	0.027*** (0.007)	0.026*** (0.007)	0.031*** (0.008)	0.032*** (0.008)	0.035*** (0.008)	0.042*** (0.009)	0.042*** (0.009)	
True-Positive	0.046*** (0.014)	0.096*** (0.019)	0.113*** (0.021)	0.125*** (0.022)	0.141*** (0.023)	0.154*** (0.024)	0.174*** (0.025)	0.186*** (0.025)	0.194*** (0.026)	0.202*** (0.026)	0.206*** (0.026)	
Enrolled- 9+ months before and 6+ months after index mammogram												
	1	2	3	4	5	6	7	8	9	10	11	N
False-Positive (≥1 week)	0.010*** (0.001)	0.018*** (0.001)	0.024*** (0.001)	0.029*** (0.001)	0.033*** (0.001)	0.039*** (0.001)	0.043*** (0.001)	0.046*** (0.001)	0.049*** (0.001)	0.052*** (0.001)	0.055*** (0.001)	403,851

False-Positive (<1 week)	0.004*** (0.001)	0.007*** (0.001)	0.010*** (0.001)	0.012*** (0.002)	0.014*** (0.002)	0.017*** (0.002)	0.020*** (0.002)	0.023*** (0.002)	0.025*** (0.002)	0.027*** (0.002)	0.029*** (0.002)	
True-Positive	0.039*** (0.004)	0.073*** (0.005)	0.097*** (0.006)	0.122*** (0.007)	0.138*** (0.007)	0.150*** (0.007)	0.163*** (0.007)	0.171*** (0.008)	0.179*** (0.008)	0.184*** (0.008)	0.191*** (0.008)	
	Enrolled- 9+ months before and 9+ months after index mammogram											
	1	2	3	4	5	6	7	8	9	10	11	N
False-Positive (≥1 week)	0.010*** (0.001)	0.019*** (0.001)	0.025*** (0.001)	0.030*** (0.001)	0.035*** (0.001)	0.040*** (0.002)	0.045*** (0.002)	0.049*** (0.002)	0.053*** (0.002)	0.058*** (0.002)	0.061*** (0.002)	270,500
False-Positive (<1 week)	0.004*** (0.001)	0.008*** (0.002)	0.010*** (0.002)	0.013*** (0.002)	0.015*** (0.002)	0.019*** (0.002)	0.022*** (0.003)	0.025*** (0.003)	0.028*** (0.003)	0.031*** (0.003)	0.033*** (0.003)	
True-Positive	0.039*** (0.004)	0.070*** (0.006)	0.093*** (0.007)	0.118*** (0.007)	0.130*** (0.008)	0.141*** (0.008)	0.156*** (0.008)	0.163*** (0.009)	0.174*** (0.009)	0.180*** (0.009)	0.190*** (0.009)	

(Robust standard errors clustered by individual)
* p<0.10, ** p<0.05, *** p<0.01
All regressions also control for age, insurance plan type, and location of residence.

Table 3-13 Regression estimates for the Medicaid population by length of follow-up (1 month)

	Enrolled- 12+ months before and 6+ months after index mammogram												
	1	2	3	4	5	6	7	8	9	10	11	12	N
False-Positive (≥1 month)	0.002 (0.003)	0.004 (0.004)	0.005 (0.005)	0.004 (0.005)	0.002 (0.006)	0.002 (0.006)	0.002 (0.006)	0.005 (0.007)	0.005 (0.007)	0.006 (0.007)	0.002 (0.007)	0.002 (0.008)	44,826
False-Positive (<1 month)	0.004 (0.002)	0.006 (0.003)	0.004 (0.004)	0.007 (0.004)	0.008 (0.005)	0.011* (0.005)	0.012* (0.005)	0.015** (0.006)	0.018** (0.006)	0.020*** (0.006)	0.018** (0.006)	0.020** (0.006)	
True-Positive	0.021** (0.008)	0.030** (0.010)	0.044*** (0.012)	0.050*** (0.013)	0.069*** (0.014)	0.093*** (0.016)	0.094*** (0.016)	0.097*** (0.017)	0.099*** (0.017)	0.105*** (0.018)	0.105*** (0.018)	0.107*** (0.018)	
	Enrolled- 12+ months before and 9+ months after index mammogram												
	1	2	3	4	5	6	7	8	9	10	11	12	N
False-Positive (≥1 month)	0.003 (0.003)	0.004 (0.005)	0.006 (0.005)	0.005 (0.006)	0.003 (0.006)	0.002 (0.007)	0.003 (0.007)	0.007 (0.008)	0.006 (0.008)	0.006 (0.008)	0.002 (0.008)	0.002 (0.009)	37,744
False-Positive (<1 month)	0.006* (0.003)	0.009* (0.004)	0.007 (0.004)	0.010* (0.005)	0.011* (0.005)	0.012* (0.006)	0.014* (0.006)	0.017** (0.006)	0.021** (0.006)	0.023*** (0.007)	0.020** (0.007)	0.023** (0.007)	
True-Positive	0.022** (0.008)	0.034** (0.011)	0.045*** (0.012)	0.053*** (0.014)	0.069*** (0.015)	0.090*** (0.017)	0.092*** (0.017)	0.096*** (0.018)	0.097*** (0.018)	0.104*** (0.019)	0.103*** (0.019)	0.103*** (0.019)	
	Enrolled- 12+ months before and 12+ months after index mammogram												
	1	2	3	4	5	6	7	8	9	10	11	12	N
False-Positive (≥1 month)	0.004 (0.004)	0.005 (0.005)	0.007 (0.006)	0.006 (0.007)	0.005 (0.007)	0.004 (0.008)	0.005 (0.008)	0.008 (0.009)	0.007 (0.009)	0.007 (0.009)	0.002 (0.009)	0.001 (0.010)	31,478
False-Positive (<1 month)	0.007* (0.003)	0.009* (0.004)	0.005 (0.005)	0.006 (0.005)	0.007 (0.006)	0.008 (0.006)	0.01 (0.006)	0.013 (0.007)	0.017* (0.007)	0.018* (0.007)	0.014 (0.007)	0.017* (0.008)	
True-Positive	0.019* (0.008)	0.032** (0.011)	0.045*** (0.013)	0.054*** (0.015)	0.069*** (0.016)	0.088*** (0.018)	0.088*** (0.018)	0.091*** (0.018)	0.091*** (0.019)	0.095*** (0.019)	0.094*** (0.019)	0.094*** (0.020)	

	Enrolled- 9+ months before and 6+ months after index mammogram												
	1	2	3	4	5	6	7	8	9	10	11	12	N
False-Positive (≥1 month)	0.005 (0.003)	0.007 (0.004)	0.007 (0.005)	0.005 (0.005)	0.004 (0.005)	0.003 (0.006)	0.002 (0.006)	0.005 (0.006)	0.005 (0.007)	0.004 (0.007)	0.001 (0.007)	0.004 (0.007)	53,207
False-Positive (<1 month)	0.007** (0.002)	0.008** (0.003)	0.007 (0.004)	0.010* (0.004)	0.009* (0.004)	0.013** (0.005)	0.014** (0.005)	0.017** (0.005)	0.021*** (0.006)	0.023*** (0.006)	0.021*** (0.006)	0.022*** (0.006)	
True-Positive	0.021** (0.007)	0.034*** (0.009)	0.047*** (0.011)	0.050*** (0.012)	0.062*** (0.013)	0.083*** (0.014)	0.081*** (0.015)	0.087*** (0.015)	0.089*** (0.015)	0.099*** (0.016)	0.097*** (0.016)	0.099*** (0.016)	
	Enrolled- 9+ months before and 9+ months after index mammogram												
	1	2	3	4	5	6	7	8	9	10	11	12	N
False-Positive (≥1 month)	0.006 (0.003)	0.007 (0.004)	0.008 (0.005)	0.005 (0.006)	0.004 (0.006)	0.002 (0.006)	0.002 (0.007)	0.005 (0.007)	0.004 (0.007)	0.003 (0.008)	-0.001 (0.008)	0.003 (0.008)	45,187
False-Positive (<1 month)	0.008** (0.003)	0.011** (0.004)	0.010* (0.004)	0.013** (0.005)	0.012* (0.005)	0.015** (0.005)	0.016** (0.006)	0.019** (0.006)	0.023*** (0.006)	0.025*** (0.006)	0.023*** (0.006)	0.024*** (0.007)	
True-Positive	0.022** (0.008)	0.036*** (0.010)	0.048*** (0.012)	0.052*** (0.013)	0.062*** (0.014)	0.081*** (0.015)	0.079*** (0.015)	0.085*** (0.016)	0.087*** (0.016)	0.097*** (0.017)	0.094*** (0.017)	0.093*** (0.017)	
	Enrolled- 9+ months before and 12+ months after index mammogram												
	1	2	3	4	5	6	7	8	9	10	11	12	N
False-Positive (≥1 month)	0.007 (0.004)	0.008 (0.005)	0.009 (0.006)	0.006 (0.006)	0.005 (0.007)	0.004 (0.007)	0.004 (0.007)	0.007 (0.008)	0.006 (0.008)	0.004 (0.008)	0.001 (0.008)	0.004 (0.009)	38,160
False-Positive (<1 month)	0.009** (0.003)	0.011** (0.004)	0.009 (0.004)	0.009 (0.005)	0.007 (0.005)	0.009 (0.006)	0.01 (0.006)	0.014* (0.006)	0.018** (0.007)	0.019** (0.007)	0.017* (0.007)	0.017* (0.007)	
True-Positive	0.019* (0.008)	0.035*** (0.010)	0.049*** (0.012)	0.054*** (0.013)	0.062*** (0.014)	0.078*** (0.015)	0.075*** (0.016)	0.080*** (0.016)	0.079*** (0.017)	0.088*** (0.017)	0.085*** (0.017)	0.083*** (0.018)	

(Robust standard errors clustered by individual)
* p<0.10, ** p<0.05, *** p<0.01
All regressions also control for age, race/ethnicity, reason for Medicaid eligibility, capitation, whether plan is comprehensive.

Table 3-14 Regression estimates for the commercially insured population by length of follow-up (1 month)

	1	2	3	4	5	6	7	8	9	10	11	N
Enrolled- 12+ months before and 6+ months after index mammogram												
False-Positive (≥1 month)	0.014*** (0.002)	0.032*** (0.003)	0.041*** (0.003)	0.048*** (0.003)	0.054*** (0.004)	0.059*** (0.004)	0.062*** (0.004)	0.066*** (0.004)	0.070*** (0.004)	0.071*** (0.004)	0.072*** (0.004)	251,177
False-Positive (<1 month)	0.007*** (0.001)	0.012*** (0.001)	0.016*** (0.001)	0.019*** (0.001)	0.022*** (0.001)	0.026*** (0.001)	0.030*** (0.001)	0.033*** (0.001)	0.035*** (0.002)	0.037*** (0.002)	0.038*** (0.002)	
True-Positive	0.042*** (0.006)	0.083*** (0.008)	0.110*** (0.009)	0.141*** (0.010)	0.163*** (0.010)	0.176*** (0.011)	0.189*** (0.011)	0.196*** (0.011)	0.201*** (0.011)	0.206*** (0.011)	0.208*** (0.011)	
Enrolled- 12+ months before and 9+ months after index mammogram												
	1	2	3	4	5	6	7	8	9	10	11	N
False-Positive (≥1 month)	0.013*** (0.003)	0.031*** (0.004)	0.041*** (0.005)	0.048*** (0.005)	0.055*** (0.005)	0.060*** (0.006)	0.064*** (0.006)	0.070*** (0.006)	0.076*** (0.006)	0.079*** (0.006)	0.082*** (0.006)	123,280
False-Positive (<1 month)	0.008*** (0.001)	0.014*** (0.001)	0.019*** (0.002)	0.022*** (0.002)	0.025*** (0.002)	0.029*** (0.002)	0.034*** (0.002)	0.037*** (0.002)	0.042*** (0.002)	0.046*** (0.002)	0.048*** (0.002)	
True-Positive	0.047*** (0.008)	0.088*** (0.011)	0.116*** (0.012)	0.148*** (0.014)	0.164*** (0.014)	0.175*** (0.015)	0.191*** (0.015)	0.199*** (0.016)	0.208*** (0.016)	0.218*** (0.016)	0.221*** (0.016)	
Enrolled- 12+ months before and 11+ months after index mammogram												
	1	2	3	4	5	6	7	8	9	10	11	N
False-Positive (≥1 month)	0.014** (0.005)	0.024*** (0.006)	0.032*** (0.007)	0.045*** (0.009)	0.051*** (0.009)	0.052*** (0.009)	0.053*** (0.009)	0.057*** (0.010)	0.059*** (0.010)	0.061*** (0.010)	0.069*** (0.011)	39,579
False-Positive (<1 month)	0.007*** (0.002)	0.015*** (0.002)	0.020*** (0.003)	0.025*** (0.003)	0.029*** (0.003)	0.034*** (0.004)	0.040*** (0.004)	0.043*** (0.004)	0.047*** (0.004)	0.054*** (0.005)	0.058*** (0.005)	
True-Positive	0.046*** (0.014)	0.096*** (0.019)	0.113*** (0.021)	0.125*** (0.022)	0.141*** (0.023)	0.154*** (0.024)	0.174*** (0.025)	0.186*** (0.025)	0.194*** (0.026)	0.202*** (0.026)	0.206*** (0.026)	

	Enrolled- 9+ months before and 6+ months after index mammogram											
	1	2	3	4	5	6	7	8	9	10	11	N
False-Positive (≥1 month)	0.015*** (0.002)	0.032*** (0.002)	0.041*** (0.002)	0.049*** (0.003)	0.056*** (0.003)	0.062*** (0.003)	0.066*** (0.003)	0.070*** (0.003)	0.073*** (0.003)	0.076*** (0.003)	0.080*** (0.003)	403,851
False-Positive (<1 month)	0.008*** (0.001)	0.012*** (0.001)	0.017*** (0.001)	0.020*** (0.001)	0.024*** (0.001)	0.028*** (0.001)	0.032*** (0.001)	0.035*** (0.001)	0.038*** (0.001)	0.041*** (0.001)	0.043*** (0.001)	
True-Positive	0.039*** (0.004)	0.073*** (0.005)	0.097*** (0.006)	0.122*** (0.007)	0.138*** (0.007)	0.150*** (0.007)	0.163*** (0.007)	0.171*** (0.008)	0.179*** (0.008)	0.184*** (0.008)	0.191*** (0.008)	
	Enrolled- 9+ months before and 9+ months after index mammogram											
	1	2	3	4	5	6	7	8	9	10	11	N
False-Positive (≥1 month)	0.014*** (0.002)	0.031*** (0.003)	0.041*** (0.003)	0.050*** (0.003)	0.057*** (0.004)	0.063*** (0.004)	0.067*** (0.004)	0.072*** (0.004)	0.076*** (0.004)	0.081*** (0.004)	0.086*** (0.004)	270,500
False-Positive (<1 month)	0.008*** (0.001)	0.013*** (0.001)	0.018*** (0.001)	0.021*** (0.001)	0.025*** (0.001)	0.030*** (0.001)	0.034*** (0.001)	0.038*** (0.002)	0.042*** (0.002)	0.046*** (0.002)	0.049*** (0.002)	
True-Positive	0.039*** (0.004)	0.070*** (0.006)	0.093*** (0.007)	0.118*** (0.007)	0.130*** (0.008)	0.141*** (0.008)	0.156*** (0.008)	0.163*** (0.009)	0.174*** (0.009)	0.180*** (0.009)	0.190*** (0.009)	
	Enrolled- 9+ months before and 11+ months after index mammogram											
	1	2	3	4	5	6	7	8	9	10	11	N
False-Positive (≥1 month)	0.016*** (0.002)	0.030*** (0.003)	0.040*** (0.004)	0.051*** (0.004)	0.057*** (0.004)	0.064*** (0.004)	0.068*** (0.005)	0.070*** (0.005)	0.074*** (0.005)	0.078*** (0.005)	0.085*** (0.005)	184,121
False-Positive (<1 month)	0.008*** (0.001)	0.013*** (0.001)	0.017*** (0.001)	0.021*** (0.001)	0.026*** (0.002)	0.031*** (0.002)	0.035*** (0.002)	0.039*** (0.002)	0.043*** (0.002)	0.048*** (0.002)	0.052*** (0.002)	
True-Positive	0.035*** (0.005)	0.064*** (0.007)	0.085*** (0.007)	0.104*** (0.008)	0.115*** (0.008)	0.126*** (0.009)	0.141*** (0.009)	0.148*** (0.009)	0.159*** (0.010)	0.164*** (0.010)	0.175*** (0.010)	

(Robust standard errors clustered by individual)
* p<0.10, ** p<0.05, *** p<0.01
All regressions also control for age, insurance plan type, and location of residence.

Table 3-15 Cost and number of fills estimates for the Medicaid population

Continuously enrolled 12+ months before

	Fills			Costs		
	6 months	9 months	12 months	6 months	9 months	12 months
False-Positive	0.003	-0.036	0.102	-8.090*	-7.084	-3.385
	(0.105)	(0.125)	(0.153)	(3.965)	(5.277)	(6.159)
True-Positive	-0.254	0.051	0.429	-9.789	-5.868	0.954
	(0.141)	(0.214)	(0.314)	(7.307)	(10.085)	(14.524)
N	3,654	4,094	4,229	3,654	4,094	4,229

Continuously enrolled 9+ months before

	Fills			Costs		
	6 months	9 months	12 months	6 months	9 months	12 months
False-Positive	0.067	0.015	0.153	-4.481	-3.26	-2.1
	(0.090)	(0.110)	(0.135)	(4.099)	(5.363)	(5.462)
True-Positive	-0.072	0.185	0.464	-8.425	-7.383	-1.823
	(0.157)	(0.225)	(0.296)	(6.711)	(8.838)	(12.027)
N	4,784	5,371	5,568	4,784	5,371	5,568

(Robust standard errors clustered by individual)
* p<0.10, ** p<0.05, *** p<0.01
All regressions also control for age, race/ethnicity, reason for Medicaid eligibility, capitation, whether plan is comprehensive.

Table 3-16 Cost and number of fills estimates for the commercially insured population

Continuously enrolled 12+ months before

	Fills			Costs		
	6 months	9 months	11 months	6 months	9 months	11 months
False-Positive	0.022	0.089	-0.162	4.745	20.619	19.099
	(0.084)	(0.147)	(0.291)	(7.103)	(11.299)	(21.858)
True-Positive	0.428**	0.673*	0.736	-3.64	27.028	48.139
	(0.137)	(0.278)	(0.545)	(9.835)	(19.862)	(44.969)
N	1,740	1,205	452	1,740	1.205	452

Continuously enrolled 9+ months before

	Fills			Costs		
	6 months	9 months	11 months	6 months	9 months	11 months
False-Positive	0.031	0.132	0.135	3.47	15.849*	17.182
	(0.061)	(0.093)	(0.114)	(5.355)	(7.493)	(9.671)
True-Positive	0.488***	0.657***	0.790***	2.426	28.005*	42.348**
	(0.103)	(0.166)	(0.209)	(7.513)	(12.119)	(16.052)
N	3,026	2,828	2,309	3,026	2,828	2,309

(Robust standard errors clustered by individual)
* p<0.10, ** p<0.05, *** p<0.01
All regressions also control for age, insurance plan type, and location of residence.

Appendix Tables

Table A-3-1 Location summary statistics for the commercially insured population

	Enrolled 6 months post			Enrolled 9 months post			Enrolled 12 months post		
Location	True Negative	True Positive	False Positive	True Negative	True Positive	False Positive	True Negative	True Positive	False Positive
Connecticut	0.00 (0.07)	0.00 (0.07)	0.00 (0.06)	0.01 (0.07)	0.00 (0.07)	0.00 (0.07)	0.00 (0.07)	0.01 (0.08)	0.00 (0.06)
Maine	0.00 (0.05)	0.00 (0.05)	0.00 (0.03)	0.00 (0.04)	0.00 (0.05)	0.00 (0.05)	0.00 (0)	0.00 (0.03)	0.00 (0.04)
Massachusetts	0.01 (0.1)	0.01 (0.1)	0.01 (0.1)	0.01 (0.11)	0.01 (0.11)	0.01 (0.11)	0.01 (0.11)	0.01 (0.12)	0.01 (0.11)
New Hampshire	0.00 (0.05)	0.00 (0.05)	0.00 (0.03)	0.00 (0.06)	0.00 (0.06)	0.00 (0.06)	0.00 (0)	0.00 (0.06)	0.00 (0.06)
Rhode Island	0.00 (0.04)	0.00 (0.04)	0.00 (0.05)	0.00 (0.03)	0.00 (0.04)	0.00 (0.04)	0.00 (0)	0.00 (0.03)	0.00 (0.04)
Vermont	0.00 (0.04)	0.00 (0.04)	0.00 (0)	0.00 (0.03)	0.00 (0.04)	0.00 (0.04)	0.00 (0)	0.00 (0.03)	0.00 (0.03)
New Jersey	0.02 (0.12)	0.01 (0.12)	0.02 (0.15)	0.02 (0.14)	0.02 (0.12)	0.01 (0.12)	0.02 (0.15)	0.02 (0.14)	0.01 (0.12)
New York	0.02 (0.14)	0.02 (0.14)	0.02 (0.13)	0.02 (0.13)	0.02 (0.14)	0.02 (0.14)	0.02 (0.13)	0.02 (0.13)	0.02 (0.14)
Pennsylvania	0.02 (0.15)	0.02 (0.15)	0.02 (0.14)	0.02 (0.15)	0.02 (0.15)	0.02 (0.15)	0.01 (0.12)	0.02 (0.15)	0.02 (0.14)
Illinois	0.04 (0.18)	0.03 (0.18)	0.03 (0.17)	0.04 (0.2)	0.04 (0.19)	0.04 (0.18)	0.03 (0.17)	0.04 (0.2)	0.04 (0.19)
Indiana	0.03 (0.17)	0.03 (0.17)	0.02 (0.15)	0.03 (0.17)	0.03 (0.17)	0.03 (0.17)	0.02 (0.14)	0.03 (0.17)	0.03 (0.17)
Michigan	0.14 (0.35)	0.14 (0.35)	0.13 (0.34)	0.12 (0.33)	0.14 (0.35)	0.14 (0.35)	0.13 (0.34)	0.12 (0.33)	0.14 (0.35)
Ohio	0.05 (0.22)	0.05 (0.22)	0.07 (0.25)	0.05 (0.22)	0.05 (0.22)	0.05 (0.22)	0.07 (0.26)	0.05 (0.23)	0.05 (0.22)
Wisconsin	0.01 (0.08)	0.01 (0.08)	0.01 (0.07)	0.01 (0.09)	0.01 (0.08)	0.01 (0.08)	0.00 (0.05)	0.01 (0.09)	0.01 (0.08)
Iowa	0.00 (0.06)	0.00 (0.06)	0.00 (0.04)	0.00 (0.07)	0.00 (0.06)	0.00 (0.06)	0.00 (0.05)	0.00 (0.07)	0.00 (0.07)
Kansas	0.02 (0.13)	0.02 (0.13)	0.02 (0.13)	0.02 (0.12)	0.02 (0.13)	0.02 (0.13)	0.02 (0.13)	0.01 (0.12)	0.02 (0.13)
Minnesota	0.00 (0.06)	0.00 (0.06)	0.00 (0.06)	0.00 (0.06)	0.00 (0.06)	0.00 (0.06)	0.01 (0.08)	0.00 (0.06)	0.00 (0.06)
Missouri	0.03 (0.17)	0.03 (0.17)	0.03 (0.17)	0.02 (0.15)	0.03 (0.17)	0.03 (0.17)	0.03 (0.17)	0.02 (0.15)	0.03 (0.17)
Nebraska	0.00 (0.04)	0.00 (0.04)	0.00 (0.05)	0.00 (0.04)	0.00 (0.04)	0.00 (0.04)	0.00 (0)	0.00 (0.03)	0.00 (0.04)

	Enrolled 6 months post			Enrolled 9 months post			Enrolled 12 months post		
Location	True Negative	True Positive	False Positive	True Negative	True Positive	False Positive	True Negative	True Positive	False Positive
North Dakota	0.00 (0.02)	0.00 (0.02)	0.00 (0)	0.00 (0.02)	0.00 (0.02)	0.00 (0.02)	0.00 (0)	0.00 (0.02)	0.00 (0.02)
South Dakota	0.00 (0.02)	0.00 (0.02)	0.00 (0)	0.00 (0.03)	0.00 (0.02)	0.00 (0.02)	0.00 (0)	0.00 (0.03)	0.00 (0.03)
Washington, DC	0.00 (0.01)	0.00 (0.01)	0.00 (0)	0.00 (0.01)	0.00 (0.01)	0.00 (0.01)	0.00 (0)	0.00 (0.01)	0.00 (0.01)
Delaware	0.00 (0.04)	0.00 (0.04)	0.00 (0.03)	0.00 (0.03)	0.00 (0.04)	0.00 (0.04)	0.00 (0.04)	0.00 (0.03)	0.00 (0.04)
Florida	0.04 (0.18)	0.03 (0.18)	0.03 (0.17)	0.04 (0.21)	0.03 (0.18)	0.03 (0.18)	0.02 (0.13)	0.04 (0.2)	0.03 (0.18)
Georgia	0.10 (0.3)	0.10 (0.3)	0.12 (0.33)	0.11 (0.31)	0.10 (0.3)	0.10 (0.3)	0.13 (0.34)	0.10 (0.31)	0.10 (0.3)
Maryland	0.01 (0.1)	0.01 (0.1)	0.01 (0.09)	0.01 (0.11)	0.01 (0.1)	0.01 (0.1)	0.01 (0.11)	0.01 (0.1)	0.01 (0.09)
North Carolina	0.02 (0.14)	0.02 (0.14)	0.01 (0.11)	0.02 (0.14)	0.02 (0.14)	0.02 (0.14)	0.01 (0.11)	0.02 (0.13)	0.02 (0.13)
South Carolina	0.01 (0.09)	0.01 (0.09)	0.00 (0.07)	0.01 (0.1)	0.01 (0.09)	0.01 (0.09)	0.01 (0.08)	0.01 (0.1)	0.01 (0.09)
Virginia	0.01 (0.12)	0.01 (0.12)	0.01 (0.11)	0.01 (0.12)	0.01 (0.11)	0.01 (0.11)	0.02 (0.14)	0.01 (0.12)	0.01 (0.1)
West Virginia	0.00 (0.06)	0.00 (0.06)	0.00 (0.06)	0.01 (0.07)	0.00 (0.07)	0.00 (0.07)	0.00 (0.05)	0.01 (0.07)	0.00 (0.06)
Alabama	0.01 (0.1)	0.01 (0.1)	0.01 (0.08)	0.01 (0.09)	0.01 (0.1)	0.01 (0.1)	0.01 (0.08)	0.01 (0.09)	0.01 (0.1)
Kentucky	0.01 (0.11)	0.01 (0.11)	0.02 (0.13)	0.01 (0.11)	0.01 (0.12)	0.01 (0.12)	0.02 (0.15)	0.01 (0.11)	0.01 (0.11)
Mississippi	0.03 (0.17)	0.03 (0.17)	0.03 (0.18)	0.03 (0.18)	0.03 (0.17)	0.03 (0.17)	0.03 (0.18)	0.03 (0.18)	0.03 (0.17)
Tennessee	0.06 (0.24)	0.06 (0.24)	0.05 (0.21)	0.06 (0.24)	0.06 (0.24)	0.06 (0.24)	0.04 (0.19)	0.06 (0.24)	0.07 (0.25)
Arkansas	0.01 (0.08)	0.01 (0.08)	0.01 (0.08)	0.01 (0.1)	0.01 (0.08)	0.01 (0.08)	0.01 (0.09)	0.01 (0.1)	0.01 (0.08)
Louisiana	0.01 (0.11)	0.01 (0.11)	0.01 (0.1)	0.01 (0.11)	0.01 (0.11)	0.01 (0.11)	0.01 (0.1)	0.01 (0.11)	0.01 (0.11)
Oklahoma	0.01 (0.08)	0.01 (0.08)	0.01 (0.08)	0.01 (0.08)	0.01 (0.08)	0.01 (0.08)	0.01 (0.08)	0.01 (0.08)	0.01 (0.08)
Texas	0.06 (0.23)	0.06 (0.23)	0.05 (0.21)	0.06 (0.25)	0.06 (0.23)	0.06 (0.23)	0.04 (0.2)	0.07 (0.25)	0.06 (0.23)
Arizona	0.01 (0.1)	0.01 (0.1)	0.01 (0.08)	0.01 (0.11)	0.01 (0.09)	0.01 (0.09)	0.01 (0.08)	0.01 (0.1)	0.01 (0.09)
Colorado	0.01 (0.08)	0.01 (0.08)	0.01 (0.08)	0.01 (0.08)	0.01 (0.08)	0.01 (0.08)	0.01 (0.09)	0.01 (0.08)	0.01 (0.08)
Idaho	0.00 (0.03)	0.00 (0.03)	0.00 (0.03)	0.00 (0.03)	0.00 (0.03)	0.00 (0.03)	0.00 (0.04)	0.00 (0.03)	0.00 (0.04)
Montana	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00

	Enrolled 6 months post			Enrolled 9 months post			Enrolled 12 months post		
Location	True Negative	True Positive	False Positive	True Negative	True Positive	False Positive	True Negative	True Positive	False Positive
	(0.03)	(0.03)	(0.04)	(0.03)	(0.03)	(0.03)	(0.04)	(0.03)	(0.03)
Nevada	0.01	0.01	0.01	0.01	0.01	0.01	0.00	0.01	0.01
	(0.08)	(0.08)	(0.07)	(0.07)	(0.08)	(0.08)	(0.05)	(0.07)	(0.08)
New Mexico	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
	(0.03)	(0.03)	(0.04)	(0.03)	(0.03)	(0.03)	(0.04)	(0.02)	(0.03)
Utah	0.00	0.00	0.00	0.00	0.00	0.00	0.01	0.00	0.00
	(0.03)	(0.03)	(0.06)	(0.03)	(0.03)	(0.03)	(0.08)	(0.03)	(0.04)
Wyoming	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
	(0.02)	(0.02)	(0.04)	(0.03)	(0.02)	(0.02)	(0.05)	(0.03)	(0.03)
Alaska	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
	(0.02)	(0.02)	(0)	(0.02)	(0.02)	(0.02)	(0)	(0.03)	(0.02)
California	0.17	0.17	0.20	0.15	0.17	0.17	0.20	0.15	0.17
	(0.38)	(0.38)	(0.4)	(0.36)	(0.38)	(0.38)	(0.4)	(0.36)	(0.37)
Hawaii	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
	(0.01)	(0.01)	(0)	(0.01)	(0.01)	(0.01)	(0)	(0.01)	(0.02)
Oregon	0.00	0.00	0.01	0.00	0.00	0.00	0.01	0.00	0.00
	(0.06)	(0.06)	(0.07)	(0.06)	(0.06)	(0.06)	(0.09)	(0.05)	(0.05)
Washington	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.00	0.00
	(0.07)	(0.07)	(0.08)	(0.08)	(0.07)	(0.07)	(0.08)	(0.07)	(0.07)
Puerto Rico	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
	(0.02)	(0.02)	(0)	(0.01)	(0.02)	(0.02)	(0)	(0.02)	(0.01)
Unknown region	0.01	0.01	0.00	0.00	0.01	0.01	0.00	0.00	0.01
	(0.07)	(0.07)	(0.06)	(0.07)	(0.07)	(0.07)	(0.05)	(0.06)	(0.07)
N	39,330	528	4,968	33,082	480	4,182	27,535	436	3,507

Mean (SD)

Table A-3-2 Alternate medication definition regression estimates for the Medicaid population

	1	2	3	4	5	6	7	8	9	10	11	12	N
Enrolled- 12+ months before and 6+ months after index mammogram													
False-Positive	0	0	0	-0.001	0.001	0.001	0.001	0.001	0.002	0.002	0.002	0.002	88,830
	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	
True-Positive	0.001	0.001	0.003	0.002	0.008*	0.014**	0.012*	0.016**	0.017**	0.017**	0.017**	0.018**	
	(0.002)	(0.002)	(0.003)	(0.003)	(0.004)	(0.005)	(0.005)	(0.005)	(0.005)	(0.006)	(0.006)	(0.006)	
Enrolled- 12+ months before and 9+ months after index mammogram													
False-Positive	<0.001	<0.001	0.001	<0.001	0.002	0.001	0.001	0.002	0.003	0.003	0.003	0.003	75,715
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	
True-Positive	0.001	<0.001	0.002	0.001	0.008	0.014**	0.012*	0.016**	0.018**	0.018**	0.018**	0.019**	
	(0.002)	(0.002)	(0.003)	(0.003)	(0.004)	(0.005)	(0.005)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	
Enrolled- 12+ months before and 12+ months after index mammogram													
False-Positive	<0.001	0.001	<0.001	0.001	0.001	0.001	0.002	0.002	0.002	0.002	0.003	<0.001	63,725
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	
True-Positive	0.001	0.002	0.001	0.008	0.014**	0.012*	0.017**	0.017**	0.017**	0.017**	0.017**	0.001	
	(0.002)	(0.003)	(0.003)	(0.005)	(0.005)	(0.005)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.002)	
Enrolled- 9+ months before and 6+ months after index mammogram													
False-Positive	0.001	0.001	0.001	0	0.002	0.002	0.002	0.002	0.003	0.003	0.002	0.003	104,141
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	
True-Positive	0.001	<0.001	0.001	0.001	0.006	0.013**	0.013**	0.016***	0.018***	0.017***	0.017**	0.017**	
	(0.002)	(0.002)	(0.002)	(0.003)	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	
Enrolled- 9+ months before and 9+ months after index mammogram													
False-Positive	0.001	0.001	0.001	0.001	0.002	0.002	0.002	0.003	0.003*	0.004*	0.003	0.004*	89,501
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	
True-Positive	0.001	<0.001	0.001	<0.001	0.006	0.014**	0.013**	0.017**	0.018***	0.018***	0.018**	0.018**	
	(0.002)	(0.002)	(0.002)	(0.003)	(0.004)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.006)	(0.006)	
Enrolled- 9+ months before and 12+ months after index mammogram													
False-Positive	<0.001	0.001	0.001	<0.001	0.002	0.002	0.002	0.002	0.003	0.003	0.003	0.004	76,197
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	
True-Positive	0.001	<0.001	0.001	<0.001	0.006	0.013**	0.013**	0.017**	0.018**	0.018**	0.017**	0.017**	
	(0.002)	(0.002)	(0.003)	(0.003)	(0.004)	(0.005)	(0.005)	(0.005)	(0.006)	(0.006)	(0.006)	(0.006)	

(Robust standard errors clustered by individual)

* p<0.10, ** p<0.05, *** p<0.01
All regressions also control for age, race/ethnicity, reason for Medicaid eligibility, capitation, whether plan is comprehensive.

Table A-3-3 Alternate medication definition regression estimates for the commercially insured population

	Enrolled- 12+ months before and 6+ months after index mammogram											
	1	2	3	4	5	6	7	8	9	10	11	N
False-Positive	0	<0.001	0.000**	0.001***	0.001***	0.001***	0.002***	0.002***	0.002***	0.002***	0.002***	262,110
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	
True-Positive	<0.001	0.001	0.001	0.002	0.002	0.003*	0.004*	0.005**	0.005**	0.006**	0.006**	
	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	
	Enrolled- 12+ months before and 9+ months after index mammogram											
	1	2	3	4	5	6	7	8	9	10	11	N
False-Positive	<0.001	<0.001	0.001*	0.001***	0.001***	0.002***	0.002***	0.002***	0.003***	0.003***	0.003***	128,533
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)	
True-Positive	<0.001	<0.001	<0.001	-0.000*	0.001	0.003	0.004	0.004	0.005*	0.006*	0.006*	
	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)	
	Enrolled- 12+ months before and 11+ months after index mammogram											
	1	2	3	4	5	6	7	8	9	10	11	N
False-Positive	<0.001	<0.001	<0.001	0.001	0.001*	0.002*	0.002*	0.002*	0.002**	0.002**	0.003**	41,149
	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	
True-Positive	<0.001	<0.001	<0.001	<0.001	0.003	0.003	0.006	0.006	0.006	0.01	0.01	
	(0.000)	(0.000)	(0.000)	(0.000)	(0.003)	(0.003)	(0.005)	(0.005)	(0.005)	(0.006)	(0.006)	
	Enrolled- 9+ months before and 6+ months after index mammogram											
	1	2	3	4	5	6	7	8	9	10	11	N
False-Positive	0.000*	0.000**	0.000***	0.001***	0.001***	0.001***	0.001***	0.002***	0.002***	0.002***	0.002***	421,099
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	
True-Positive	-0.000*	<0.001	0.001	0.001	0.002*	0.003**	0.003**	0.003**	0.004***	0.004***	0.004***	
	(0.000)	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	
	Enrolled- 9+ months before and 9+ months after index mammogram											
	1	2	3	4	5	6	7	8	9	10	11	N
False-Positive	<0.001	0.000**	0.000***	0.001***	0.001***	0.001***	0.002***	0.002***	0.002***	0.002***	0.002***	281,863
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	
True-Positive	<0.001	-0.000*	<0.001	<0.001	0.001	0.002*	0.002*	0.003*	0.003**	0.004**	0.004**	
	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	
	Enrolled- 9+ months before and 11+ months after index mammogram											
	1	2	3	4	5	6	7	8	9	10	11	N
False-Positive	<0.001	0.000*	0.000*	0.001**	0.001***	0.001***	0.001***	0.001***	0.002***	0.002***	0.002***	191,680
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	
True-Positive	<0.001	-0.000*	<0.001	<0.001	0.001	0.001	0.002	0.002*	0.003*	0.004*	0.003*	
	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	

(Robust standard errors clustered by individual)

* p<0.10, ** p<0.05, *** p<0.01
All regressions also control for age, insurance plan type, and location of residence.

Table A-3-4 Baseline regressions restricted to women with previous depression or anxiety fill but not in last 6 months

Continuously enrolled 12+ months before

	Medicaid			Commercial		
	6 months	9 months	12 months	6 months	9 months	11 months
False-Positive	-0.001	<0.001	<0.001	0.002	0.004	-0.006
	(0.004)	(0.003)	(0.002)	(0.005)	(0.004)	(0.007)
True-Positive	0.005	0.006	0.002	-0.006	-0.003	-0.012
	(0.012)	(0.008)	(0.006)	(0.014)	(0.012)	(0.022)
N	40,926	36,211	31,271	5,909	2,981	888

Continuously enrolled 9+ months before

	Medicaid			Commercial		
	6 months	9 months	12 months	6 months	9 months	11 months
False-Positive	<0.001	<0.001	<0.001	0.003	<0.001	-0.004
	(0.004)	(0.003)	(0.002)	(0.005)	(0.004)	(0.003)
True-Positive	0.001	0.006	0.004	-0.006	<0.001	-0.004
	(0.010)	(0.008)	(0.005)	(0.011)	(0.008)	(0.007)
N	46,785	41,654	36,334	9,745	6,706	4,543

(Robust standard errors clustered by individual)
* p<0.10, ** p<0.05, *** p<0.01
Medicaid regressions also control for age, race/ethnicity, reason for Medicaid eligibility, capitation, whether plan is comprehensive.
Commercial regressions also control for age, insurance plan type, and location of residence.

Table A-3-5 Diagnosis and procedure codes

Condition	Code type	Code #	Source
Screening mammogram	CPT	76092	Freeman et al. (2002); Tan et al. (2006); Randolph et al. (2002)
	ICD-9	V76.12	Adams and Koch (2014); Randolph et al. (2002)
Other mammogram	CPT	76090, 76091	Freeman et al. (2002); Tan et al. (2006); Randolph et al. (2002)
	ICD-9	87.37, V76.11	Freeman et al. (2002)
Breast biopsy	CPT	19100, 19101, 19120	Freeman et al. (2002); Tan et al. (2006)
	ICD-9	85.11, 85.12, 85.20, 85.21	Freeman et al. (2002); Tan et al. (2006)
Breast ultrasound	CPT	76645	Freeman et al. (2002); Tan et al. (2006)
Other breast radiological procedures	CPT	76003, 76086, 76087, 76088, 76095, 76098, 76100, 76101, 76102, 76120, 76125, 76140, 76150, 76350, 76355, 76360, 76362, 76365	Freeman et al. (2002)
	ICD-9	87.35, 87.36, 87.73, 88.85	Freeman et al. (2002)
Breast cancer treatment	CPT	19160, 19162, 19180, 19200, 19220, 19240, 38740, 38745	Warren et al. (1999); Cooper et al. (1999); Freeman et al. (2000)
	ICD-9	174.x, 233.0, V103	Warren et al. (1999); Cooper et al. (1999); Freeman et al. (2000)

Chapter 4.
Stocking up or delaying: the effects of future unemployment and uninsurance on preventive health care utilization

5.1 Introduction

Despite evidence of its effectiveness in improving health, utilization of preventive health services remains suboptimal (Maciosek et al., 2006; Maciosek et al., 2010; McGlynn et al., 2003). This set of circumstances helped in part to spur the requirement in the ACA that all health plans cover a variety of preventive services without cost sharing (Koh and Sebelius, 2010; Health and Human Services, 2012). In addition to the broad efforts to increase rates of preventive care utilization as part of the ACA, recent studies have also pointed to economic downturns as periods when preventive health services may be particularly under-utilized (Tefft and Kageleiry, 2014; King et al., 2014; Lusardi et al., 2015). Despite this recent evidence, two issues remain unexplored. First, it is not clear how much of the effect is due to the loss of a job, the loss of health insurance, or the combination of the two. Second, current studies ignore potentially important anticipatory effects—anticipated unemployment or uninsurance may have very different effects than concurrent unemployment or uninsurance and ignoring the anticipatory effects may mask important outcomes. For example, if in anticipation individuals stock up on care this may mean current negative effects are overestimated while if individuals delay care this may mean currently estimated negative effects actually understate the problem. This study provides the first estimates of the effect of future unemployment and/or uninsurance

on preventive health utilization, showing that anticipated unemployment may lead to increased preventive care utilization (stocking up) while anticipated uninsurance may lead to reduced utilization (delaying).

This has important policy implications, in particular in terms of how best to create policy to encourage preventive health care utilization during recessions and which populations may need to be targeted. The results suggest that if an individual is able to maintain a source of health insurance then the observed stocking up behavior may mean anticipated unemployment is less of an issue. However, policies to improve access to preventive services as well as policies to better guide individuals at risk of losing health insurance to affordable options for health insurance coverage that cover care for cancer and other chronic diseases without excessive cost sharing[28] may be particularly important during economic recessions.

5.2 Background

This study combines several different strands of the health economic and health services research literature. First, it fits in broadly with the work that has been done both on how unemployment and uninsurance may affect utilization as well as how cyclical variations in the macroeconomy may affect employment and insurance and in turn affect utilization. This study also fits into the literature on anticipatory effects on care utilization, specifically focusing on how the anticipated effects of both unemployment and uninsurance may affect preventive care utilization.

At the individual level, most of the work on the effects of unemployment on health and health care utilization has focused on mental health, finding worse mental health and increased mental health care following unemployment (Hamilton et al., 1990; Dooley, Fielding, Levi,

[28] This could include marketplace plans or Medicaid.

1996; Keefe et al., 2002). Some studies also find worse physical functioning in addition to worse mental health (Gallo et al., 1999; Burgard, Brand, and House, 2007), although certainly one of the difficulties in estimating the effect of unemployment on utilization and health is reverse causality—poor health may lead to unemployment (Riphahn, 1998; Garcia-Gomez, 2011; Garcia-Gomez, Jones, and Rice 2010). In part to get around this issue, many studies that examine the effect of unemployment on health care utilization focus more on state-level cyclical variations of employment to identify the effects on utilization. Given that these studies primarily examine the effect of cyclical variations, I focus on this set of studies in more detail in the subsequent set of studies. Finally, much of the effect of unemployment on health care utilization is likely to operate through its effect on insurance, so disentangling the two, although not often done, is important.

An even greater number of studies have tried to analyze the effect of health insurance on both health and health care utilization. Similar to unemployment, one of the econometric difficulties is that worse health may lead to a lower likelihood of health insurance (Levy and Meltzer, 2008) or at least employer-provided health insurance (Buchmueller et al., 2005). Although evidence of the effect of insurance on health, especially in terms of magnitude, remains somewhat mixed (Levy and Meltzer, 2008; Baicker et al., 2013) there is fairly strong evidence that insurance leads to increased utilization including preventive care utilization (Buchmueller et al., 2005; Card, Dobkin, and Maestas, 2008; Finkelstein and McKnight, 2008; Finkelstein et al., 2012; Baicker et al., 2013).

While the aforementioned work has focused on the effect of unemployment and insurance on health care utilization, a related set of literature has then further focused on the effect of cyclical variation in the macroeconomy, particularly changes in unemployment, on both

health and health care utilization. A study by Ruhm (2003) finds modest increases in any hospitalization and any physician visit with declining unemployment (pro-cyclical effects), while two studies by McInerney and Mellor (2012a, 2012b) find significant increases in hospital care, physician and clinical services, and nursing home care as state unemployment levels rise among Medicare beneficiaries (counter-cyclical effects). A study by Quinn, Catalano, and Felber (2009) finds significant decreases in preventive dental visits when unemployment rises—further suggesting utilization may be pro-cyclical. Similarly, Tefft and Kageleiry (2014) find that the quantity of preventive services utilized appears to be pro-cyclical, using an index of services including mammograms, Pap tests, colorectal cancer scope exams, PSA tests, digital rectal exams, annual checkups, and seasonal flu vaccinations. However, one limitation with all of these studies is the use of state unemployment rates as the main variable of interest, which makes understanding the mechanism of the result difficult to interpret, particularly when trying to disentangle the current and anticipatory effects. Furthermore, it masks potentially important differences in the effects of unemployment and uninsurance.[29]

Several studies have also focused specifically on the Great Recession of 2008-2009 finding that breast and cervical cancer screening decreased significantly (King et al., 2014) as did screening colonoscopy among the insured (Dorn et al., 2012). Finally, using cross-national data Lusardi, Schneider, and Tufano (2015) find significant reductions in routine care utilization during the recession, with the largest declines among those who become unemployed or have the greatest losses in wealth as well as for individuals living in the United States. However, similar

[29] There is also a substantial literature on cyclical variation in weight, smoking, and physical activity (see e.g. Ruhm, 2000; Charles and DeCicca, 2008; Colman and Dave, 2014; Currie, Duque, and Garfinkel, 2014), although these are outside the main focus of this paper which is on medical care, and more specifically preventive care utilization. One main reason I focus on preventive rather than curative care is that preventive care is typically more discretionary.

to the aforementioned studies, these are limited by the inability to separate concurrent and anticipatory effects.[30]

While the literature on cyclical variations in care utilization has mostly ignored anticipatory effects, several studies have focused on the existence of anticipatory effects more generally in the literature on medical care utilization. Two earlier studies find limited evidence of individuals stocking up in anticipation of losing health insurance. A study by Long et al. (1998) analyzes the effect of health insurance transitions on physician visits and inpatient admissions, finding little evidence that individuals who transition from being insured to uninsured utilize more of either type of care. The study has several limitations, though. First, the number of individuals who transition is low. Second, and more importantly, the measures of utilization are not ideal for detecting stocking up. Inpatient admissions are far less discretionary and are a poor candidate for stocking up, while total physician visits as opposed to preventive care or other more elective visits may suffer from similar problems. Finally, the study does include any exogenous source of uninsurance. This is a problem as I will show later, since the population who becomes uninsured may be low utilizers and therefore value health insurance less. The second study that finds limited evidence of stocking up is a working paper by Gross (2009) that focuses on medical utilization in the teenage population. This study takes advantage of the plausibly exogenous loss of health insurance in the population that turns 19 and therefore can no longer be on a parent's health insurance. However, one limitation with this study is that the young adult population typically has low rates of medical care utilization. Therefore it may not be surprising that little evidence of stocking up is found.

[30] In the case of the study by Lusardi, Schneider, and Tufano (2015), the effect is largely due to unemployment in Germany, France, and the United Kingdom given the national health insurance programs. However, the limitation still exists for the results for the United States, where the unemployment and insurance effects are not teased apart.

While these two earlier studies are more suggestive of myopic behavior, three more recent studies suggest the possibility of anticipatory behavior in medical care utilization. A recent working paper by Alpert (2014) finds seniors may have reduced chronic disease (but not acute condition) medication utilization between the time the Medicare Part D law was passed and when it was actually implemented, which suggests anticipatory behavior. Another study finding evidence of anticipatory behavior is one by Aron-Dine et al. (2015) that examines how individuals that face the same "spot" price but different "future" prices have different rates of utilization. The difference in prices comes from comparing individuals with different join dates, and therefore different probabilities of reaching the plan deductible. While the current price paid does not differ, individuals who join the firm later in the year are less likely to meet the deductible, therefore face a higher "future" price, and are found to have lower initial month utilization. Aron-Dine et al. extend the analysis and find similar results for prescription drug utilization using Medicare Part D, showing anticipatory behavior appears to exist across a wide set of populations.

Finally, a study by Hughes and Khaliq (2014) uses data on state unemployment rates but tries to focus on anticipatory effects by restricting the analyses to individuals who remain employed and insured. The authors use 1996-2008 Medical Expenditure Panel Survey (MEPS) data to examine the effect of state unemployment rates on counts of different types of provider visits (inpatient, outpatient, office-based, ER) as well as select services (sonogram, x-ray, mammogram, MRI and CT, or other diagnostic services). The study finds an increase in the state unemployment rates is associated with a significant increase in hospital outpatient and emergency room visits, and in mammography among women ages 50 and older. However, the study has several limitations including: focusing on fairly broad types of visits, largely missing

the Great Recession, not separating unemployment and uninsurance, and not making it clear how at risk the population was of becoming unemployed or uninsured by focusing solely on the always employed and insured population. This last point is important because the paper intends to focus on anticipatory effects, so knowing how likely either unemployment of uninsurance is for the study population is critical to understanding how to interpret the anticipatory effects.

Building on these different strands of the literature, the current study makes several contributions to the literature. It is the first study to separately estimate the effect of both unemployment and uninsurance on utilization; and the first to focus on both in terms of anticipated effects. Finally, it is the first to examine anticipated effects in the time period that includes the Great Recession.

5.3 Theoretical Framework

While most of the existing research has focused on how current unemployment or uninsurance may affect health or health care utilization, this paper focuses on the potential anticipatory effects of future unemployment or uninsurance on utilization. Anticipatory effects can manifest themselves in one of two forms—stocking up (i.e. increasing utilization) or delaying (i.e. decreasing utilization) in anticipation of unemployment or uninsurance. The effect of anticipated unemployment and uninsurance can both have multiple effects operating in opposite directions. Although the overall sign of each effect is ambiguous, they are composed of the following potential effects.

Anticipated unemployment could have three potential effects.

(1) Becoming unemployed could mean potentially becoming uninsured, the effects of which will be discussed shortly. However even if unemployment does not lead to

uninsurance (or even temporary uninsurance), it could mean switching health plans and therefore needing to change providers. If the current provider is well liked, an individual might try to stock up on care with the current provider.

(2) The anticipated reduced future income could lead to lower current period utilization as preventive health care is delayed until income is higher in a future period.

(3) Finally, if an individual anticipates potential unemployment, she may try to work additional hours in order to lower the likelihood of being laid off or otherwise becoming unemployed[31]. With time as an important input in the health production function (Grossman, 1972), this could lead to delayed care as time is more constrained. Similarly, if an individual thinks she is likely to become unemployed, she may delay care until the time of unemployment when there may be more time to go to an appointment, although this ignores any potential changes in the out-of-pocket price of care.

As with anticipated unemployment, the effect of anticipated uninsurance has an ambiguous effect on preventive health utilization with two possible contrasting effects.

(1) Anticipated uninsurance would mean that the future price of preventive care would be higher thus leading to greater current utilization (stocking up).

(2) However, preventive care is a unique type of medical care. While care such as a flu vaccination may directly prevent an illness (primary prevention), more commonly (secondary) preventive care is used to identify a condition early on in order to treat it more effectively (Dekker and Sibai, 2001). For secondary prevention, subsequent

[31] In addition to not wanting to leave work for an appointment during a period when unemployment may become more likely, an individual may also want to avoid utilizing care that may make her seem like a more costly employee. Certainly employment decisions cannot be made based on what an employee's health care costs are but if the employee believes they are this might further discourage care.

treatment is how an individual is able to improve health. Individuals who believe they are likely to lose health insurance may be less likely to utilize secondary preventive health care because there is little benefit if they will be unable or less able to afford subsequent treatments. This may be particularly true of screening for high cost conditions such as cancer.

One final possible effect is that anticipated unemployment could also mean an individual is more likely to have to use public health insurance[32], as result of either losing employer provided health insurance and/or reduced income. Given that not all providers accept Medicaid (Decker 2012, Decker 2013), an individual might worry about having to find an alternate provider. In anticipation of this, she might stock up on preventive care with the current, preferred provider[33]. This also suggests that anticipating a greater likelihood of using public insurance might lead to greater stocking up. In addition, the possibility of public insurance may mitigate the delaying of preventive care because an individual would still have future treatment covered in the event the screening came back positive. Despite these factors leading to the possibility of stocking up, an individual's out-of-pocket price is likely lower under public insurance given the relatively low levels of cost sharing in Medicaid (Centers for Medicare and Medicaid Services, 2013). Thus there is also potentially an incentive to delay preventive care. Therefore, a secondary set of analyses include measures of anticipated move from private to public insurance to capture these potential effects.

[32] In addition to public insurance there are also various programs that provide free or reduced cost preventive care. For example, the National Breast and Cervical Cancer Early Detection Program (NBCCEDP) provides free or reduced cost mammograms and Pap tests to low income and uninsured or under-insured women (see http://www.cdc.gov/cancer/nbccedp/).
[33] This is admittedly a somewhat unlikely event given that physicians are likely to accept Medicaid insurance from an existing patient even if they are less likely to accept a new patient with Medicaid.

5.4 Hypotheses

Given the numerous possible effects of anticipated unemployment or uninsurance on preventive health utilization, it is difficult to empirically disentangle each of the effects, so the empirical analyses will focus on the overall effect, which is likely a combination of multiple, simulataneous effects. However, there are a few predictable effects that I hypothesize are likely to occur.

First, although the sign on the future uninsurance variable is unclear, it is likely to be more negative (or positive and smaller in magnitude) for the cancer screenings. To the extent that cancer is a particularly expensive disease (Mariotto et al., 2011) and known to lead to higher rates of bankruptcy (Himmelstein et al., 2009), an individual may be more likely to delay if she expects to be uninsured in the near future when any potential treatments might be likely to occur.

Second, regardless of the sign on either the unemployment or uninsurance variable, the effect is likely larger the more likely the individual is to become unemployed or uninsured. I therefore predict that the magnitudes of the unemployment and uninsurance variables are likely to increase as I move from the baseline sample, to the sample in the 75^{th} percentile of greater of predicted unemployment or uninsurance to the 90^{th} percentile or greater.

Third, again regardless of the sign of the unemployment and uninsurance effects, the effects should be larger in magnitude during the Great Recession of 2008-2009. If the effects are truly a result of anticipated unemployment and uninsurance then the effects should be largest when the risk of both is highest, i.e. during the Great Recession. I estimate this by restricting the sample to the year 2007-2010 to allow for a year before and after the Great Recession.

5.5 Data

In this study I use 2000-2012 Medical Expenditure Panel Survey (MEPS) data, a nationally representative survey of the civilian, non-institutionalized population that collects demographic, employment, health status, health insurance, and health care utilization data. The survey is a two year overlapping panel design, where each individual provides five rounds of data over two years. In any given year of the survey, approximately half of the sample is in their first year of collection and half is in their second. (Cohen, Cohen, and Banthin, 2009). For nearly all individuals, rounds 1 and 2 are collected in the first calendar year; round 3 spans calendar years 1 and 2; and rounds 4 and 5 are collected in the second calendar year.

The survey design is particularly important in terms of understanding the creation of the three main sets of variables of interest in this study. Employment and insurance status are collected from respondents in each round. However, data on utilization of preventive health services are collected only in rounds 3 and 5. For most of the preventive care services (i.e. physical exam, blood pressure checked, cholesterol checked, flu vaccination, mammogram, Pap test, PSA test) I define the variable as having received the service within the past year based on the individual's response in round 3 of MEPS[34]. To then best match anticipated unemployment[35] and uninsurance to these measures of the dependent variable, I define future unemployment/uninsurance as a response of being unemployed/uninsured in rounds 4 or 5.

5.6 Methods

One of the important empirical difficulties in estimating the effect of future employment and/or uninsurance on preventive care utilization is the issue of endogeneity. Multiple studies

[34] One note is that I will refer to rounds 1-3 as the "first year" and rounds 4-5 as the "second year" even though round 3 may straddle the two calendar years. However, the reference is largely due to the fact that the round 3 preventive service utilization questions refer to the past year.
[35] One note about using the term unemployment is that it should be most accurately be described as not employed, but unfortunately there is no MEPS variable about looking for employment so I cannot truly identify unemployment in the strict economic sense.

have shown that poor health or health shocks may lead to unemployment (Riphahn, 1998; Garcia-Gomez, 2011; Garcia-Gomez, Jones, and Rice 2010) and increase the length of the unemployment spell (Stewart, 2001), making it difficult to disentangle the direction of the effect of anticipated unemployment on preventive health care utilization. The issue exists for preventive care utilization, although the direction of the bias is not necessarily clear. Low preventive care utilization could lead to poor health outcomes and therefore a greater likelihood of unemployment. However, individuals in worse health or with a greater number of chronic conditions might use more preventive care to avoid complications but then also be more likely to become unemployed. A similar problem exists for the possibility of becoming uninsured. For individuals with employer provided health insurance, worse health leading to job loss could also lead to loss of health insurance (Bradley, Neumark, and Motika, 2012). However, the issue is further complicated by the issue of job lock, whereby individuals in worse health are may be more likely to remain at a job in order to maintain health insurance (Gruber and Madrian, 2002; Bradley, Neumark, and Barkowski, 2013).

While each of these mechanisms may be important, the purpose of describing them is to highlight the potential problem with solely using measures of future unemployment or uninsurance in a regression framework. To try to ameliorate these problems I use two different analytic techniques. The first is to use predicted unemployment and uninsurance to create a subsample of individuals who are most likely to become unemployed or uninsured and the second is to use layoffs as a more plausibly exogenous reason for becoming unemployed and/or uninsured. One alternative approach will also be to use the predicted values themselves in place of the actual unemployment and uninsurance variables, although I will discuss some of the drawbacks to this approach later.

The general analytic approach is to estimate the effect of next period unemployment and/or uninsurance on the probability of utilizing various types of preventive care in the current period while controlling for various current period characteristics. To most accurately capture the effects of anticipated unemployment and uninsurance on preventive care utilization, I make several sample restrictions. First, I restrict the sample to individuals who have two years of MEPS data so that I have multiple observable periods allowing for the use of future unemployment and uninsurance. Then I restrict the sample to individuals between the ages of 24 and 64. The lower age bound is similar to the one used by Colman and Dave (2015) and ensures that the analyses are focused on the full-time employed population rather than students; while the upper bound is to exclude the Medicare insured population, who would likely not become uninsured and may be more likely to retire and therefore have a very different set of employment effects.

The next restriction is to limit the sample to individuals who are employed and insured their entire first year (i.e. rounds 1, 2, and 3). This restriction is to ensure that any differences in preventive care utilization are due to future or anticipated unemployment and uninsurance and not concurrent unemployment and uninsurance. I also exclude all women who are pregnant as their health care utilization and potential changes in employment status may be very different from the rest of the sample. I further exclude individuals who retire in the second period because this decision is likely to have very different effects than becoming unemployed.Finally, the sample also varies slightly depending on the specific type of preventive care. For example, the sample only includes women ages 40 and older when estimating models of mammogram utilization, women for models of Pap tests, and men ages 50 and older for models of PSA testing.

5.7 Econometric Model

Baseline regressions analyses are estimated using the following format:

$$Y_{i,t} = \beta_0 + \beta_1 Unemployed_{i,t+1} + \beta_2 Uninsured_{i,t+1} + \beta_3 Unemployed\ x\ Uninsured_{i,t+1} + \beta_4 X_{i,t} + \beta_5 T + \varepsilon_i$$

I run separate analyses for each dependent variable, $Y_{i,t-1}$, which is a measure of preventive care utilization including: physical within the past year, blood pressure checked within the past year, cholesterol checked within the past year, flu vaccination within the past year, mammogram within the past year, Pap test within the past year, PSA test within the past year[36]. The dependent variables are all created using responses from round 3 and therefore refer to care within the respondent's first year in MEPS[37]. The unemployed variable indicates whether the individual reports being unemployed in either round 4 or 5, and similarly the uninsured variable indicates whether the individual is uninsured in rounds 4 or 5. I therefore regress current utilization on future unemployment and uninsurance while controlling for various current individual level factors (X) as well as year fixed effects (T). Alternatively, in addition to the measure of uninsurance, I also include a measure of a future move from private insurance to public insurance' which includes any response for receiving Medicaid, Medicare, or TRICARE in the second year. For ease of interpretation, I run these as linear probability models.

The individual level factors I control for include age, sex, race/ethnicity, education, marital status, income, number of employees in firm, region, urban residence, self-reported health status, presence of physical limitations of work, smoking status, a series of risk preference

[36] Initial models also included colonoscopy/sigmoidoscopy but the results were very inconsistent, so I drop this dependent variable. The variability is likely a result of the infrequency of recommended screening, i.e. every 5-10 years (Levin et al., 2008; Qaseem et al., 2012).
[37] The preventive care questions are only asked in rounds 3 and 5. Therefore responses of utilization in the past year refer, respectively, to the first and second year in MEPS, respectively.

measures[38], and presence of comorbidities[39] both in oneself and in one's spouse. The risk preference measures are included to attempt to control for the fact that individual risk preferences may influence decisions to utilize preventive care, which are inherently risk-reducing measures rather than amelioration of any type of illness, pain, or discomfort. The measures are based on responses to questions about beliefs on an individual's need for health insurance, whether health insurance is worth the cost, whether an individual can overcome illness without medical help, and whether the individual is more likely to take risk. Although the risk measures are included in most specifications, excluding them ultimately has little effect on any reported results. This may be in part due to the sample restriction that individuals must be insured for the entire first year thereby eliminating many of the very risk tolerant individuals.

In addition to the baseline model, instead of using the full sample I first predict the likelihood of unemployment or uninsurance and then restrict the preventive care utilization analyses to the individuals who are at higher risk of becoming unemployed or uninsurance, namely those in either the 75^{th} percentile or greater of predicted unemployment or uninsurance. To get the predictions, I first estimate the probability of becoming unemployed or uninsured using a probit model and regressors similar to the ones used in the baseline mode as well as additional job-related measures including industry, occupation, hours worked, and hourly wage. One limitation with the MEPS data is that both the industry and occupation codes are significantly condensed in the publicly available data, somewhat limiting their predictive power. The additional measures are factors that may make an individual more or less likely to become unemployed or uninsured, and more importantly might help to inform an individual's own

[38] See Table A-4-1 for means by unemployment and uninsurance transition population.
[39] Comorbidities include cancer, diabetes, disorders of lipid metabolism (i.e. high cholesterol), cardiovascular disease, chronic obstructive pulmonary disease (COPD), asthma, kidney disease, joint disorders, osteoporosis, anxiety disorders, or mood disorders based largely on the Charlson Comorbidity Index (e.g. Quan et al., 2011) along with two types of mental disorders. One difference from the standard comorbidity indices is that I have limited ICD-9 detail so I use the MEPS clinical classification codes.

assessment of her likelihood of becoming unemployed or uninsured. While I use actual future unemployment, uninsurance, and their cross-product in the preventive care utilization regressions, I restrict the sample to all individuals in in either the 75th percentile or greater of predicted unemployment or uninsurance. I also vary the percentile cutoff to the 90th percentile. Although, I go into more detail later, one reason I use this approach rather than using the predicted values themselves is that, especially given the industry and occupation variable limitations, the predictions are relatively weak. I am therefore worried about biased estimates with these weak "instruments" (Bound, Jaeger, and Baker, 1995).

For the second alternate specification I define two sets of unemployed and uninsured variables. The first is unemployment as a result of being laid off while the second is unemployment for all other reasons (uninsurance is similarly defined). This approach is similar to the one used by Colman and Dave (2015) who use layoffs as a potentially more exogenous source of unemployment.

For each of the three main sets of regressions, I run various alternate specifications. I estimate all analyses restricted to the population with only private health insurance for all rounds of the baseline period, and to the population with employer provided health insurance for all rounds of the baseline. Finally, I also restrict the analyses to the year 2007-2010 to specifically target the Great Recession of 2008-2009.

In addition to the main analyses I run two alternate sets of regressions. The first sensitivity analysis is to replace actual unemployment and uninsurance with their predicted values from the prediction equationsThe predicted values are estimated using the same analyses used to create the predictions for the restricted sample analyses, where the "instruments" are

occupation and industry. I estimate predicted unemployment and uninsurance using both a logged version of the prediction as well as using an indicator for being in the 75th percentile or greater of the prediction distribution. Given that I am using predicted unemployment and uninsurance in the second stage of the model, the standard errors would be underestimated by not taking into account the variation from the first part of the model (Murphy and Topel, 1985; Inoue and Solon, 2010). However, the one difficulty with estimating the standard errors using the traditional approach suggested by Murphy and Topel (1985) or Inoue and Solon (2010) is the use of two separate first stage regressions. I therefore estimate bootstrapped standard errors using 1000 replications, similar to one of the suggestions in Inoue and Solon (2010).

While I include this analysis as a sensitivity analysis, the reason to not include it in the main set of analyses is that the "instruments" are relatively weak[40]. As previously mentioned, while the predictions are higher for those who actually become unemployed or uninsured, I am still unable to predict most instances of unemployment or uninsurance based on observables. Again, the likely main limitation is that the occupation and industry codes are condensed in the publicly available MEPS files, meaning larger groups of employees with varying levels of potential unemployment and uninsurance risk are grouped together, making these variables relatively weak in terms of serving as instrumental variables. In addition there is a concern about the excludability of the "instruments", namely that individuals in particular industries or occupations may vary in their propensity to utilize preventive services based on some set of unobservables. This is somewhat mitigated by the inclusion of controls for education, income, other job related variables, and several measures of risk preferences (see Table A-4-1).

[40] Namely what would be first stage F-statistics of typically around 3-4.

5.8 Results

In this section, I first present baseline estimates using measures of actual future unemployment and uninsurance but then present evidence that suggests there may be endogeneity issues. I therefore focus most of the results and discussion on the two preferred analytic techniques—restricting the sample to individuals with high predicted values of unemployment or uninsurance and using layoffs. Table 4-1 shows summary demographic and socioeconomic information for the overall sample and by second year unemployment and uninsurance status. Overall, approximately 2 percent of the sample becomes unemployed and 3.5 percent of the sample becomes uninsured, including nearly 1 percent of the sample becoming both unemployed and uninsured. Although these rates may seem low, it is largely due to the sample restriction that all individuals are both employed and insured for the first year. Somewhat surprisingly, unemployment and uninsurance are relatively uncoupled. Within the population that becomes unemployed about twice as many people maintain insurance as become uninsured. The difference is even more dramatic on the uninsurance side, where nearly five times as many people remain employed as become unemployed. Therefore, as will be seen, the main effects tend to be more significant than the interaction effects.

Not surprisingly, the characteristics of the populations with different employment and insurance trajectories vary considerably. Generally, the population that remains insured but becomes unemployed actually looks more similar to the population that remains employed and insured rather than to the two populations that become uninsured. The one exception is that individuals that become unemployed (both those who remain insured and those who become uninsured) are more likely to report fair or poor health and are more likely to report limitations to their daily activities. Most of the observed differences are in the population that becomes

uninsured (both those that remain employed and those that become unemployed) who are in general younger, more likely non-white, lower education, less likely married, lower income, and more likely to smoke in the first year.

In addition to differences in demographic and socioeconomic characteristics, there are important health related differences between the four different unemployment and uninsurance transition populations. In general, as Table 4-2 shows, the highest rates for the various health conditions tend to be in the population that becomes unemployed but remains insured. This is consistent with existing evidence that serious health conditions may make the probability of unemployment more likely (Garcia-Gomez, 2011; Garcia-Gomez, Jones, and Rice 2010; Riphahn, 1998). Although, interestingly, compared to the population that becomes unemployed but retains health the insurance, the population that also becomes uninsured is less likely to have many of the health conditions. This may be a selection issue whereby individuals who have serious health conditions and become unemployed have a strong incentive to find a source of health insurance. Alternatively, if an individual has a serious health condition and another possible source of health insurance, she may be more likely to leave the job and become unemployed at least temporarily. The final common pattern is that the population that remains employed but becomes uninsured has the lowest rates of most of the health conditions. This could be in part due to this population being the youngest, but may also be an artifact of the selection story whereby they have less of an incentive to find alternative sources of health insurance if they lose it or they may be more willing to choose another job even if it does not offer health insurance because they are in relatively better health. Overall, this provides some initial evidence that endogeneity and selection issues may be present and may bias estimates of

the effect of actual future unemployment and uninsurance measures on preventive care utilization.

Further bolstering the argument that there may endogeneity issues, are the results for the mean utilization rates of the various preventive service measures. As Table 4-3 shows, typically the highest rates of use are the in the population that becomes unemployed but remains insured, while the lowest are in the population that remains employed but becomes uninsured. While this does not necessarily indicate an issue, the rates of going at least 5 or more years without utilizing the various types of preventive care are more suggestive of selection issues. As Table A-4-2 shows, the population that becomes uninsured but remains employed is the most likely to go five or more years without utilizing each of the preventive care measures, with Pap test utilization being the sole exception. Conversely, the population that becomes unemployed but remains insured is the least likely to go five or more years without utilizing each of the preventive care measures, again with the Pap test being the sole exception. The main advantage of using this 5 year measure is that it isolates the individuals that are particularly low utilizers, as a 5 year window makes utilization particularly non-proximate to any of the unemployment or uninsurance transitions that are observed in the data[41].

Baseline regressions

The baseline regression estimates of the effect of future unemployment and uninsurance on general preventive care and cancer screening are shown, respectively, in Table 4-4 and Table 4-5. For each table, I present estimates for the overall population in column (1), for those with private health insurance for the entire first year in column (2), for those with employer provided

[41] Certainly one limitation is that I do not know about potential unemployment or uninsurance transitions in the 4 years prior to the observable MEPS data. An individual who becomes unemployed and/or uninsured in the MEPS data may be more likely to be previously unemployed or uninsured, so this may be capturing some of this effect.

health insurance for the entire first year in column (3), and for those are not married in column (4). Despite some exceptions, the general pattern of results indicates a positive coefficient on the unemployment term, a negative coefficient on the uninsurance term, and a somewhat mixed set of results for the interaction term between unemployment and uninsurance. Although this is potentially consistent with stocking up if an individual anticipates unemployment but does expect to become uninsured; and potential delaying if an individual expects to become uninsured it is also consistent with the aforementioned selection story demonstrated by the mean utilization variables in Table 4-3 and Table A-4-2.

Adding an indicator for moving from private health insurance in the first year to public health insurance any time in the second year does not drastically change the coefficients on the unemployment or uninsurance variables as shown in Table 4-6 and Table 4-7. The coefficient on the term indicating a move from private to public insurance is somewhat mixed with little effect on a physical in the last year, blood pressure checked in the last year, or a PSA test in the last year. With varying degrees of statistical significance, there appear to be signs of delaying for a move from private to public insurance for whether an individual has cholesterol checked in the past year, mammogram in the past year, or a Pap test in the past year. But for flu shots, a future move to public insurance is associated with a significant increase in utilization. However, given the potential endogeneity problems, I focus more on the following results rather than the baseline estimates.

<u>Regressions restricted to 75th percentile of predicted unemployment or uninsurance</u>

The first set of results I focus on are those restricted to the population with a predicted probability of unemployment or uninsurance in the 75^{th} percentile or above (Table 4-8 and Table

4-9). I make this restriction to focus on the population most likely to become unemployed or uninsured in order to reduce the potential endogeneity problem. Although as Table A-4-3 shows, there are still some remaining differences in observables between the populations who become unemployed and/or uninsured. Again, this is likely a limitation of my prediction model, where I have a limited set of observables and condensed forms of the occupation and industry variables with which to predict unemployment or uninsurance, while an individual likely has unobservable information that would help make a better prediction of her own likelihood of unemployment or uninsurance. In terms of predicted unemployment, for an individual that actually becomes unemployed I find a predicted unemployment value of 3.7% for those who become unemployed compared to 1.7% for those who do not become unemployed. Similarly, I find a predicted uninsurance value of 7.4% for those who become uninsured compared to 4.0% for those who do not become uninsured. Although in percentage terms, my predictions are much higher (roughly 45-55% higher for each), the predictions are clearly far from perfect.

For both the general preventive care utilization and cancer screening utilization results, I see fairly consistent patterns. The coefficient on the unemployment term tends to remain positive (indicating stocking up) in nearly every all regressions. While only a handful of the coefficients are statistically significant, many of the positive effects[42] are economically large with increases of 0.3 to 11.4 percentage points for a physical, 2.7 to 6.0 percentage points for a blood pressure check, 1.9 to 6.9 percentage points for a cholesterol check, 0.9 to 4.5 percentage points for a flu shot, 9.1 percentage points for a mammogram, 3.3 to 9.7 percentage points for a Pap test, and 4.5 to 19.8 percentage points for a PSA test[43]. Typically I find the largest effects in the subsample

[42] Although some coefficients are negative they tend to be much fewer and smaller in magnitude.

[43] A note about the interpretation of these results is that the effects can be interpreted as the anticipated effect of actual, future unemployment and/or uninsurance on a higher risk population rather than the effect of the small increase in the marginal probability of becoming unemployed and/or uninsured on preventive care utilization. This is likely due in part to the previously mentioned issue of underestimating the true probability

that has employer provided health insurance throughout the first year. This is consistent with the theoretical explanation that employer provider coverage is likely more generous (McDevitt et al., 2010) and therefore possibly more likely to have a well-liked provider, as well as be attached to a job that may be more conducive to allowing time off to receive preventive care.

However, in terms of future uninsurance I find the opposite effect, namely evidence of delaying. Across nearly each regression I find the coefficient on the uninsurance in year 2 variable to be negative, with many statistically significant or nearly so. The opposite effect seen here, though, is not necessarily surprising compared to the evidence of stocking up I see for the unemployment variable. Since most of the preventive care services are screening services of some type (the exception of course is the flu vaccine and potentially the physician visit), the real benefit is in detecting a serious condition early. However, if an individual expects to be at least temporarily uninsured, then treatment may have a very high out-of-pocket cost (Warren et al., 2008) lowering the value to screening in the current period. This is further supported by the larger negative values for the cancer screening services compared to the general preventive care services, as predicted by the first hypothesis. One final uninsurance result that may seem surprising at first is the relatively smaller magnitude of the delaying in the population that has employer provided health insurance (column 3 in the tables) since this population might be expected to have more generous coverage and be more reactive to the possibility of losing coverage. However, unlike the other regressions, the cross product between unemployment and uninsurance is large and negative for the population with employer provided coverage. Therefore the variables associated with future uninsurance are both negative as would be expected.

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of an individual's unemployment or uninsurance—i.e. an individual likely has additional unobserved information that affects her own prediction. So an individual's behavior in anticipation of unemployment and/or uninsurance is likely in response to larger predicted probabilities.

In terms of the interaction term more generally, I find noisier and typically not statistically significant results for the cross product of unemployment and uninsurance. Except for flu vaccine, the coefficients are negative, although most are not statistically significant. The generally negatively signed interaction terms indicates that the negative insurance effects seems to outweigh the positive unemployment effect.The lack of significance may in part be due to the competing effects attributed to unemployment and uninsurance rendering this interaction term closer to zero. As the results have largely shown, the unemployment and uninsurance variables are differently signed so the interaction term could be including these opposing effects.

In addition to these results, I run separate analyses with a variable indicating whether an individual moves from private insurance in the first year to public insurance in the second year. This is meant to capture the possibility that even if an individual loses employer provided insurance, she does not necessarily become uninsured. As shown in Table 4-10 and Table 4-11, including the private to public insurance indicator variables has little effect on the other coefficients. In terms of the private to public insurance variables themselves, none are statistically significant except in the flu regressions; while in terms of sign, the coefficients tend to be positive for physicals and mammograms but negative for cholesterol check and Pap test. Given the lack of statistical significance and relatively inconsistent results there is not really a clear picture of the effect of an anticipated move to public insurance. However, one thing to note is that compared to the uninsurance terms there is much less evidence of delaying, especially for the cancer screening regressions. This further highlights the possibility that individuals expecting to become uninsured may delay cancer screenings, but anticipated access to public insurance could help mitigate this effect.

As stated in the second hypothesis, the magnitudes of the effect should increase as we move to individuals with higher and higher predicted unemployment and uninsurance (i.e. the coefficients should be larger in the regressions restricted to those in the 75th percentile or greater of predicted unemployment or uninsurance compared to the baseline, and larger still in the regressions restricted to individuals in the 90th percentile or greater). I see this in terms of both the unemployment and insurance coefficients. The stocking up behavior indicated by the positive coefficients on the unemployment main effect variables increase in magnitude as we move from individuals who are less likely to become unemployed to those who are more likely. We see this as the coefficients appear to increase in magnitude as we move from the baseline regressions (Table 4-4 and Table 4-5) to those restricted to individuals in the 75th percentile or greater of predicted unemployment or uninsurance (Table 4-8 and Table 4-9) and finally to those restricted to individuals in the 90th percentile or greater (Table A-4-4 and Table A-4-5). Similarly, the results indicating that increased future uninsurance is associated with increased delaying of preventive care follow a similar pattern. If delaying is an issue, we would expect to see it occurring more in the individuals with the higher levels of predicted uninsurance and in particular individuals who are not married so they do not have an alternate source of insurance, which we do in Table A-4-4 and Table A-4-5. The magnitude of the negative coefficient on future uninsurance typically becomes larger in the analyses restricted to the 75th and then larger still in the analyses restricted to the 90th percentile of predicted unemployment or uninsurance, particularly for indivdiuals who are not married.

Finally, as the third hypothesis states generally I find larger magnitudes in the regressions restricted to the Great Recession time period. Focusing on the population with employer provided insurance, while the magnitude of stocking up on the unemployment variables is

similar, the magnitude of the delaying on the uninsurance variables are larger during the Great Recession time period as Table A-4-10 shows. Given the much smaller sample size, the standard errors are also larger but many of the effects are large enough to remain statistically significant.

Laid off regressions

In the regressions where I split unemployment into layoffs and non-layoffs, I find less evidence of stocking up with the primary evidence supporting stocking up appearing in the population with employer provided health insurance for physicals, blood pressure checks, cholesterol checks, mammograms, and Pap tests (Table 4-12 and Table 4-13). Otherwise across all the regressions, I typically find coefficients on the unemployed due to lay off terms that are smaller in magnitude, compared to the previous unemployment coefficients.

There are a few possible reasons why I may be seeing less evidence of stocking up in the laid off specifications. First, layoffs may be more likely to also include a loss of health insurance making stocking up less likely. In fact, I find support for this in the data where the likelihood of becoming uninsured is twice as high in the laid off population compared to the unemployed but not laid off population—49.4% compared to only 31.6%. Second, there is also the possibility that layoffs may be more unexpected or at least less certain of whether they will occur. While this helps in terms of exogeneity, the unexpected nature may mean individuals have less time to respond thereby leading to a response that is smaller in magnitude. Therefore, it appears the previous results for unemployment are largely driven by the population who becomes unemployed but not through layoff.

Similarly, the previously seen delaying related to the uninsurance variables appears to be largely in the non-layoff population, except in the case of mammograms and Pap tests. Again

that delaying is seen most strongly in cancer screening is not surprising as cancer screening largely derives its benefit if cancer can be caught early and treated, which may have large out-of-pocket costs for individuals who become uninsured. Like the earlier results, these appear to support the first hypothesis that negative insurance effects are more likely in the cancer screening analyses.

I also find support for the third hypothesis that the magnitude of the effects is likely to be largest during the Great Recession in Table A-4-11. Although the sample size is much smaller leading to larger standard error, the effect sizes themselves are much larger. I see fairly large positive coefficients on the laid off variables, showing greater evidence of stocking up that in the other time periods. In part this may because the possibility of layoffs was more anticipated during the Great Recession than in other periods, allowing individuals the time to react. I also see fairly large, but less precisely estimated, negative effects on the uninsurance variables. In all cases the flu shot results end up with different signed results, although this is maybe not surprising as it is the only type preventive care that actually directly prevents a condition.

Finally, together with the initial laid-off regressions, I re-run the regressions including a term for individuals who move from private to public insurance in the second year. Again this has somewhat mixed effects. I see significant delaying related to moving to public insurance for cholesterol checks and Pap tests as well as some modest evidence of delaying for mammograms and PSA tests. I see significant stocking up for flu shots but almost no effect on physicals or blood pressure checks. These results are similar in sign to the regressions run using unemployment and uninsurance in the sample restricted to those in the 75^{th} percentile or greater of predicted unemployment or uninsurance.

Predicted unemployment and uninsurance regressions

The final alternate set of analyses include the full sample but use predicted unemployment and predicted uninsurance in place of the actual values. While this is done as a method of finding an exogenous source variation in the unemployment and uninsurance variables, as described previously, the predictions are relatively weak, lowering my confidence in the estimates[44] which is why I focus primarily on the earlier estimates. Furthermore, using predicted unemployment and uninsurance changes the interpretation slightly. For the predicted values, individuals may have a high predicted value but may not actually become unemployed or uninsured. For example, if an individual thinks she might have a higher probability of becoming unemployed she might work additional hours to try to avoid becoming unemployed or as in the case of a job change might work additionally to obtain a new job.

For the predicted unemployment and uninsurance regressions I run two sets of regressions. First, I use logged predicted unemployment and uninsurance as the regressors of interest. I use logged values so that the coefficients can be interpreted as percent changes in unemployment and uninsurance. And second, I use indicators for an individual being in the 75th percentile and greater for predicted unemployment or uninsurance and their cross-product.

Using logged unemployment and uninsurance, I find modest effects of unemployment on preventive care utilization, although I do find some evidence of significant delaying for having blood pressure, having cholesterol checked, and flu vaccination[45]. However, as Table A-4-6 and Table A-4-7 show, I find more consistent evidence of significant delaying on the predicted uninsurance coefficients for most types of care. The main exception is the positive coefficients

[44] The first-stage F tests have values in the range of 3-4.
[45] I do not have a great explanation for why flu vaccination is so different and in the direction of increased delaying. Part of this may be the weak predictions not accurately capturing the true likelihood of unemployment.

for PSA testing. The effects are on the order of a 1 percent increase in the predicted probability of uninsurance leading to a 1-2 percentage point decrease in the probability of having a physical, having blood pressure checked, or having cholesterol checked. The effect is even larger for flu vaccination, mammography, and Pap testing with effects ranging from a 1 percent increase in the predicted uninsurance rate leading to a 4-8 percentage point decline in utilization. So while I see more limited support for stocking up on the unemployment variables, I continue to see significant evidence of delaying when looking at predicted uninsurance.

The results, however, change somewhat when I use indicators for being in the 75th percentile or greater of predicted unemployment or uninsurance. As Table A-4-8 and Table A-4-9 show, I find significant signs of delaying for the main effect unemployment variables for physicals, blood pressure checks, cholesterol checks, and Pap tests among women with employer-sponsored health insurance. While the unemployment main effect is negative for mammogram screening, it is small and not statistically significant. These results appear to be the opposite of what I find previously. One possible explanation is that since most of these people do not end up actually becoming unemployed, they may be working additional hours to avoid becoming unemployed. Except for the flu shot regressions where I find positive and significant effects of predicted uninsurance, I mostly find negative effects for the other types of care although the effects are mostly not statistically significant.

The other main finding that differs from the previous ones is that I find significant evidence of stocking up on the interaction term, namely those people who have the highest predicted probability of becoming both unemployed and uninsured, in the physical, blood pressure, and cholesterol check regressions. Since blood pressure and cholesterol are often checked during a physical, these may all be happening during one visit. So one possible

explanation for the effect is that individuals who believe they may become unemployed and uninsured stock up on care from their usual provider. It is not exactly clear why this effect exists for predicted unemployment and uninsurance but not actual unemployment and uninsurance (see Table 4-8 and Table 4-9) but the types of people who are at risk for unemployment and uninsurance but are able to avoid it may be different than those who are unable to.

5.9 Discussion

To summarize my findings, I find some evidence that future unemployment alone may lead to stocking up of preventive care services. However, I find stronger and more consistent evidence of delaying related to future uninsurance. Finally, I find limited evidence of an interaction between the two. The lack of a finding about the interaction of the two is likely because of two factors—multiple competing effects all acting at once potentially masking significant individual-level heterogeneity as well as a somewhat limited ability to create strong predictions of unemployment and uninsurance using the available observables. The latter is also hurt by having a more limited number of individuals who become both unemployed and uninsured.

Although I see fairly consistent results within specific sets of regressions, one of the major conclusions is that the effects of future unemployment and uninsurance on preventive care utilization appear to be heterogeneous and complicated. One of the main novel components of this paper is the inclusion of both unemployment and uninsurance in the analyses, whereas previous studies have tended to focus on one or the other. When thinking about concurrent unemployment or uninsurance, focusing on only one of the factors can be an important way to find exogenous variation that can lead to causal inference about the effect of unemployment or uninsurance on utilization. However, when thinking about future unemployment and

uninsurance, it becomes more difficult to disentangle the two. The findings in this study indicate that expectations about both are likely important and may be interwoven in a complicated manner.

The combination of results leads to several potential policy implications. To the extent that employed and insured individuals face primarily a risk of unemployment but not uninsurance (particularly post-ACA), concerns about underutilizing preventive care may not be as critical if these individuals are more likely to stock up on preventive care. A slightly different perspective might also be that given a good potential alternate source of health insurance, the risk of unemployment may be more minimal in its effect on under-utilizing preventive care. Relatedly, it appears the main policy concern is with future uninsurance rather than unemployment, where the risk of becoming uninsured may lead individuals to delay preventive care, particularly cancer screening. This further highlights that potentially the more critical cyclical effect is on making sure individuals are able to maintain insurance coverage more so than employment related effects[46], particularly in terms of preventive care utilization. This means further ensuring that individuals who may become uninsured have knowledge about and are able to sign up for Medicaid coverage where eligible or know about and are able to sign up for marketplace coverage may be critical to ensuring appropriate preventive care utilization.

Certainly one potentially important issue not examined in this study is the effect of anticipated changes in the generosity of health insurance coverage. If an individual does not expect to become uninsured but does anticipate a change in coverage that leads to potentially less generous coverage, one question is how that might affect current period preventive health care utilization. If the effects are similar to the uninsured results, one policy concern might be that

[46] This is certainly not to minimize the negative effects of unemployment, just that they may be more limited in their effect specifically on preventive care utilization.

individuals who expect to have less comprehensive coverage may be more likely to delay preventive care, and in particular cancer screening.. This issue may be particularly relevant in the post-ACA world when individuals are required to have coverage, but given macroeconomic variations may go from more comprehensive employer provided coverage to other less comprehensive forms of coverage.

While I am limited to the publicly available MEPS data for this study, a future extension using restricted access data could provide several interesting improvements. First, with state of residence I could try to compare the results of future state unemployment to the commonly used concurrent state unemployment to better compare results to existing studies. One issue is that some of the negative effects of concurrent unemployment effects could in part be due to prior period stocking up. Second, with state, and actually county, of residence and fully specified industry and occupation codes I could likely dramatically improve the predicted unemployment and uninsurance variables further strengthening the predicted unemployment and uninsurance analyses. Furthermore, improved predicted values could be particularly helpful in better teasing out why there are differences in the results between using actual and predicted unemployment and uninsurance.

5.10 Limitations

Despite running a variety of analyses to try to narrow in on capturing the true effects of anticipated unemployment and uninsurance on preventive care utilization, this study has several important limitations. The first is that I do not know the exact date of unemployment, uninsurance, or preventive care utilization. However, given the format of the utilization questions—utilization in the past year is asked in round 3, and the requirement that all individuals are employed and insured in rounds 1-3, I can be fairly assured that any

unemployment or uninsurance occurs subsequent to the utilization. The main limitation then remains that it is not clear how proximate utilization or unemployment and uninsurance are. However, this largely means the results should be interpreted as the effect of potential but not guaranteed unemployment or uninsurance on utilization. The one obvious exception would be in the case of individuals utilizing care in the short period when they are told they will become unemployed or uninsured. However, this is likely somewhat minimized by the fact that it is difficult to schedule appointments quickly enough (Murray and Berwick, 2003; Farria et al., 2005) to see a provider in between finding out that one will become unemployed or uninsured but before it actually happens.

A related limitation is that one factor that remains unobservable is an individual's expectation about the length of unemployment or uninsurance, which could be an important factor involved in the decision to either stock up or delay utilization of prevent care. The results in Table A-4-10 and Table A-4-11 are suggestive of this, where the coefficients on the unemployment variables are similarly sized or larger, but more strikingly, negative effects on the uninsurance variables are larger when restricting the analyses to the years 2007-2010 (i.e. just before to just after the Great Recession of 2008-2009). This suggests that in a worse recession where unemployment and/or uninsurance may be expected to last longer, the effects of anticipated unemployment or uninsurance may be larger.

Another limitation is that with only two years of MEPS data available, I am not able to run a true individual fixed effects model similar to those by Currie, Duque, and Garfinkel (2014) or Colman and Dave (2015), which might be better able to control for some of the selection concerns. However, a couple of factors may help to mitigate this limitation. By restricting my analysis sample to all individuals who are both employed and insured for the entire first year in

the MEPS data, I control for some of the factors that may be driving both unemployment or uninsurance and utilization of preventive care. In addition, I try to use both predicted unemployment and uninsurance as well as variables for being laid off to create more exogenous sources of variation in unemployment and uninsurance.

Although restricting the sample to those who have the highest likelihood of being unemployed or uninsured is intended to mitigate some of the endogeneity issues, the predictions themselves are not ideal. One of the main limitations is that I can only use observables such as occupation, industry, and some other fairly general individual and job level characteristics. However, individuals likely have other unobservable information that may make their own predictions of how likely they are to become unemployed or uninsured better than the ones I am able to estimate.

Another limitation is the generalizability of the results. The study is restricted to the age 24-64 working population who is employed and insured for the entire first year. The results certainly may not be as applicable to individuals outside of this age range. From a policy perspective, another important limitation is that these results may not apply to individuals with much more volatile employment and insurance statuses or to individuals who are outside of the labor force, groups that may be particularly vulnerable to underutilization of preventive care.

One reason I focus on preventive care in this paper is that it is more elective than most acute types of care and therefore more likely to be affected by anticipated unemployment or uninsurance. Another possible type of care to examine is elective care such as elective surgeries or other procedures. While I do try to run similar analyses for elective care, I get very inconsistent and non-significant results. However, this is largely due to limitations in the level of

detail reported for the ICD-9 codes. In the publicly available MEPS data, most of the specific codes are aggregated to 3-digit codes to preserve respondent confidentiality, which unfortunately makes it very difficult to distinguish between elective and non-elective procedures. Although another future research extension could be to examine this issue using restricted access MEPS data that can provide more fully specified diagnosis and condition codes.

One additional limitation in terms of interpreting results is that I find the most inconsistent results for PSA testing both in terms of variation across specifications and in comparison to the other types of preventive care. However, this may not be surprising given the much less frequent recommendations for this type of screening (little evidence of effectiveness more frequently than every 4 years) and the debate over its effectiveness for low risk men (Qaseem et al., 2013). This means that unlike the other types of care, in any given year it is much less likely that an individual would be recommended to have a PSA test. Furthermore, this means that in a given year utilization in the preceding years has an important bearing on whether an individual should get the screening and therefore may depend less on next year's possible unemployment or uninsurance. There is also the further complication for PSA testing that starting in 2008 there became increasing evidence and pressure to limit the amount of PSA testing[47].

Finally, one concern might be that to some extent the issues discussed in this study may be lessened by the ACA. The intent of the ACA is to both lower rates of uninsurance and improve access to preventive health care, which should reduce the existence of stocking up or delaying recessions if the law works as intended. However, initial estimate suggest only about 1/3 the previously uninsured have gained health insurance following the implementation of the

[47] Although it was not until 2012 that the USPTF gave the final recommendation against PSA testing in men of all ages, somewhat limiting the effect in this study.

ACA (Assistant Secretary for Planning and Evaluation, 2015). In addition, I provide evidence that some of the unemployment or job-related effects (time constraints or concerns about changing providers) may exist independent of anticipated uninsurance. Furthermore, I find some modest, but mixed evidence of delaying among individuals who move from private insurance to Medicaid insurance, an important component of the ACA health insurance expansion. Finally there is the issue that even if the ACA works as intended people's perceptions could still play an important role. For example, if an individual with employer provided health insurance anticipates possible unemployment and believed exchange plans might offer less desirable provider options, she might stock up on care. Therefore even if the ACA should mitigate many of the effects seen in this study it remains an empirical question whether or to what extent that will actually occur.

5.11 Conclusion

I provide the first set of estimates for how both anticipated unemployment and uninsurance may affect preventive health care utilization. Importantly, the two effects appear very different from one another. I find some mixed evidence that increased expected unemployment but not uninsurance may lead to stocking up of preventive care. However, the stronger evidence I find is that individuals with increased expected uninsurance but not unemployment may delay preventive care, especially cancer screening. One important factor may be screening is of more limited value if an individual expects to be uninsured for a period of time because the real value behind screening is early treatment. Especially for the case of cancer, which is particularly costly, an individual may opt to delay screening until her insurance status is more stable. In addition, I see particularly strong evidence of these effects during the time

surrounding the Great Recession, suggesting the effects may be strongest during longer-lasting and more severe economic downturns.

The results suggest that from a policy perspective anticipated uninsurance is the more important focus in terms of ensuring individuals do not under-utilize preventive care. On the other hand, anticipated unemployment may have more limited negative effects and, in fact, individuals may actually stock up on preventive care potentially biasing the estimates of the effect of unemployment on preventive care utilization downwards.

5.12 References

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Table 4-1 Summary statistics of demographic and socioeconomic characteristics by unemployment and uninsurance transition group

	Overall	Employed Insured	Unemployed Insured	Employed Uninsured	Unemployed Uninsured
Age	45.04	45.22	48.16	39.83	41.33
	(11.63)	(11.6)	(13.67)	(10.23)	(11.1)
Female	0.48	0.48	0.65	0.47	0.56
	(0.5)	(0.5)	(0.48)	(0.5)	(0.5)
White	0.77	0.77	0.77	0.75	0.68
	(0.42)	(0.42)	(0.42)	(0.43)	(0.47)
Black	0.15	0.15	0.16	0.18	0.23
	(0.35)	(0.35)	(0.36)	(0.38)	(0.42)
Other race	0.08	0.08	0.08	0.07	0.08
	(0.28)	(0.28)	(0.27)	(0.26)	(0.28)
Hispanic	0.15	0.15	0.16	0.24	0.26
	(0.36)	(0.36)	(0.37)	(0.43)	(0.44)
Education - No degree	0.09	0.08	0.18	0.17	0.17
	(0.28)	(0.28)	(0.38)	(0.37)	(0.38)
Education - High School	0.47	0.47	0.48	0.53	0.55
	(0.5)	(0.5)	(0.5)	(0.5)	(0.5)
Education - Bachelor's degree	0.22	0.22	0.19	0.17	0.14
	(0.42)	(0.42)	(0.39)	(0.38)	(0.35)
Education - Advanced degree	0.12	0.13	0.08	0.06	0.03
	(0.33)	(0.33)	(0.28)	(0.23)	(0.18)
Education - Other	0.10	0.10	0.06	0.07	0.09
	(0.29)	(0.3)	(0.24)	(0.26)	(0.28)
Education - Missing	0.00	0.00	0.01	0.01	0.02
	(0.07)	(0.06)	(0.08)	(0.08)	(0.13)
Marital status - Married	0.68	0.69	0.68	0.56	0.46
	(0.47)	(0.46)	(0.47)	(0.5)	(0.5)
Marital status - Widowed	0.02	0.02	0.06	0.01	0.03
	(0.15)	(0.15)	(0.24)	(0.12)	(0.16)
Marital status - Separated/Divorced	0.14	0.14	0.13	0.19	0.25
	(0.35)	(0.35)	(0.34)	(0.4)	(0.44)
Marital status - Never married	0.15	0.15	0.12	0.23	0.27
	(0.36)	(0.36)	(0.33)	(0.42)	(0.44)
Marital status - Missing	0.00	0.00	0.00	0.00	0.00
	(0)	(0)	(0)	(0)	(0)
Region - Northeast	0.17	0.17	0.19	0.14	0.18
	(0.38)	(0.38)	(0.39)	(0.34)	(0.38)
Region - Midwest	0.23	0.24	0.22	0.19	0.20
	(0.42)	(0.42)	(0.42)	(0.39)	(0.4)
Region - South	0.35	0.35	0.35	0.40	0.36
	(0.48)	(0.48)	(0.48)	(0.49)	(0.48)
Region - West	0.25	0.24	0.24	0.27	0.26
	(0.43)	(0.43)	(0.43)	(0.45)	(0.44)
Fair/poor self-reported health in any base year round	0.15	0.15	0.30	0.18	0.30
	(0.36)	(0.36)	(0.46)	(0.39)	(0.46)
Missing self-reported health	0.00	0.00	0.00	0.00	0.00
	(0.03)	(0.03)	(0)	(0.04)	(0)
Any report of limitation of daily	0.03	0.03	0.12	0.03	0.09

	Overall	Employed Insured	Unemployed Insured	Employed Uninsured	Unemployed Uninsured
activities in base year round					
	(0.17)	(0.16)	(0.33)	(0.17)	(0.29)
Missing reported limitation of daily activities	0.00	0.00	0.01	0.01	0.01
	(0.06)	(0.05)	(0.08)	(0.09)	(0.08)
Non-rural residence	0.84	0.84	0.85	0.85	0.86
	(0.37)	(0.37)	(0.36)	(0.36)	(0.35)
Family income <100% FPL	0.03	0.03	0.09	0.09	0.07
	(0.17)	(0.16)	(0.29)	(0.29)	(0.26)
Family income 100% - <125% FPL	0.02	0.02	0.03	0.05	0.04
	(0.14)	(0.13)	(0.18)	(0.21)	(0.19)
Family income 125% - <200% FPL	0.09	0.09	0.15	0.18	0.16
	(0.29)	(0.28)	(0.35)	(0.39)	(0.36)
Family income 200% - <400% FPL	0.35	0.34	0.29	0.38	0.44
	(0.48)	(0.47)	(0.45)	(0.49)	(0.5)
Family income >=400% FPL	0.51	0.52	0.44	0.30	0.30
	(0.5)	(0.5)	(0.5)	(0.46)	(0.46)
Hours worked	40.61	40.72	33.10	40.11	39.94
	(11.72)	(11.67)	(14.55)	(11.8)	(8.73)
Hours worked missing	0.01	0.01	0.01	0.01	0.01
	(0.08)	(0.08)	(0.11)	(0.09)	(0.08)
Hourly wage	16.97	17.19	13.39	12.77	14.29
	(12.83)	(12.93)	(10.82)	(10.3)	(8.4)
Number of employees	150.78	153.14	118.68	98.81	140.12
	(186.94)	(187.85)	(177.13)	(155.71)	(177.56)
Current smoker	0.16	0.16	0.18	0.21	0.31
	(0.37)	(0.36)	(0.38)	(0.41)	(0.46)
Smoking status missing	0.01	0.01	0.02	0.01	0.01
	(0.12)	(0.12)	(0.15)	(0.11)	(0.08)
Observations	45,573	43,178	507	1,587	301

Means (SD);
* All values are from the baseline period (year 1)

Table 4-2 Summary statistics of comorbidities and spouse comorbidities by unemployment and uninsurance transition group

	Overall	Employed Insured	Unemployed Insured	Employed Uninsured	Unemployed Uninsured
Cancer	0.06	0.06	0.09	0.04	0.04
	(0.24)	(0.24)	(0.28)	(0.2)	(0.2)
Diabetes	0.06	0.06	0.11	0.05	0.09
	(0.24)	(0.24)	(0.31)	(0.23)	(0.28)
Disorders of lipid metabolism	0.14	0.14	0.16	0.08	0.11
	(0.34)	(0.34)	(0.37)	(0.28)	(0.31)
Cardiovascular disease	0.23	0.23	0.33	0.14	0.27
	(0.42)	(0.42)	(0.47)	(0.35)	(0.45)
COPD	0.04	0.04	0.07	0.03	0.04
	(0.21)	(0.21)	(0.25)	(0.18)	(0.19)
Asthma	0.04	0.04	0.08	0.04	0.05
	(0.2)	(0.2)	(0.27)	(0.2)	(0.22)
Joint disorders	0.22	0.22	0.28	0.17	0.25
	(0.41)	(0.41)	(0.45)	(0.38)	(0.43)
Anxiety disorders	0.07	0.07	0.11	0.08	0.11
	(0.25)	(0.25)	(0.31)	(0.27)	(0.32)
Mood disorders	0.05	0.05	0.08	0.05	0.10
	(0.21)	(0.21)	(0.28)	(0.22)	(0.3)
Cancer (spouse)	0.02	0.02	0.02	0.01	0.02
	(0.14)	(0.14)	(0.15)	(0.08)	(0.13)
Diabetes (spouse)	0.02	0.02	0.05	0.02	0.02
	(0.15)	(0.15)	(0.21)	(0.13)	(0.14)
Disorders of lipid metabolism (spouse)	0.05	0.05	0.08	0.03	0.04
	(0.23)	(0.23)	(0.27)	(0.17)	(0.19)
Cardiovascular disease (spouse)	0.07	0.07	0.09	0.04	0.05
	(0.26)	(0.26)	(0.29)	(0.2)	(0.22)
COPD (spouse)	0.01	0.01	0.02	0.01	0.01
	(0.11)	(0.11)	(0.12)	(0.08)	(0.11)
Asthma (spouse)	0.01	0.01	0.01	0.01	0.00
	(0.11)	(0.12)	(0.08)	(0.09)	(0)
Joint disorders (spouse)	0.07	0.07	0.09	0.04	0.06
	(0.26)	(0.26)	(0.29)	(0.2)	(0.24)
Anxiety disorders (spouse)	0.02	0.02	0.02	0.02	0.01
	(0.14)	(0.14)	(0.15)	(0.13)	(0.11)
Mood disorders (spouse)	0.02	0.02	0.01	0.02	0.02
	(0.14)	(0.14)	(0.12)	(0.13)	(0.14)
N	45,573	43,178	507	1,587	301

Means (SD); * All values are from the baseline period (year 1)

Table 4-3 Summary statistics of preventive care utilization by unemployment and uninsurance transition group

	Overall	Employed Insured	Unemployed Insured	Employed Uninsured	Unemployed Uninsured
Physical	0.63	0.64	0.69	0.53	0.62
	(0.48)	(0.48)	(0.46)	(0.5)	(0.49)
BP check	0.83	0.83	0.88	0.76	0.83
	(0.38)	(0.38)	(0.32)	(0.43)	(0.37)
Cholesterol checked	0.59	0.60	0.66	0.46	0.56
	(0.49)	(0.49)	(0.47)	(0.5)	(0.5)
Flu shot	0.30	0.31	0.36	0.19	0.25
	(0.46)	(0.46)	(0.48)	(0.39)	(0.43)
Mammogram	0.50	0.50	0.50	0.38	0.28
	(0.5)	(0.5)	(0.5)	(0.49)	(0.45)
Pap test	0.66	0.66	0.66	0.61	0.60
	(0.47)	(0.47)	(0.48)	(0.49)	(0.49)
PSA test	0.34	0.35	0.50	0.22	0.31
	(0.48)	(0.48)	(0.5)	(0.41)	(0.47)
Observations	45,573	43,178	507	1,587	301

Means (SD)

* All values are utilization within the past year unless otherwise noted and based on reports from the baseline period (year 1)

Table 4-4 Baseline regressions estimates for general preventive care utilization

	(1)	(2)	(3)	(4)
		Physical in Past Year		
Unemployed year 2	0.003	0.005	0.062**	0.053*
	(0.015)	(0.017)	(0.020)	(0.026)
Uninsured year 2	-0.030*	-0.027*	-0.027	-0.011
	(0.012)	(0.013)	(0.017)	(0.019)
Unemployed x Uninsured year 2	0.025	0.025	-0.029	-0.091
	(0.033)	(0.034)	(0.038)	(0.049)
N	44,721	41,775	30,426	14,191
		Blood Pressure Checked in Past Year		
Unemployed year 2	0.004	-0.001	0.02	0.015
	(0.011)	(0.012)	(0.015)	(0.019)
Uninsured year 2	-0.019	-0.019	-0.02	-0.009
	(0.011)	(0.011)	(0.015)	(0.016)
Unemployed x Uninsured year 2	0.021	0.027	0.007	-0.020
	(0.025)	(0.026)	(0.030)	(0.037)
N	44,871	41,904	30,515	14,229
		Cholesterol Checked in Past Year		
Unemployed year 2	-0.001	-0.003	0.022	0.031
	(0.015)	(0.017)	(0.021)	(0.027)
Uninsured year 2	-0.043***	-0.041**	-0.041*	-0.034
	(0.012)	(0.013)	(0.017)	(0.019)
Unemployed x Uninsured year 2	0.042	0.052	0.027	-0.031
	(0.032)	(0.034)	(0.039)	(0.049)
N	43,348	40,508	29,454	13,774
		Flu Shot in Past Year		
Unemployed year 2	0.044**	0.026	0.021	0.058
	(0.017)	(0.019)	(0.025)	(0.031)
Uninsured year 2	-0.030**	-0.026*	-0.026	-0.044**
	(0.010)	(0.011)	(0.014)	(0.015)
Unemployed x Uninsured year 2	-0.022	-0.010	-0.001	-0.017
	(0.030)	(0.032)	(0.038)	(0.046)
N	44,934	41,983	30,588	14,293

* p<0.05, ** p<0.01, *** p<0.001

(1) Baseline (2) Private insurance entire year 1 (3) Employer provided insurance entire year 1 (4) Not married

All regressions also control for age, age, sex, race/ethnicity, education, marital status, income, number of employees in firm, region, urban residence, self-reported health status, presence of physical limitations of work, smoking status, a series of risk preference measures, and presence of comorbidities both in oneself and in one's spouse.

Table 4-5 Baseline regressions estimates for cancer screening utilization

	(1)	(2)	(3)	(4)
		Mammogram in Past Year		
Unemployed year 2	0.024	0.018	0.051	0.037
	(0.026)	(0.028)	(0.037)	(0.043)
Uninsured year 2	-0.060*	-0.069*	-0.051	-0.067
	(0.026)	(0.028)	(0.040)	(0.039)
Unemployed x Uninsured year 2	-0.081	-0.067	-0.123	-0.113
	(0.062)	(0.064)	(0.075)	(0.092)
N	14,311	13,296	8,913	5,255
		Pap Test in Past Year		
Unemployed year 2	0.020	-0.005	0.031	0.081*
	(0.023)	(0.025)	(0.036)	(0.038)
Uninsured year 2	-0.036	-0.044*	-0.042	-0.005
	(0.018)	(0.020)	(0.028)	(0.025)
Unemployed x Uninsured year 2	-0.024	-0.001	-0.048	-0.186**
	(0.047)	(0.050)	(0.061)	(0.069)
N	21,057	19,481	13,371	8,308
		PSA Test in Past Year		
Unemployed year 2	0.025	0.042	0.03	0.041
	(0.032)	(0.037)	(0.045)	(0.089)
Uninsured year 2	-0.044	-0.048	-0.052	-0.127
	(0.038)	(0.040)	(0.053)	(0.074)
Unemployed x Uninsured year 2	0.065	0.056	0.040	0.142
	(0.083)	(0.086)	(0.099)	(0.151)
N	7,794	7,063	5,023	1,523

* p<0.05, ** p<0.01, *** p<0.001

(1) Baseline (2) Private insurance entire year 1 (3) Employer provided insurance entire year 1 (4) Not married

All regressions also control for age, age, sex, race/ethnicity, education, marital status, income, number of employees in firm, region, urban residence, self-reported health status, presence of physical limitations of work, smoking status, a series of risk preference measures, and presence of comorbidities both in oneself and in one's spouse.

Table 4-6 Baseline regressions estimates including move from private to public health insurance for general preventive care utilization

	(1)	(2)	(3)	(4)
		Physical in Past Year		
Unemployed year 2	0.003	0.005	0.061**	0.051
	(0.015)	(0.017)	(0.020)	(0.026)
Uninsured year 2	-0.030*	-0.027*	-0.027	-0.011
	(0.012)	(0.013)	(0.017)	(0.019)
Unemployed x Uninsured year 2	0.025	0.025	-0.029	-0.090
	(0.033)	(0.034)	(0.039)	(0.049)
Private (year 1) to public health insurance (year 2)	0.001	-0.002	0.002	0.026
	(0.009)	(0.009)	(0.012)	(0.016)
N	44,721	41,775	30,426	14,191
		Blood Pressure Checked in Past Year		
Unemployed year 2	0.004	-0.001	0.019	0.015
	(0.011)	(0.012)	(0.015)	(0.019)
Uninsured year 2	-0.019	-0.019	-0.02	-0.009
	(0.011)	(0.011)	(0.015)	(0.016)
Unemployed x Uninsured year 2	0.021	0.026	0.007	-0.020
	(0.025)	(0.026)	(0.030)	(0.037)
Private (year 1) to public health insurance (year 2)	-0.004	-0.002	0.003	-0.003
	(0.007)	(0.007)	(0.009)	(0.012)
N	44,871	41,904	30,515	14,229
		Cholesterol Checked in Past Year		
Unemployed year 2	0.002	0.003	0.027	0.032
	(0.015)	(0.017)	(0.021)	(0.027)
Uninsured year 2	-0.043***	-0.042**	-0.041*	-0.034
	(0.012)	(0.013)	(0.017)	(0.019)
Unemployed x Uninsured year 2	0.040	0.048	0.022	-0.031
	(0.032)	(0.034)	(0.039)	(0.049)
Private (year 1) to public health insurance (year 2)	-0.029**	-0.039***	-0.032*	-0.005
	(0.009)	(0.009)	(0.013)	(0.016)
N	43,348	40,508	29,454	13,774

	Flu Shot in Past Year			
Unemployed year 2	0.036*	0.012	0.007	0.051
	(0.017)	(0.019)	(0.025)	(0.031)
Uninsured year 2	-0.029**	-0.026*	-0.026	-0.044**
	(0.010)	(0.011)	(0.014)	(0.015)
Unemployed x Uninsured year 2	-0.018	<0.001	0.010	-0.013
	(0.030)	(0.032)	(0.038)	(0.046)
Private (year 1) to public health insurance (year 2)	0.080***	0.096***	0.078***	0.077***
	(0.011)	(0.011)	(0.015)	(0.019)
N	44,934	41,983	30,588	14,293

* p<0.05, ** p<0.01, *** p<0.001

(1) Baseline (2) Private insurance entire year 1 (3) Employer provided insurance entire year 1 (4) Not married

All regressions also control for age, age, sex, race/ethnicity, education, marital status, income, number of employees in firm, region, urban residence, self-reported health status, presence of physical limitations of work, smoking status, a series of risk preference measures, and presence of comorbidities both in oneself and in one's spouse.

Table 4-7 Baseline regressions estimates including move from private to public health insurance for cancer screening utilization

	(1)	(2)	(3)	(4)
		Mammogram in Past Year		
Unemployed year 2	0.025	0.022	0.053	0.035
	(0.026)	(0.028)	(0.038)	(0.044)
Uninsured year 2	-0.060*	-0.070*	-0.052	-0.067
	(0.026)	(0.028)	(0.040)	(0.039)
Unemployed x Uninsured year 2	-0.081	-0.070	-0.125	-0.112
	(0.062)	(0.064)	(0.075)	(0.092)
Private (year 1) to public health insurance (year 2)	-0.007	-0.031	-0.014	0.027
	(0.017)	(0.018)	(0.024)	(0.026)
N	14,311	13,296	8,913	5,255
		Pap Test in Past Year		
Unemployed year 2	0.023	0.001	0.038	0.084*
	(0.023)	(0.025)	(0.036)	(0.038)
Uninsured year 2	-0.036*	-0.044*	-0.042	-0.006
	(0.018)	(0.020)	(0.028)	(0.025)
Unemployed x Uninsured year 2	-0.025	-0.005	-0.054	-0.186**
	(0.047)	(0.051)	(0.061)	(0.070)
Private (year 1) to public health insurance (year 2)	-0.036*	-0.050**	-0.035	-0.028
	(0.016)	(0.016)	(0.022)	(0.023)
N	21,057	19,481	13,371	8,308
		PSA Test in Past Year		
Unemployed year 2	0.025	0.042	0.030	0.037
	(0.032)	(0.037)	(0.045)	(0.089)
Uninsured year 2	-0.044	-0.049	-0.053	-0.126
	(0.038)	(0.040)	(0.053)	(0.074)
Unemployed x Uninsured year 2	0.064	0.056	0.040	0.144
	(0.083)	(0.086)	(0.099)	(0.151)
Private (year 1) to public health insurance (year 2)	0.012	-0.005	-0.014	0.030
	(0.019)	(0.022)	(0.028)	(0.044)
N	7,794	7,063	5,023	1,523

* p<0.05, ** p<0.01, *** p<0.001

(1) Baseline (2) Private insurance entire year 1 (3) Employer provided insurance entire year 1 (4) Not married

All regressions also control for age, age, sex, race/ethnicity, education, marital status, income, number of employees in firm, region, urban residence, self-reported health status, presence of physical limitations of work, smoking status, a series of risk preference measures, and presence of comorbidities both in oneself and in one's spouse.

Table 4-8 Regression estimates for general preventive care utilization restricted to the 75th percentile of predicted unemployment or uninsurance

	(1)	(2)	(3)	(4)
		Physical in Past Year		
Unemployed year 2	-0.006	0.003	0.114**	0.022
	(0.026)	(0.032)	(0.044)	(0.040)
Uninsured year 2	-0.028	-0.023	-0.015	-0.02
	(0.016)	(0.017)	(0.022)	(0.022)
Unemployed x Uninsured year 2	-0.014	-0.022	-0.145*	-0.075
	(0.044)	(0.049)	(0.061)	(0.062)
N	16,745	14,762	10,258	7,750
		Blood Pressure Checked in Past Year		
Unemployed year 2	0.027	0.033	0.060*	0.031
	(0.017)	(0.020)	(0.030)	(0.030)
Uninsured year 2	-0.010	-0.01	-0.005	-0.026
	(0.014)	(0.015)	(0.020)	(0.019)
Unemployed x Uninsured year 2	-0.031	-0.036	-0.072	-0.015
	(0.033)	(0.036)	(0.046)	(0.047)
N	16,832	14,835	10,315	7,779
		Cholesterol Checked in Past Year		
Unemployed year 2	0.019	0.024	0.069	0.031
	(0.025)	(0.031)	(0.048)	(0.040)
Uninsured year 2	-0.031*	-0.024	-0.032	-0.024
	(0.015)	(0.017)	(0.022)	(0.021)
Unemployed x Uninsured year 2	-0.020	-0.015	-0.063	-0.045
	(0.043)	(0.049)	(0.063)	(0.062)
N	16,160	14,250	9,875	7,500
		Flu Shot in Past Year		
Unemployed year 2	0.045	0.009	-0.019	0.035
	(0.025)	(0.031)	(0.049)	(0.041)
Uninsured year 2	-0.036**	-0.032*	-0.026	-0.052**
	(0.012)	(0.013)	(0.017)	(0.016)
Unemployed x Uninsured year 2	-0.021	0.006	0.039	0.013
	(0.039)	(0.044)	(0.060)	(0.056)
N	16,909	14,910	10,377	7,832

* p<0.05, ** p<0.01, *** p<0.001

(1) Baseline (2) Private insurance entire year 1 (3) Employer provided insurance entire year 1 (4) Not married

All regressions also control for age, age, sex, race/ethnicity, education, marital status, income, number of employees in firm, region, urban residence, self-reported health status, presence of physical limitations of work, smoking status, a series of risk preference measures, and presence of comorbidities both in oneself and in one's spouse.

Table 4-9 Regression estimates for general cancer screening utilization restricted to the 75th percentile of predicted unemployment or uninsurance

	(1)	(2)	(3)	(4)
		Mammogram in Past Year		
Unemployed year 2	<0.001	-0.012	0.091	-0.010
	(0.043)	(0.051)	(0.073)	(0.069)
Uninsured year 2	-0.068*	-0.086*	-0.051	-0.072
	(0.034)	(0.039)	(0.057)	(0.047)
Unemployed x Uninsured year 2	-0.104	-0.076	-0.227*	-0.078
	(0.083)	(0.091)	(0.112)	(0.118)
N	5,592	4,875	3,022	2,637
		Pap Test in Past Year		
Unemployed year 2	0.033	-0.020	0.097	0.081
	(0.033)	(0.039)	(0.061)	(0.051)
Uninsured year 2	-0.018	-0.024	-0.030	-0.005
	(0.022)	(0.026)	(0.036)	(0.028)
Unemployed x Uninsured year 2	-0.062	-0.018	-0.146	-0.148
	(0.059)	(0.066)	(0.086)	(0.081)
N	9,468	8,225	5,378	4,836
		PSA Test in Past Year		
Unemployed year 2	0.080	0.198*	0.045	0.080
	(0.066)	(0.086)	(0.164)	(0.129)
Uninsured year 2	-0.110	-0.092	-0.033	-0.021
	(0.068)	(0.079)	(0.112)	(0.127)
Unemployed x Uninsured year 2	-0.051	-0.176	-0.133	-0.162
	(0.143)	(0.159)	(0.225)	(0.222)
N	1,400	1,151	767	462

* p<0.05, ** p<0.01, *** p<0.001

(1) Baseline (2) Private insurance entire year 1 (3) Employer provided insurance entire year 1 (4) Not married

All regressions also control for age, age, sex, race/ethnicity, education, marital status, income, number of employees in firm, region, urban residence, self-reported health status, presence of physical limitations of work, smoking status, a series of risk preference measures, and presence of comorbidities both in oneself and in one's spouse.

Table 4-10 Regression estimates for general preventive care utilization restricted to the 75th percentile of predicted unemployment or uninsurance including move from private to public insurance

	(1)	(2)	(3)	(4)
		Physical in Past Year		
Unemployed year 2	-0.007	<0.001	0.107*	0.022
	(0.026)	(0.032)	(0.044)	(0.040)
Uninsured year 2	-0.028	-0.023	-0.014	-0.020
	(0.016)	(0.017)	(0.022)	(0.022)
Unemployed x Uninsured year 2	-0.014	-0.020	-0.141*	-0.078
	(0.044)	(0.049)	(0.061)	(0.062)
Private (year 1) to public health insurance (year 2)	0.020	0.025	0.045*	0.032
	(0.015)	(0.016)	(0.022)	(0.023)
N	16,745	14,762	10,258	7,750
		Blood Pressure Checked in Past Year		
Unemployed year 2	0.028	0.033	0.057	0.031
	(0.017)	(0.020)	(0.030)	(0.030)
Uninsured year 2	-0.01	-0.01	-0.005	-0.026
	(0.014)	(0.015)	(0.020)	(0.019)
Unemployed x Uninsured year 2	-0.031	-0.036	-0.070	-0.014
	(0.033)	(0.036)	(0.046)	(0.047)
Private (year 1) to public health insurance (year 2)	-0.006	-0.001	0.021	-0.006
	(0.012)	(0.012)	(0.017)	(0.017)
N	16,832	14,835	10,315	7,779
		Cholesterol Checked in Past Year		
Unemployed year 2	0.020	0.027	0.073	0.031
	(0.025)	(0.031)	(0.047)	(0.040)
Uninsured year 2	-0.031*	-0.024	-0.032	-0.024
	(0.015)	(0.017)	(0.022)	(0.021)
Unemployed x Uninsured year 2	-0.019	-0.016	-0.066	-0.045
	(0.044)	(0.049)	(0.063)	(0.062)
Private (year 1) to public health insurance (year 2)	-0.014	-0.027	-0.027	0.010
	(0.016)	(0.017)	(0.023)	(0.023)
N	16,160	14,250	9,875	7,500

		Flu Shot in Past Year		
Unemployed year 2	0.041	-0.003	-0.027	0.035
	(0.025)	(0.031)	(0.049)	(0.041)
Uninsured year 2	-0.035**	-0.032*	-0.025	-0.052**
	(0.012)	(0.013)	(0.017)	(0.016)
Unemployed x Uninsured year 2	-0.022	0.011	0.043	0.007
	(0.039)	(0.044)	(0.060)	(0.056)
Private (year 1) to public health insurance (year 2)	0.070***	0.091***	0.051*	0.079**
	(0.017)	(0.018)	(0.025)	(0.026)
N	16,909	14,910	10,377	7,832

* p<0.05, ** p<0.01, *** p<0.001

(1) Baseline (2) Private insurance entire year 1 (3) Employer provided insurance entire year 1 (4) Not married

All regressions also control for age, age, sex, race/ethnicity, education, marital status, income, number of employees in firm, region, urban residence, self-reported health status, presence of physical limitations of work, smoking status, a series of risk preference measures, and presence of comorbidities both in oneself and in one's spouse.

Table 4-11 Regression estimates for cancer screening utilization restricted to the 75th percentile of predicted unemployment or uninsurance including move from private to public insurance

	(1)	(2)	(3)	(4)
		Mammogram in Past Year		
Unemployed year 2	-0.001	-0.012	0.081	-0.008
	(0.043)	(0.051)	(0.073)	(0.070)
Uninsured year 2	-0.067*	-0.086*	-0.049	-0.071
	(0.034)	(0.039)	(0.057)	(0.047)
Unemployed x Uninsured year 2	-0.105	-0.076	-0.224*	-0.086
	(0.083)	(0.091)	(0.113)	(0.119)
Private (year 1) to public health insurance (year 2)	0.020	0.002	0.066	0.045
	(0.026)	(0.028)	(0.040)	(0.036)
N	5,592	4,875	3,022	2,637
		Pap Test in Past Year		
Unemployed year 2	0.034	-0.018	0.095	0.081
	(0.033)	(0.039)	(0.062)	(0.050)
Uninsured year 2	-0.018	-0.024	-0.030	-0.005
	(0.022)	(0.026)	(0.036)	(0.028)
Unemployed x Uninsured year 2	-0.062	-0.018	-0.145	-0.146
	(0.059)	(0.066)	(0.086)	(0.081)
Private (year 1) to public health insurance (year 2)	-0.004	-0.016	0.006	-0.022
	(0.023)	(0.024)	(0.034)	(0.031)
N	9,468	8,225	5,378	4,836
		PSA Test in Past Year		
Unemployed year 2	0.080	0.203*	0.055	0.077
	(0.066)	(0.087)	(0.163)	(0.128)
Uninsured year 2	-0.110	-0.094	-0.036	-0.02
	(0.068)	(0.079)	(0.112)	(0.127)
Unemployed x Uninsured year 2	-0.052	-0.176	-0.140	-0.160
	(0.142)	(0.161)	(0.226)	(0.221)
Private (year 1) to public health insurance (year 2)	0.006	-0.029	-0.062	0.045
	(0.041)	(0.055)	(0.071)	(0.082)
N	1,400	1,151	767	462

* p<0.05, ** p<0.01, *** p<0.001

(1) Baseline (2) Private insurance entire year 1 (3) Employer provided insurance entire year 1 (4) Not married

All regressions also control for age, age, sex, race/ethnicity, education, marital status, income, number of employees in firm, region, urban residence, self-reported health status, presence of physical limitations of work, smoking status, a series of risk preference measures, and presence of comorbidities both in oneself and in one's spouse.

Table 4-12 Regression estimates for general preventive care utilization by laid off and non-laid off unemployment

	(1)	(2)	(3)	(4)
		Physical in Past Year		
Laid off year 2	-0.031	-0.024	0.046	0.021
	(0.042)	(0.047)	(0.056)	(0.073)
Uninsured due to lay off year 2	0.041	0.041	-0.019	-0.065
	(0.060)	(0.064)	(0.072)	(0.095)
Unemployed (not laid off) year 2	0.008	0.011	0.064**	0.059*
	(0.016)	(0.018)	(0.022)	(0.027)
Uninsured (not due to lay off) year 2	-0.030*	-0.027*	-0.027	-0.012
	(0.012)	(0.013)	(0.017)	(0.019)
N	44,721	41,775	30,426	14,191
		Blood Pressure Checked in Past Year		
Laid off year 2	0.010	0.029	0.050	0.003
	(0.028)	(0.030)	(0.039)	(0.053)
Uninsured due to lay off year 2	0.032	0.015	0.002	0.022
	(0.042)	(0.044)	(0.051)	(0.070)
Unemployed (not laid off) year 2	<0.001	-0.008	0.010	0.013
	(0.011)	(0.013)	(0.016)	(0.020)
Uninsured (not due to lay off) year 2	-0.019	-0.018	-0.02	-0.009
	(0.011)	(0.011)	(0.015)	(0.016)
N	44,871	41,904	30,515	14,229
		Cholesterol Checked in Past Year		
Laid off year 2	-0.002	0.016	0.021	-0.002
	(0.039)	(0.043)	(0.056)	(0.073)
Uninsured due to lay off year 2	0.013	0.019	0.018	-0.012
	(0.058)	(0.062)	(0.072)	(0.095)
Unemployed (not laid off) year 2	-0.002	-0.007	0.021	0.038
	(0.016)	(0.018)	(0.023)	(0.028)
Uninsured (not due to lay off) year 2	-0.043***	-0.041**	-0.041*	-0.034
	(0.012)	(0.013)	(0.017)	(0.019)
N	43,348	40,508	29,454	13,774

	Flu Shot in Past Year			
Laid off year 2	0.018	-0.029	-0.063	0.069
	(0.040)	(0.043)	(0.056)	(0.069)
Uninsured due to lay off year 2	-0.012	0.036	0.075	-0.057
	(0.056)	(0.059)	(0.070)	(0.087)
Unemployed (not laid off) year 2	0.049**	0.035	0.039	0.056
	(0.018)	(0.021)	(0.028)	(0.034)
Uninsured (not due to lay off) year 2	-0.030**	-0.026*	-0.027	-0.044**
	(0.010)	(0.011)	(0.014)	(0.015)
N	44,948	41,995	30,590	14,293

* p<0.05, ** p<0.01, *** p<0.001

(1) Baseline (2) Private insurance entire year 1 (3) Employer provided insurance entire year 1 (4) Not married

All regressions also control for age, age, sex, race/ethnicity, education, marital status, income, number of employees in firm, region, urban residence, self-reported health status, presence of physical limitations of work, smoking status, a series of risk preference measures, and presence of comorbidities both in oneself and in one's spouse.

Table 4-13 Regression estimates for cancer screening utilization by laid off and non-laid off unemployment

	(1)	(2)	(3)	(4)
		Mammogram in Past Year		
Laid off year 2	0.022	0.032	0.210*	0.010
	(0.071)	(0.080)	(0.085)	(0.116)
Uninsured due to lay off year 2	-0.159	-0.156	-0.320*	-0.204
	(0.115)	(0.123)	(0.127)	(0.179)
Unemployed (not laid off) year 2	0.026	0.016	0.029	0.045
	(0.027)	(0.030)	(0.040)	(0.046)
Uninsured (not due to lay off) year 2	-0.059*	-0.067*	-0.051	-0.067
	(0.026)	(0.028)	(0.040)	(0.039)
N	14,311	13,296	8,913	5,255
		Pap Test in Past Year		
Laid off year 2	-0.006	-0.018	0.111	0.077
	(0.057)	(0.063)	(0.071)	(0.079)
Uninsured due to lay off year 2	-0.051	-0.026	-0.169	-0.245
	(0.090)	(0.095)	(0.102)	(0.127)
Unemployed (not laid off) year 2	0.023	-0.003	0.014	0.081
	(0.024)	(0.027)	(0.040)	(0.042)
Uninsured (not due to lay off) year 2	-0.036*	-0.044*	-0.042	-0.005
	(0.018)	(0.020)	(0.028)	(0.025)
N	21,057	19,481	13,371	8,308
		PSA Test in Past Year		
Laid off year 2	-0.006	-0.002	-0.017	-0.129
	(0.079)	(0.090)	(0.127)	(0.189)
Uninsured due to lay off year 2	0.176	0.179	0.198	0.163
	(0.130)	(0.136)	(0.166)	(0.237)
Unemployed (not laid off) year 2	0.030	0.050	0.036	0.084
	(0.035)	(0.040)	(0.048)	(0.099)
Uninsured (not due to lay off) year 2	-0.044	-0.048	-0.052	-0.128
	(0.038)	(0.040)	(0.053)	(0.074)
N	7,794	7,063	5,023	1,523

* p<0.05, ** p<0.01, *** p<0.001

(1) Baseline (2) Private insurance entire year 1 (3) Employer provided insurance entire year 1 (4) Not married

All regressions also control for age, age, sex, race/ethnicity, education, marital status, income, number of employees in firm, region, urban residence, self-reported health status, presence of physical limitations of work, smoking status, a series of risk preference measures, and presence of comorbidities both in oneself and in one's spouse.

Table 4-14 Regression estimates for general preventive care utilization by laid off and non-laid off unemployment including move from private to public insurance

	(1)	(2)	(3)	(4)
		Physical in Past Year		
Laid off year 2	-0.031	-0.024	0.046	0.021
	(0.042)	(0.047)	(0.056)	(0.073)
Uninsured due to lay off year 2	0.041	0.041	-0.019	-0.066
	(0.060)	(0.064)	(0.072)	(0.095)
Unemployed (not laid off) year 2	0.008	0.011	0.063**	0.056*
	(0.016)	(0.018)	(0.022)	(0.027)
Uninsured (not due to lay off) year 2	-0.030*	-0.027*	-0.027	-0.011
	(0.012)	(0.013)	(0.017)	(0.019)
Private (year 1) to public health insurance (year 2)	<0.001	-0.002	0.002	0.026
	(0.009)	(0.009)	(0.012)	(0.016)
N	44,721	41,775	30,426	14,191
		Blood Pressure Checked in Past Year		
Laid off year 2	0.010	0.029	0.050	0.003
	(0.028)	(0.030)	(0.039)	(0.053)
Uninsured due to lay off year 2	0.032	0.015	0.002	0.022
	(0.042)	(0.044)	(0.051)	(0.070)
Unemployed (not laid off) year 2	0.001	-0.008	0.01	0.013
	(0.012)	(0.013)	(0.016)	(0.020)
Uninsured (not due to lay off) year 2	-0.019	-0.018	-0.02	-0.009
	(0.011)	(0.011)	(0.015)	(0.016)
Private (year 1) to public health insurance (year 2)	-0.003	-0.001	0.004	-0.003
	(0.007)	(0.007)	(0.009)	(0.012)
N	44,871	41,904	30,515	14,229
		Cholesterol Checked in Past Year		
Laid off year 2	-0.002	0.016	0.021	-0.002
	(0.039)	(0.043)	(0.055)	(0.073)
Uninsured due to lay off year 2	0.014	0.019	0.019	-0.012
	(0.058)	(0.062)	(0.072)	(0.096)
Unemployed (not laid off) year 2	0.001	<0.001	0.028	0.039
	(0.016)	(0.018)	(0.023)	(0.028)
Uninsured (not due to lay off) year 2	-0.043***	-0.042**	-0.041*	-0.034
	(0.012)	(0.013)	(0.017)	(0.019)
Private (year 1) to public health insurance (year 2)	-0.029**	-0.039***	-0.032*	-0.006
	(0.009)	(0.009)	(0.013)	(0.016)
N	43,348	40,508	29,454	13,774

		Flu Shot in Past Year		
Laid off year 2	0.018	-0.032	-0.063	0.071
	(0.040)	(0.044)	(0.056)	(0.070)
Uninsured due to lay off year 2	-0.015	0.036	0.072	-0.060
	(0.056)	(0.059)	(0.070)	(0.088)
Unemployed (not laid off) year 2	0.039*	0.019	0.023	0.047
	(0.018)	(0.021)	(0.028)	(0.034)
Uninsured (not due to lay off) year 2	-0.029**	-0.026*	-0.026	-0.044**
	(0.010)	(0.011)	(0.014)	(0.015)
Private (year 1) to public health insurance (year 2)	0.080***	0.096***	0.077***	0.077***
	(0.011)	(0.011)	(0.015)	(0.019)
N	44,934	41,983	30,588	14,293

* p<0.05, ** p<0.01, *** p<0.001

(1) Baseline (2) Private insurance entire year 1 (3) Employer provided insurance entire year 1 (4) Not married

All regressions also control for age, age, sex, race/ethnicity, education, marital status, income, number of employees in firm, region, urban residence, self-reported health status, presence of physical limitations of work, smoking status, a series of risk preference measures, and presence of comorbidities both in oneself and in one's spouse.

Table 4-15 Regression estimates for cancer screening utilization by laid off and non-laid off unemployment including move from private to public insurance

	(1)	(2)	(3)	(4)
		Mammogram in Past Year		
Laid off year 2	0.021	0.031	0.209*	0.013
	(0.071)	(0.079)	(0.085)	(0.117)
Uninsured due to lay off year 2	-0.159	-0.158	-0.319*	-0.205
	(0.115)	(0.122)	(0.127)	(0.180)
Unemployed (not laid off) year 2	0.027	0.020	0.032	0.043
	(0.027)	(0.030)	(0.041)	(0.047)
Uninsured (not due to lay off) year 2	-0.059*	-0.068*	-0.052	-0.067
	(0.026)	(0.028)	(0.040)	(0.039)
Private (year 1) to public health insurance (year 2)	-0.007	-0.031	-0.012	0.027
	(0.017)	(0.018)	(0.024)	(0.026)
N	14,311	13,296	8,913	5,255
		Pap Test in Past Year		
Laid off year 2	-0.006	-0.018	0.109	0.076
	(0.057)	(0.063)	(0.071)	(0.079)
Uninsured due to lay off year 2	-0.051	-0.027	-0.168	-0.244
	(0.090)	(0.095)	(0.102)	(0.128)
Unemployed (not laid off) year 2	0.027	0.004	0.022	0.084*
	(0.024)	(0.027)	(0.040)	(0.042)
Uninsured (not due to lay off) year 2	-0.037*	-0.045*	-0.042	-0.006
	(0.018)	(0.020)	(0.028)	(0.025)
Private (year 1) to public health insurance (year 2)	-0.036*	-0.050**	-0.034	-0.028
	(0.016)	(0.016)	(0.022)	(0.024)
N	21,057	19,481	13,371	8,308
		PSA Test in Past Year		
Laid off year 2	-0.006	-0.002	-0.017	-0.126
	(0.079)	(0.090)	(0.127)	(0.188)
Uninsured due to lay off year 2	0.175	0.179	0.198	0.161
	(0.130)	(0.136)	(0.166)	(0.236)
Unemployed (not laid off) year 2	0.030	0.050	0.037	0.079
	(0.035)	(0.040)	(0.048)	(0.099)
Uninsured (not due to lay off) year 2	-0.044	-0.049	-0.053	-0.127
	(0.038)	(0.040)	(0.053)	(0.074)
Private (year 1) to public health insurance (year 2)	0.012	-0.005	-0.015	0.027
	(0.019)	(0.022)	(0.027)	(0.044)

N	7,794	7,063	5,023	1,523

* p<0.05, ** p<0.01, *** p<0.001
(1) Baseline (2) Private insurance entire year 1 (3) Employer provided insurance entire year 1 (4) Not married

All regressions also control for age, age, sex, race/ethnicity, education, marital status, income, number of employees in firm, region, urban residence, self-reported health status, presence of physical limitations of work, smoking status, a series of risk preference measures, and presence of comorbidities both in oneself and in one's spouse.

Table A-4-1 Summary statistics risk preferences by unemployment and uninsurance transition group

	Overall	Employed Insured	Unemployed Insured	Employed Uninsured	Unemployed Uninsured
Risk Preferences - High (Strongly believe do not need health insurance)	0.02	0.02	0.02	0.03	0.02
	(0.13)	(0.13)	(0.14)	(0.16)	(0.15)
Risk Preferences - Moderate (Somewhat believe do not need health insurance)	0.07	0.06	0.06	0.09	0.04
	(0.25)	(0.25)	(0.24)	(0.28)	(0.2)
Risk Preferences - Low (Uncertain or disagree somewhat that do not need health insurance)	0.18	0.18	0.16	0.24	0.21
	(0.39)	(0.39)	(0.36)	(0.43)	(0.41)
Risk Preferences - Minimal (Strongly disagree that do not need health insurance)	0.65	0.65	0.68	0.55	0.60
	(0.48)	(0.48)	(0.47)	(0.5)	(0.49)
Risk Preferences - Missing (Health insurance)	0.09	0.09	0.08	0.10	0.12
	(0.28)	(0.28)	(0.27)	(0.29)	(0.33)
Risk Preferences - High (Strongly believe health insurance not worth the cost)	0.07	0.07	0.06	0.11	0.07
	(0.25)	(0.25)	(0.24)	(0.31)	(0.26)
Risk Preferences - Moderate (Somewhat agree health insurance not worth the cost)	0.14	0.14	0.15	0.18	0.18
	(0.35)	(0.35)	(0.36)	(0.38)	(0.39)
Risk Preferences - Low (Uncertain or disagree that health insurance not worth the cost)	0.28	0.28	0.27	0.28	0.28
	(0.45)	(0.45)	(0.44)	(0.45)	(0.45)
Risk Preferences - Minimal (Strongly disagree that health insurance not worth the cost)	0.43	0.43	0.43	0.34	0.34
	(0.49)	(0.49)	(0.5)	(0.47)	(0.48)
Risk Preferences - Missing (Health insurance not worth cost)	0.09	0.09	0.09	0.10	0.13
	(0.28)	(0.28)	(0.29)	(0.3)	(0.33)
Risk Preferences - High (Strongly believe can overcome illness without medical help)	0.03	0.03	0.03	0.05	0.03
	(0.17)	(0.17)	(0.17)	(0.21)	(0.18)
Risk Preferences - Moderate (Somewhat agree can overcome illness without medical help)	0.18	0.18	0.14	0.19	0.16
	(0.39)	(0.39)	(0.35)	(0.4)	(0.36)
Risk Preferences - Low	0.31	0.31	0.30	0.32	0.31

	Overall	Employed Insured	Unemployed Insured	Employed Uninsured	Unemployed Uninsured
(Uncertain or disagree that can overcome illness without medical help)					
	(0.46)	(0.46)	(0.46)	(0.47)	(0.46)
Risk Preferences - Minimal (Strongly disagree that can overcome illness without medical help)	0.39	0.39	0.44	0.34	0.38
	(0.49)	(0.49)	(0.5)	(0.47)	(0.49)
Risk Preferences - Missing (Can overcome illness without medical help)	0.09	0.09	0.08	0.10	0.13
	(0.28)	(0.28)	(0.28)	(0.3)	(0.33)
Risk Preferences - High (Strongly agree more likely to take risks)	0.04	0.04	0.03	0.05	0.07
	(0.19)	(0.19)	(0.18)	(0.21)	(0.25)
Risk Preferences - Moderate (Somewhat agree more likely to take risks)	0.15	0.15	0.12	0.18	0.15
	(0.36)	(0.36)	(0.33)	(0.38)	(0.36)
Risk Preferences - Low (Uncertain or disagree more likely to take risks)	0.35	0.35	0.35	0.36	0.33
	(0.48)	(0.48)	(0.48)	(0.48)	(0.47)
Risk Preferences - Minimal (Strongly disagree more likely to take risks)	0.37	0.38	0.41	0.31	0.33
	(0.48)	(0.48)	(0.49)	(0.46)	(0.47)
Risk Preferences - Missing (More likely to take risks)	0.09	0.09	0.09	0.10	0.12
	(0.29)	(0.28)	(0.28)	(0.3)	(0.33)
N	45,573	43178	507	1,587	301

Means (SD)

* All values are from the baseline period (year 1)

Table A-4-2 Rates of infrequent preventive care utilization by unemployment and uninsurance transition group

	N	Employed Insured	Unemployed Insured	Employed Uninsured	Unemployed Uninsured
Physical 5+ years ago	44,293	0.12	0.12	0.08	0.18
		(0.33)	(0.32)	(0.27)	(0.39)
Blood pressure last checked 5+ years ago	44,442	0.03	0.03	0.01	0.05
		(0.17)	(0.17)	(0.11)	(0.22)
Cholesterol last Checked 5+ years ago	42,297	0.17	0.17	0.14	0.30
		(0.38)	(0.37)	(0.34)	(0.46)
Flu shot 5+ Years	44,507	0.55	0.55	0.49	0.67
		(0.50)	(0.50)	(0.50)	(0.47)
Mammogram 5+ Years	10,982	0.10	0.09	0.12	0.16
		(0.29)	(0.29)	(0.33)	(0.37)
Pap test 5+ years ago	21,861	0.08	0.07	0.08	0.08
		(0.26)	(0.26)	(0.27)	(0.27)
PSA test 5+ years ago	4,675	0.24	0.24	0.21	0.30
		(0.43)	(0.43)	(0.41)	(0.46)

Means (SD)

* All values are from the baseline period (year 1)

The sample sizes vary depending on both availability of responses as well as different age- and sex-based eligibility. In addition, to the usual age restrictions I limit the mammogram sample to women 45 years of age and old, and the PSA sample to men 55 years of age and older to allow 5 years from the original lower age bound.

Table A-4-3 Summary statistics restricted to 75th percentile of predicted unemployment or uninsurance

	Overall	Employed Insured	Unemployed Insured	Employed Uninsured	Unemployed Uninsured
Age	42.16	42.38	48.16	37.49	39.84
	(12.41)	(12.46)	(14.57)	(9.55)	(10.71)
Female	0.57	0.57	0.71	0.51	0.60
	(0.49)	(0.49)	(0.45)	(0.5)	(0.49)
White	0.73	0.73	0.76	0.74	0.65
	(0.45)	(0.45)	(0.43)	(0.44)	(0.48)
Black	0.20	0.20	0.16	0.21	0.27
	(0.4)	(0.4)	(0.37)	(0.41)	(0.45)
Other race	0.08	0.08	0.07	0.06	0.08
	(0.27)	(0.27)	(0.26)	(0.23)	(0.27)
Hispanic	0.25	0.25	0.21	0.30	0.31
	(0.43)	(0.43)	(0.41)	(0.46)	(0.46)
Education - No degree	0.19	0.19	0.27	0.23	0.22
	(0.39)	(0.39)	(0.44)	(0.42)	(0.42)
Education - High School	0.56	0.56	0.51	0.59	0.55
	(0.5)	(0.5)	(0.5)	(0.49)	(0.5)
Education - Bachelor's degree	0.15	0.15	0.11	0.10	0.12
	(0.35)	(0.36)	(0.32)	(0.3)	(0.32)
Education - Advanced degree	0.03	0.03	0.05	0.02	0.01
	(0.17)	(0.18)	(0.22)	(0.13)	(0.12)
Education - Other	0.06	0.06	0.05	0.05	0.07
	(0.24)	(0.24)	(0.21)	(0.21)	(0.26)
Education - Missing	0.01	0.01	0.01	0.01	0.02
	(0.09)	(0.09)	(0.1)	(0.09)	(0.15)
Marital status - Married	0.54	0.54	0.60	0.47	0.39
	(0.5)	(0.5)	(0.49)	(0.5)	(0.49)
Marital status - Widowed	0.04	0.04	0.08	0.02	0.03
	(0.2)	(0.2)	(0.27)	(0.14)	(0.16)
Marital status - Separated/Divorced	0.20	0.20	0.16	0.23	0.27
	(0.4)	(0.4)	(0.37)	(0.42)	(0.45)
Marital status - Never married	0.23	0.22	0.17	0.27	0.31
	(0.42)	(0.42)	(0.37)	(0.45)	(0.46)
Marital status - Missing	-	-	-	-	-
	-	-	-	-	-
Region - Northeast	0.16	0.17	0.20	0.14	0.16
	(0.37)	(0.37)	(0.4)	(0.35)	(0.37)
Region - Midwest	0.19	0.19	0.20	0.16	0.19
	(0.39)	(0.39)	(0.4)	(0.37)	(0.39)
Region - South	0.39	0.38	0.34	0.42	0.38
	(0.49)	(0.49)	(0.47)	(0.49)	(0.49)
Region - West	0.27	0.26	0.27	0.29	0.27
	(0.44)	(0.44)	(0.44)	(0.45)	(0.44)
Fair/poor self-reported health in any base year round	0.27	0.26	0.42	0.23	0.39
	(0.44)	(0.44)	(0.49)	(0.42)	(0.49)
Missing self-reported health	0.00	0.00	-	0.00	-
	(0.03)	(0.03)	-	(0.04)	-

	Overall	Employed Insured	Unemployed Insured	Employed Uninsured	Unemployed Uninsured
Any report of limitation of daily activities in base year round	0.07	0.07	0.19	0.04	0.13
	(0.26)	(0.25)	(0.39)	(0.2)	(0.33)
Missing reported limitation of daily activities	0.01	0.01	0.01	0.01	0.01
	(0.08)	(0.08)	(0.10)	(0.10)	(0.10)
Non-rural residence	0.86	0.86	0.85	0.86	0.89
	(0.35)	(0.35)	(0.36)	(0.35)	(0.31)
Family income <100% FPL	0.08	0.07	0.15	0.14	0.10
	(0.26)	(0.25)	(0.36)	(0.35)	(0.3)
Family income 100% - <125% FPL	0.04	0.04	0.05	0.06	0.05
	(0.21)	(0.2)	(0.21)	(0.25)	(0.22)
Family income 125% - <200% FPL	0.19	0.19	0.20	0.26	0.20
	(0.4)	(0.39)	(0.40)	(0.44)	(0.40)
Family income 200% - <400% FPL	0.42	0.42	0.28	0.39	0.46
	(0.49)	(0.49)	(0.45)	(0.49)	(0.50)
Family income >=400% FPL	0.27	0.28	0.32	0.15	0.20
	(0.44)	(0.45)	(0.47)	(0.36)	(0.4)
Hours worked	37.19	37.22	29.14	38.79	39.11
	(12.44)	(12.39)	(14.19)	(12.33)	(9.17)
Hours worked missing	0.01	0.01	0.02	0.01	0.01
	(0.09)	(0.09)	(0.12)	(0.09)	(0.10)
Hourly wage	11.85	11.99	10.03	10.04	12.85
	(8.53)	(8.61)	(8.1)	(7.35)	(7.28)
Number of employees	100.4	102.0	89.5	75.8	118.0
	(152.6)	(153.3)	(159.1)	(133.3)	(165.6)
Current smoker	0.25	0.25	0.23	0.26	0.37
	(0.43)	(0.43)	(0.42)	(0.44)	(0.48)
Smoking status missing	0.02	0.02	0.02	0.01	-
	(0.12)	(0.12)	(0.14)	(0.11)	-
Observations	17,339	15,763	322	1035	219

Means (SD)

* All values are from the baseline period (year 1)

Table A-4-4 Regression estimates for general preventive care utilization restricted to the 90th percentile of predicted unemployment or uninsurance

	(1)	(2)	(3)	(4)
		Physical in Past Year		
Unemployed year 2	0.011	0.028	0.127*	0.039
	(0.033)	(0.044)	(0.061)	(0.047)
Uninsured year 2	-0.023	-0.016	-0.002	-0.042
	(0.020)	(0.023)	(0.030)	(0.026)
Unemployed x Uninsured year 2	-0.026	-0.040	-0.163*	-0.089
	(0.056)	(0.066)	(0.082)	(0.074)
N	7,288	5,883	4,043	3,906
		Blood Pressure Checked in Past Year		
Unemployed year 2	0.036	0.045	0.049	0.053
	(0.021)	(0.026)	(0.043)	(0.031)
Uninsured year 2	-0.017	-0.015	-0.009	-0.038
	(0.018)	(0.021)	(0.028)	(0.023)
Unemployed x Uninsured year 2	-0.012	-0.018	-0.033	-0.003
	(0.041)	(0.047)	(0.063)	(0.053)
N	7,321	5,904	4,055	3,925
		Cholesterol Checked in Past Year		
Unemployed year 2	0.036	0.040	0.071	0.072
	(0.031)	(0.042)	(0.063)	(0.045)
Uninsured year 2	-0.034	-0.029	-0.029	-0.035
	(0.020)	(0.023)	(0.029)	(0.026)
Unemployed x Uninsured year 2	-0.034	-0.018	-0.073	-0.112
	(0.055)	(0.065)	(0.083)	(0.074)
N	7,023	5,673	3,879	3,769
		Flu Shot in Past Year		
Unemployed year 2	0.064*	0.007	0.012	0.071
	(0.032)	(0.042)	(0.063)	(0.048)
Uninsured year 2	-0.022	-0.016	-0.007	-0.027
	(0.016)	(0.018)	(0.023)	(0.021)
Unemployed x Uninsured year 2	-0.031	0.013	0.012	-0.018
	(0.050)	(0.059)	(0.079)	(0.068)
N	7,340	5,923	4,074	3,942

* p<0.05, ** p<0.01, *** p<0.001

(1) Baseline (2) Private insurance entire year 1 (3) Employer provided insurance entire year 1 (4) Not married

All regressions also control for age, age, sex, race/ethnicity, education, marital status, income, number of employees in firm, region, urban residence, self-reported health status, presence of physical limitations of work, smoking status, a series of risk preference measures, and presence of comorbidities both in oneself and in one's spouse.

Table A-4-5 Regression estimates for general cancer screening utilization restricted to the 90th percentile of predicted unemployment or uninsurance

	(1)	(2)	(3)	(4)
		Mammogram in Past Year		
Unemployed year 2	-0.022	-0.035	0.183*	0.032
	(0.055)	(0.069)	(0.083)	(0.080)
Uninsured year 2	-0.024	-0.004	-0.022	-0.128*
	(0.046)	(0.053)	(0.079)	(0.057)
Unemployed x Uninsured year 2	-0.206	-0.223	-0.454**	-0.072
	(0.107)	(0.119)	(0.143)	(0.143)
N	2,475	1,995	1,192	1,344
		Pap Test in Past Year		
Unemployed year 2	0.035	-0.048	0.132*	0.131*
	(0.040)	(0.051)	(0.066)	(0.058)
Uninsured year 2	-0.017	-0.015	-0.034	-0.016
	(0.029)	(0.034)	(0.047)	(0.034)
Unemployed x Uninsured year 2	-0.058	0.001	-0.192	-0.156
	(0.075)	(0.087)	(0.105)	(0.096)
N	4,341	3,415	2,203	2,585
		PSA Test in Past Year		
Unemployed year 2	0.072	0.238	-0.061	0.185
	(0.098)	(0.143)	(0.226)	(0.150)
Uninsured year 2	0.039	0.233	0.498**	0.259
	(0.122)	(0.166)	(0.190)	(0.274)
Unemployed x Uninsured year 2	-0.241	-0.606*	-0.589	-0.358
	(0.209)	(0.242)	(0.325)	(0.366)
N	486	363	239	186

* p<0.05, ** p<0.01, *** p<0.001

(1) Baseline (2) Private insurance entire year 1 (3) Employer provided insurance entire year 1 (4) Not married

All regressions also control for age, age, sex, race/ethnicity, education, marital status, income, number of employees in firm, region, urban residence, self-reported health status, presence of physical limitations of work, smoking status, a series of risk preference measures, and presence of comorbidities both in oneself and in one's spouse.

Table A-4-6 Regression estimates of predicted future unemployment or uninsurance for general preventive care utilization

	(1)	(3)
	Physical in Past Year	
Log (Predicted unemployed year 2)	-0.007	-0.010
	(0.006)	(0.008)
Log (Predicted uninsured year 2)	-0.020*	-0.018
	(0.009)	(0.011)
N	44,293	30,166
	Blood Pressure Checked in Past Year	
Log (Predicted unemployed year 2)	-0.010*	-0.015*
	(0.005)	(0.007)
Log (Predicted uninsured year 2)	-0.023**	-0.029**
	(0.007)	(0.009)
N	44,442	30,253
	Cholesterol Checked in Past Year	
Log (Predicted unemployed year 2)	-0.015*	-0.009
	(0.006)	(0.008)
Log (Predicted uninsured year 2)	-0.024**	-0.018
	(0.008)	(0.010)
N	42,927	29,198
	Flu Shot in Past Year	
Log (Predicted unemployed year 2)	-0.004	-0.026*
	(0.010)	(0.012)
Log (Predicted uninsured year 2)	-0.080***	-0.057**
	(0.016)	(0.017)
N	44,507	30,326

* p<0.05, ** p<0.01, *** p<0.001
(1) Baseline (3) Employer provided insurance entire year 1

All regressions also control for age, age, sex, race/ethnicity, education, marital status, income, number of employees in firm, region, urban residence, self-reported health status, presence of physical limitations of work, smoking status, a series of risk preference measures, and presence of comorbidities both in oneself and in one's spouse.

Table A-4-7 Regression estimates of predicted future unemployment or uninsurance for cancer screening utilization

	(1)	(3)
	Mammogram in Past Year	
Log (Predicted unemployed year 2)	0.017	0.022
	(0.011)	(0.014)
Log (Predicted uninsured year 2)	-0.070***	-0.058**
	(0.017)	(0.020)
N	14,177	8,792
	Pap Test in Past Year	
Log (Predicted unemployed year 2)	-0.006	-0.001
	(0.009)	(0.013)
Log (Predicted uninsured year 2)	-0.049**	-0.045*
	(0.015)	(0.019)
N	20,951	13,306
	PSA Test in Past Year	
Log (Predicted unemployed year 2)	-0.001	-0.019
	(0.011)	(0.016)
Log (Predicted uninsured year 2)	-0.001	0.008
	(0.015)	(0.020)
N	7,594	4,909

* p<0.05, ** p<0.01, *** p<0.001
(1) Baseline (3) Employer provided insurance entire year 1

All regressions also control for age, age, sex, race/ethnicity, education, marital status, income, number of employees in firm, region, urban residence, self-reported health status, presence of physical limitations of work, smoking status, a series of risk preference measures, and presence of comorbidities both in oneself and in one's spouse.

Table A-4-8 Regression estimates of predicted 75th percentile of future unemployment or uninsurance for general preventive care utilization

	(1)	(3)
	Physical in Past Year	
≥75th Percentile of predicted unemployment	-0.037***	-0.036**
	(0.010)	(0.013)
≥75th Percentile of predicted uninsurance	-0.016	-0.017
	(0.011)	(0.013)
≥75th Percentile of predicted unemployment and uninsurance	0.065***	0.067***
	(0.015)	(0.018)
N	44,293	30,166
	Blood Pressure Checked in Past Year	
≥75th Percentile of predicted unemployment	-0.023**	-0.030**
	(0.007)	(0.010)
≥75th Percentile of predicted uninsurance	-0.041***	-0.042***
	(0.009)	(0.011)
≥75th Percentile of predicted unemployment and uninsurance	0.054***	0.058***
	(0.012)	(0.015)
N	44,442	30,253
	Cholesterol Checked in Past Year	
≥75th Percentile of predicted unemployment	-0.038***	-0.032*
	(0.010)	(0.013)
≥75th Percentile of predicted uninsurance	-0.034**	-0.030*
	(0.010)	(0.013)
≥75th Percentile of predicted unemployment and uninsurance	0.060***	0.060**
	(0.014)	(0.018)
N	42,927	29,198
	Flu Shot in Past Year	
≥75th Percentile of predicted unemployment	-0.005	-0.014
	(0.011)	(0.014)
≥75th Percentile of predicted uninsurance	0.010	0.009
	(0.010)	(0.012)
≥75th Percentile of predicted unemployment and uninsurance	<0.001	0.004
	(0.014)	(0.017)
N	44,507	30,326

* p<0.05, ** p<0.01, *** p<0.001

(1) Baseline (3) Employer provided insurance entire year 1

All regressions also control for age, age, sex, race/ethnicity, education, marital status, income, number of employees in firm, region, urban residence, self-reported health status, presence of physical limitations of work, smoking status, a series of risk preference measures, and presence of comorbidities both in oneself and in one's spouse.

Table A-4-9 Regression estimates of predicted 75th percentile of future unemployment or uninsurance for cancer screening utilization

	(1)	(3)
	Mammogram in Past Year	
≥75th Percentile of predicted unemployment	-0.023	-0.035
	(0.015)	(0.020)
≥75th Percentile of predicted uninsurance	-0.029	-0.027
	(0.031)	(0.037)
≥75th Percentile of predicted unemployment and uninsurance	0.068*	0.071
	(0.034)	(0.043)
N	14,177	8,792
	Pap Test in Past Year	
≥75th Percentile of predicted unemployment	-0.024	-0.037*
	(0.013)	(0.018)
≥75th Percentile of predicted uninsurance	0.007	0.016
	(0.018)	(0.021)
≥75th Percentile of predicted unemployment and uninsurance	0.038	0.038
	(0.020)	(0.026)
N	20,951	13,306
	PSA Test in Past Year	
≥75th Percentile of predicted unemployment	0.040	0.075*
	(0.026)	(0.036)
≥75th Percentile of predicted uninsurance	0.023	0.012
	(0.040)	(0.050)
≥75th Percentile of predicted unemployment and uninsurance	-0.033	-0.061
	(0.056)	(0.072)
N	7,594	4,909

* p<0.05, ** p<0.01, *** p<0.001
(1) Baseline (3) Employer provided insurance entire year 1

All regressions also control for age, age, sex, race/ethnicity, education, marital status, income, number of employees in firm, region, urban residence, self-reported health status, presence of physical limitations of work, smoking status, a series of risk preference measures, and presence of comorbidities both in oneself and in one's spouse.

Table A-4-10 Regression estimates restricted to 75th percentile or greater of predicted unemployment or uninsurance for individuals with employer provided health insurance in year 1 for years 2007-2010

	Physical in Past Year	Blood Pressure Checked in Past Year	Cholesterol Checked in Past Year	Flu Shot in Past Year	Mammogram in Past Year	Pap Test in Past Year	PSA Test in Past Year
Unemployed year 2	0.111	0.052	0.062	-0.035	0.040	0.081	0.11
	(0.076)	(0.047)	(0.079)	(0.084)	(0.116)	(0.114)	(0.305)
Uninsured year 2	-0.051	<0.001	-0.017	-0.029	-0.124	-0.151*	0.176
	(0.039)	(0.035)	(0.040)	(0.033)	(0.094)	(0.069)	(0.181)
Unemployed x Uninsured year 2	-0.064	-0.008	-0.032	0.046	-0.072	0.092	-0.351
	(0.101)	(0.072)	(0.105)	(0.102)	(0.204)	(0.155)	(0.392)
N	3,361	3,396	3,245	3,385	924	1,593	341

* p<0.05, ** p<0.01, *** p<0.001

All regressions also control for age, age, sex, race/ethnicity, education, marital status, income, number of employees in firm, region, urban residence, self-reported health status, presence of physical limitations of work, smoking status, a series of risk preference measures, and presence of comorbidities both in oneself and in one's spouse.

Table A-4-11 Regression estimates by laid off and non-laid off unemployment for individuals with employer provided health insurance in year 1 in years 2007-2010

	Physical in Past Year	Blood Pressure Checked in Past Year	Cholesterol Checked in Past Year	Flu Shot in Past Year	Mammogram in Past Year	Pap Test in Past Year	PSA Test in Past Year
Laid off year 2	0.165*	0.117**	0.051	-0.063	0.205	0.198	0.095
	(0.077)	(0.041)	(0.079)	(0.088)	(0.126)	(0.113)	(0.190)
Uninsured due to lay off year 2	-0.108	-0.019	-0.003	0.10	-0.236	-0.187	-0.03
	(0.097)	(0.056)	(0.098)	(0.105)	(0.174)	(0.144)	(0.230)
Unemployed (not laid off) year 2	0.133	0.047	0.001	-0.010	0.055	0.136	0.089
	(0.069)	(0.049)	(0.088)	(0.085)	(0.120)	(0.110)	(0.303)
Uninsured (not due to lay off) year 2	-0.064*	-0.037	-0.034	-0.050	-0.114	-0.125*	0.106
	(0.033)	(0.029)	(0.033)	(0.029)	(0.082)	(0.058)	(0.119)
N	9,596	9,660	9,284	9,628	2,881	4,284	1,642

* p<0.05, ** p<0.01, *** p<0.001

All regressions also control for age, age, sex, race/ethnicity, education, marital status, income, number of employees in firm, region, urban residence, self-reported health status, presence of physical limitations of work, smoking status, a series of risk preference measures, and presence of comorbidities both in oneself and in one's spouse.

Chapter 5.
Conclusion

This dissertation examines how various factors affect preventive health utilization decisions, including factors not directly related to the specific type of preventive health care itself. These include complex interactions between different types of care and macroeconomic variations in the likelihood of future unemployment or uninsurance.

In the first paper, I analyze how a false-positive mammogram can have significant spillover effects onto medication adherence, finding that a false-positive mammogram leads to improved cholesterol medication adherence for the Medicaid insured population but reduced adherence for the commercially insured. I further provide some suggestive evidence that the improvement in the Medicaid population may be due to increased interaction with the health care system. This suggests policies to encourage providers to check on all aspects of a patient care may be important in improving medication adherence. In addition, policies to provide low cost providers to ensure that populations that may have access problems are able to interact with the health care system may also help improve medication adherence.

On the other hand, the worsening of cholesterol medication adherence in the commercially insured population demonstrates that the negative experience of a false-positive mammogram can have wide ranging negative effects. Particularly in combination with the results from the second paper, which shows significant increases in depression and anxiety medication initiation following a false-positive mammogram, health policies providing additional follow-up care for

women experiencing a false-positive may be important both to help women deal with increased anxiety as well as ensure that they continue to utilize appropriate preventive care. The negative effects also further indicate that there are serious adverse effects of false-positive mammograms that need to be considered when determining recommended rates of mammography screening.

Finally, in the third paper I show that anticipated unemployment and uninsurance may have significant, but opposing effects on preventive care utilization. I find evidence of stocking up for future unemployment, but even stronger evidence for delaying as a result of future uninsurance. The results suggest that efforts to ensure appropriate preventive care utilization during economic downturns may be best targeted on the insurance side. In particular, policies to help individuals find and obtain affordable coverage, whether it's marketplace coverage, Medicaid, or some other type of additional coverage, for both preventive and curable care are important.