UNIVERSITY OF CALIFORNIA,
IRVINE

The Influence of Inefficiencies in Health Care and the Labor Market

DISSERTATION

submitted in partial satisfaction of the requirements
for the degree of

DOCTOR OF PHILOSOPHY

in Economics

by

Scott Barkowski

Dissertation Committee:
Professor David Neumark, Chair
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DEDICATION

To my parents, my brother and sister, my in-laws, and, most of all, my wife and my daughters. Their love and support helped make this work possible.

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WORKING PAPERS

* "Does Government Health Insurance Reduce Job Lock and Job Push?"

Summary: I estimate the effects of employment contingent health insurance on job mobility, known as job lock and job push, using a difference-in-differences model where imputed Medicaid eligibility measures variation in the demand for health insurance. I find large estimates

of job lock and job push for men, but not for women.

* "Does Regulation of Physicians Reduce Health Care Spending?"

Summary: Physicians accused of malpractice face punishment from regulators like medical boards in addition to civil litigation. I study the effect of such punishments on health care spending. Increased risk of punishment appears to decrease state level spending slightly, while civil litigation risk appears to have no effect.

* "The Effect of Specialist Cost Information on Primary Care Physician Referral Patterns: A Field Experiment" (Approved by UCI Human Research Protections staff)

Summary: Prices are rarely known by decision makers in health care transactions. In partnership with private medical group, I randomly distribute information on costliness of specialist physicians to primary care physicians (PCPs) and observe whether the information leads to different referral choices by PCPs.

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ABSTRACT OF THE DISSERTATION

The Influence of Inefficiencies in Health Care and the Labor Market

By

Scott Barkowski

Doctor of Philosophy in Economics

University of California, Irvine, 2014

Professor David Neumark, Chair

In the first essay, "Does Government Health Insurance Reduce Job Lock and Job Push?", I estimate the extent that job mobility is affected by the link between health insurance and employment. Workers holding employment-contingent health insurance (ECHI) are often thought to stay in jobs that are otherwise inferior matches out of fear of losing their ECHI, while those without insurance may leave employment states that are otherwise good matches seeking access to ECHI. These two phenomena are known as job lock and job push, respectively. During the late 1980s and early 1990s, Medicaid expansions resulted in many working class households gaining Medicaid eligibility for one or more family members, an alternative source of health insurance that is not contingent on employment. Using this eligibility as a measure of variation in the dependence on ECHI for health insurance coverage, I find large estimates of job lock and job push for men. Medicaid eligibility for one household member results in an increase in the likelihood of a voluntary job exit for men over a four-month period by approximately 34%. Similarly, moves into jobs with ECHI fall by approximately 25% in response to Medicaid eligibility. For women, I do not find evidence consistent with job lock. For the case of job push, some of my estimates suggest large effects, though these estimates have interpretive difficulties.

The second essay, titled "Does Regulation of Physicians Reduce Health Care Spending?",

examines the fear among physicians that legal liability increases health care spending. Theoretically, the effect of legal risk could be positive or negative on spending, and empirical evidence has supported both cases. Previous empirical work, however, has ignored that physicians face risk from centralized regulators – industry oversight groups like medical boards – in addition to civil litigation risk. This paper addresses this omission by incorporating previously unused data on punishments by oversight groups against physicians, known as adverse actions, along with malpractice payments data to study state-level health care spending. My analysis suggests that health care spending does not rise in response to higher levels of risk. An increase in adverse actions equal to 16, the mean year-to-year change within a state, is found to be associated with statistically significant average spending decreases of approximately 0.11% to 0.21%. Malpractice payments were generally estimated to have smaller, statistically insignificant effects.

The final essay, "Preliminary Results On The Effect of Specialist Cost Information on Primary Care Physician Referral Patterns", reports early results on a field experiment designed to test whether primary care physicians (PCPs) would use information on specialist costs in allocating their patient referrals between doctors within the specialty. The experiment was performed in partnership with a private-sector group of medical practices organized as an Independent Practice Association (IPA). Randomly chosen PCP practices within the IPA were provided with a report listing average cost information for Ophthalmology practices within the IPA. The response of the PCPs is compared to a control group of PCP practices within the IPA to see if the information influenced which Ophthalmology practices received PCP referrals. Analysis of experimental data so far available does not find any effects that are statistically significant at conventional levels. These results, however, are based on data from a very short post-period, and are not considered final. The experiment is ongoing at the time of the writing of this essay.

Chapter 1

Does Government Health Insurance Reduce Job Lock and Job Push?

1.1 Introduction

The American health insurance market shares a close association with the country's labor market, a link made evident by the fact that in 2010, 78% of full-time, year-round workers bought health insurance through an employer (Brault and Blumenthal, 2011, Table 1). The main reason for this link is that employment-contingent health insurance (ECHI) is generally provided at much lower cost than insurance coverage available through alternative sources like the individual market. This lower cost is a result of factors such as tax incentives for both the employer and the employee, reduced administrative costs for groups, and reduced incidence of adverse selection (Gruber, 2000). The benefits of employer provision of insurance come at a cost, though, as access to this lower cost insurance is usually limited to firm employees and their immediate families.

Such restrictions on access could have important implications on the frequency that workers

enter and exit jobs. For workers with ECHI, non-portability of coverage implies costs that must be incurred by the worker when leaving an employer. These costs could be relatively small, such as losing access to certain physicians under a new insurance plan, or large, as when a health condition delays or prevents the worker from obtaining coverage. To avoid these costs, an employee may choose to stay in a job he or she would otherwise leave, a phenomenon referred to as "job lock". On the other hand, when a person does not have ECHI, whether in a job or not, the higher cost of obtaining insurance through other sources will provide an incentive to find a job that provides access to ECHI. When such a person chooses to take a job that he or she would otherwise not in order to gain access to health insurance, it is called "job push" (Cooper and Monheit, 1993; Anderson, 1997; Hamersma and Kim, 2009). In either case, job lock or job push, the underlying mechanism is the same: the relationship between employment and insurance increases the value of jobs offering access to health insurance relative to other employment states.

Understanding the role ECHI plays in the operation of the labor market has taken a renewed importance in recent years with the debate over, and eventual passage of, the Affordable Care Act in 2010. Before the law was adopted in 2009, supporters of the law argued that the reduction of job lock was one mechanism through which the law would improve the economy (e.g., Council of Economic Advisors, 2009; Gruber, 2009). However, there has been little consensus in the literature regarding the prevalence of job mobility effects of ECHI. Empirical studies have estimated effect sizes ranging from large (e.g., Cooper and Monheit, 1993; Madrian, 1994; Gruber and Madrian, 1994; Buchmueller and Valletta, 1996), to moderate but only affecting certain sub-populations (e.g., Gilleskie and Lutz, 2002; Hamersma and Kim, 2009), to small or non-existent (Holtz-Eakin, 1994; Kapur, 1997; Berger et al., 2004). Moreover, Sanz-De-Galdeano (2006) found that a previous law intended to alleviate job lock, the Health Insurance Portability and Accountability Act of 1996, had no effect.

The goal of this paper is to contribute to this debate regarding the existence of job lock

and job push in the American labor market. Using individual-level panel data from the Survey of Income and Program Participation (SIPP), I estimate job mobility effects that are identified via variation in eligibility for Medicaid, the state and federal government provided health insurance program. Medicaid provides a source of portable insurance coverage, so its availability represents a reduction in the cost of leaving an employer for those who hold ECHI. We would, therefore, expect workers to be more likely to leave their jobs when they themselves, or their family members, are eligible for Medicaid coverage. Conversely, for individuals without ECHI, Medicaid eligibility makes their current state of employment more attractive relative to job options that offer ECHI, so one would expect these people to be less likely to move into such jobs. Medicaid eligibility, rather than outright Medicaid enrollment, is used since eligible individuals always have the option to enroll in the program after becoming sick without exclusions. Thus, eligibility is the same as actual enrollment, as far as behavior that is affected by insurance coverage is concerned (Cutler and Gruber, 1996; Gruber and Yelowitz, 1999).

To compensate for the possibility that Medicaid eligibility is correlated with unobserved factors that influence the propensity to move between jobs, but that are unrelated to worker concerns over health insurance, I rely on a difference-in-differences (DD) regression framework for estimation. In this strategy, individuals who have insurance through sources that are not their employers, such as through spouse employers, are used in control groups. For these individuals, Medicaid eligibility represents a redundant source of health insurance, rendering it a placebo. This strategy is in the spirit of Madrian (1994), who first used a DD approach to estimate job lock. Her most similar analysis relied on secondary sources of insurance to generate variability in dependence on employment for health insurance access. In her case, individuals without ECHI were used in control groups to control for factors correlated with alternative sources of health insurance. Madrian's analysis suggested job lock effects were large, as she estimated that ECHI caused a reduction in job exit probability for married men ranging from 25% to 70%.

Madrian's work was sharply criticized by Kapur (1997), who argued that individuals without ECHI were not appropriate for use in a control group as they were too dissimilar to those with ECHI. Kapur's work used only individuals with ECHI for the control group, and based identification on variation in an index of survey reported health issues. Using the same data as Madrian, Kapur found no job lock effects with this approach.[1] My analysis addresses the critique levied by Kapur, since my primary control group is composed of individuals with the same ECHI status as those in the treatment group. My use of Medicaid eligibility as the identification method, though, improves on her work for two reasons. The first is that survey reported health conditions may not actually generate significant variation in dependence on employment for health insurance. The sorts of health conditions reported to surveys would tend to be chronic and easily observable, which may be important predictors for some types of future health care spending, but they would not necessarily predict individual valuation of coverage for unexpected and catastrophic health issues. Thus, individuals *not* reporting health conditions to surveys may have just as strong demand for ECHI as individuals who do. Medicaid, however, is actual insurance coverage and does cover unexpected and catastrophic expenses, in addition to chronic issues, making it a more plausible source of variation in the dependence on employment for health insurance.

The second reason my approach improves on Kapur's methods (and the previous job lock literature more generally) is that the primary identifying variation in my analysis comes from time variation in eligibility for Medicaid. During the time period I study, the late 1980s and early 1990s, the government significantly expanded Medicaid eligibility for children in an effort to reduce the number of kids without health insurance. This generated significant within-person time-variation in Medicaid coverage that allows me to identify and include individual-level fixed effects in my models (a first in the job lock literature), and thereby account for individual heterogeneity in job exit propensities that is not already accounted for by the DD specification.

[1] Berger et al. (2004) used a similar approach and also found no job lock effect.

Using this empirical framework, I find consistent evidence of both job lock and job push for men that, when considered in proportion to population rates of mobility, are substantial in size. For job lock, I estimate that Medicaid eligibility for one household member of a male worker increases his probability of leaving his job voluntarily over a four-month period by approximately 34% on average, as compared to his likelihood if the household member were dependent on the worker's ECHI for coverage. For job push, I estimate that eligibility versus no coverage at all for a household member results in a 25% lower likelihood of transitioning into a job with ECHI over four months. Thus, for men, I produce estimates that are generally of the same magnitude as Madrian's, despite that I address the critique made by Kapur and improve on her method of identification. For women, I do not find evidence of a job lock effect and only weak evidence (at best) for job push. Those estimates of job push for women that do indicate an effect suffer from problems such as unrealisticly large effect sizes, sign changes under robustness checks, and inconsistency with estimates for other household members.

1.2 Medicaid during the late 1980s and early 1990s

Medicaid is a joint federal and state program that provides health care insurance coverage for poor children and their parents, poor pregnant women, and poor elderly and disabled.[2] Each state administers its own Medicaid program and sets its own eligibility rules that are subject to federal requirements. It covers a broad range of medical services with little or no cost to the beneficiary, making its coverage valuable to the covered individual.

Before 1984, Medicaid was only available to non-elderly, non-disabled individuals if they participated in the federal cash welfare program, Aid to Families with Dependent Children

[2]The discussion in this section relies heavily on a number of sources, each of which contributed important facts, concepts, and background information throughout. To avoid distracting the reader with numerous citations, I acknowledge these works here except to note a source when specific statistics are mentioned: Committee on Ways and Means, U.S. House of Representatives (1987); Congressional Research Service (1988, 1993); Currie and Gruber (1996a,b); Cutler and Gruber (1996); Gruber (2003); Gruber and Yelowitz (1999); Yelowitz (1995).

(AFDC).[3] During this time period, AFDC provided income to very poor families with a primary goal of supporting children lacking parental support. Eligibility rules for AFDC were quite restrictive, though, admitting only some family composition types in addition to requiring low incomes. For example, childless adults and, in many states, two-parent families with children were ineligible. As a result, eligible families were nearly entirely headed by single women who did not work. In 1984, 90% of AFDC families were headed by single mothers, fewer than 5% of participating mothers worked, and 88% of participating families had no income at all outside of AFDC (Committee on Ways and Means, U.S. House of Representatives, 1987, p. 432).

Medicaid's link to AFDC combined with the restrictiveness of AFDC eligibility resulted in a large number of low-income families being excluded from Medicaid. This feature of the Medicaid program drew attention from Congress, which feared that children were going without health care since their families could not afford their own insurance coverage. Beginning in 1984 and continuing into the 1990's, Congress addressed the issue by passing a series of federal level laws that gradually reduced the link between AFDC and Medicaid, resulting in the expansion of Medicaid eligibility to historically excluded segments of the population. In particular, families with much higher income levels than before became eligible, including many headed by adults in the working ranks.

Given the government's goal of ensuring insurance coverage for children, the expansions were designed to either directly make kids Medicaid eligible, or to indirectly cover them by providing coverage of pregnancy care for women (that is, only pre-natal care and birth related expenses were covered for women, not general health care when not pregnant). Men, therefore, were generally not eligible for Medicaid themselves.[4] However, men did still receive

[3]The elderly and disabled were eligible for Medicaid if they were eligible for the federal cash welfare program, Supplemental Security Income.

[4]Men could be directly covered via eligibility for AFDC, which primarily covered women but did cover some men such as the case of a poor, single father. Eligibility of AFDC did expand during this period, but its magnitude was very small compared to the expansion of eligibility for women beyond AFDC (Cutler and Gruber, 1996). Men could also be eligible for Medicaid as a recipient of Supplemental Security Income (SSI),

indirect benefits from the expansion since their children or spouses could be covered (or any female adult family members, whether spouses or not).

The implementation of the expansion took place via a number of different eligibility rule changes. Eligibility age limits for children were increased, family income thresholds were raised, and family composition restrictions were eased. These changes were effected in a varied fashion, with federal law at times imposing nationwide requirements, and at others giving states the option to expand eligibility requirements. These optional expansions were then, in some cases, later made requirements. As a result, Medicaid eligibility expanded over time and did so at different rates across states. This variation across states is illustrated in Figures 1.1 and 1.2, which show the expansion of Medicaid in the five largest states and my home state of South Carolina for children under 15 and women 21 and older, respectively.[5]

1.3 Identifying job mobility effects

Before proceeding with the discussion of identifying job mobility, it would be useful to note that I estimate models which explicitly account for insurance status of multiple members of a given household, even though the job mobility effects estimated by a given model only refer to one member of the household. To avoid ambiguity, I refer the the individual whose mobility is being modeled (that is, the individual to whom the outcome variables refer) as "the worker", "the analysis sample member", or simply "the sample member". I use "household members" to refer to individuals living with the sample member at the time of his or her inclusion in my analysis samples.[6]

a federal welfare program for poor elderly, blind, and disabled. However, those individuals are excluded from my analysis.

[5]Estimates of eligibility are weighted using person-level, cross-sectional weights for the month of reference.

[6]Since I estimate models for both men and women separately, there are some cases where two members of the same household might be a sample member in one model but a household member in another. For example, in the case of a married couple, the husband is an analysis sample member in the regressions on my sample of men, with his wife a household member in those regressions. In the regressions on my sample

1.3.1 Job lock

The dependent variable of interest in my job lock analysis, Y_{it}, equals one if worker i reported a voluntary exit, either a quit or a retirement, from his or her job during period t, and zero otherwise. The goal of the analysis is to estimate λ, the effect of the link between health insurance and employment on Y_{it}:

$$\begin{aligned}\lambda \equiv & E[Y_{it}|i \text{ } \underline{\text{not}} \text{ dependent on employment for health insurance during } t] \\ & - E[Y_{it}|i \text{ dependent on employment for health insurance during } t]. \end{aligned} \tag{1.1}$$

A positive value for λ indicates that voluntary job exits are more frequent when workers are not dependent on their jobs for health insurance. That is, a positive estimate of λ would be evidence consistent with job lock.

To estimate λ, I start by restricting my SIPP sample to include only workers from households in which all members are covered by an employer-provided health insurance plan. This step is necessary to be able to differentiate job lock from job push; workers in households where everyone is covered by insurance from an employer have no incentive to seek alternative employment in order to gain employer-provided coverage (which would be job push, not job lock). Without this restriction, the sign of the theoretical effect of ECHI is ambiguous. Consider the example of a single worker with one child, whose job provides coverage for the worker only. I might assume that the worker is dependent on her job for insurance and hence less likely to leave her job. However, this may not be the case – the worker may seek another job that provides coverage for both her and her child, and so is more likely to leave her job. So by conditioning on ECHI coverage for the entire household, I omit individuals who are more likely to be influenced by job push rather than job lock.

Having so restricted the data, I rely on Medicaid *eligibility* to provide variation in worker

of women, though, the wife is the analysis sample member while the husband is a household member.

ECHI dependence. Medicaid is a source of health insurance that is not contingent on employment and, due to the expansions of the program during the time period studied, common among the working population. Eligibility, rather than explicit enrollment, is used because it implies coverage due to the fact that eligible individuals always have the option to enroll even after becoming sick (or pregnant, in the case of women). Cutler and Gruber (1996, p. 392) called this aspect of Medicaid eligibility "conditional coverage". I therefore base identification on the comparison of the Medicaid eligible and ineligible. While Medicaid was not a direct replacement for most employer provided plans, for the eligible its availability did reduce the potential costs associated with choosing to leave a job, since they would not be completely without health insurance. For those who were not eligible, leaving a job still implied that access to health insurance would be limited. Thus, if job mobility is truly affected by ECHI, we would expect to observe different mobility rates between those eligible and not eligible for Medicaid.

To account for the possibility that individuals with and without Medicaid eligibility differ in important and unobserved ways, I implement an expanded version of the difference-in-differences (DD) framework applied by previous authors (e.g., Madrian, 1994; Holtz-Eakin, 1994; Buchmueller and Valletta, 1996). My model of voluntary job terminations takes the following form:

$$\begin{aligned} Y_{i(t+1)} = {} & \beta_1 E_{it} + \beta_2 M_{it} + \beta_3 E_{it} N_{it} + \beta_4 E_{it} M_{it} + \beta_5 E_{it} N_{it} M_{it} \\ & + \beta_6 \left(\sum_{j=1}^{J_{it}} E_{itj} \right) + \beta_7 \left(\sum_{j=1}^{J_{it}} M_{itj} \right) + \beta_8 \left(\sum_{j=1}^{J_{it}} E_{itj} N_{itj} \right) + \beta_9 \left(\sum_{j=1}^{J_{it}} E_{itj} M_{itj} \right) \\ & + \beta_{10} \left(\sum_{j=1}^{J_{it}} E_{itj} N_{itj} M_{itj} \right) + X_{it}' \gamma + u_{it}. \end{aligned} \tag{1.2}$$

In equation 1.2, E_{it} equal to one indicates that individual i holds ECHI coverage through his or her own employer during period t, with zero indicating otherwise. N_{it} indicates whether the worker is covered by insurance through a source that is not the worker's own employer

and not Medicaid. That is, i holds "non-ECHI", an example of which would be insurance obtained through another household member's employer. M_{it} identifies individuals who are Medicaid eligible (including those explicitly enrolled in Medicaid). Both N_{it} and M_{it} take values analogously to E_{it}. Regressors with j subscripts indicate insurance statuses for household members of the worker. More explicitly, the ECHI, non-ECHI, and Medicaid statuses of each of J_{it} members of individual i's household (not including i) during time t are indicated respectively by E_{itj}, N_{itj}, and M_{itj}, with j identifying the household member. Coverage in each case is indicated by one and a lack thereof by zero. Finally, X_{it} is a vector of other controls and u_{it} is the unobserved error term.

The household member version of the Medicaid variable is the perfect analog to the analysis sample member version. The ECHI and non-ECHI variables, however, have a subtle, yet important difference from their sample member versions. E_{itj} equals one if the household member has insurance coverage from sample member i's employer; it is not based on the household member's employer. If household member j has coverage from any other source (besides Medicaid), including his or her own employer, then N_{itj} equals one. For example, consider a household consisting of a husband and wife. Suppose both work and have insurance coverage through their own employers that cover themselves only, not their spouses. Moreover, neither spouse has any other source of health insurance. For my regressions on my sample of males, the husband i is the sample member and his wife, his only household member, has index $j = 1$.[7] Since he only has insurance through his employer, his status variables are then $E_{it} = 1$, $N_{it} = 0$, and $M_{it} = 0$. His wife's coverage is indicated as $E_{it1} = 0$, $N_{it1} = 1$, and $M_{it1} = 0$, since her only insurance is not through *his* employer, even though it is through hers. Other combinations are possible, but all values are assigned in this fashion.

To better explain the rationale behind equation 1.2, I first consider a simpler version of the

[7]Note that these roles would be reversed for regressions on my sample of females.

model which has no terms for household members, only the worker him- or herself:

$$Y_{i(t+1)} = \beta_1 E_{it} + \beta_2 M_{it} + \beta_3 E_{it} N_{it} + \beta_4 E_{it} M_{it} + \beta_5 E_{it} N_{it} M_{it} + X'_{it}\gamma + u_{it}. \tag{1.3}$$

This model could be interpreted as a standard triple-differences specification in which N_{it} and $N_{it}M_{it}$ are excluded due to the restricted sample,[8] but I argue that it is more appropriate to think of it as a dual difference-in-differences specification, a point which is illustrated in Table 1.1. The "treatment" group is composed of individuals who depended on their employers for insurance access. That is, those with ECHI but without non-ECHI. For comparison, this specification offers two potential "control" groups. The first contains individuals with both ECHI and non-ECHI, while the second contains those without ECHI but with non-ECHI. In this framework, Medicaid eligibility represents a treatment for individuals in the treatment group since it was their only insurance besides ECHI. For both of the control groups, however, the members already had insurance not related to their own employment, rendering Medicaid eligibility redundant (and hence, a placebo) as far as job mobility is concerned.

As Table 1.1 shows, the availability of two control groups allows for the identification of the job lock parameter, λ, via two separate DD rationales, given by

$$\begin{aligned}\lambda_1 \equiv\ & E[Y_{i(t+1)}|E_{it}=1, N_{it}=0, M_{it}=1] - E[Y_{i(t+1)}|E_{it}=1, N_{it}=0, M_{it}=0] \\ & - \big(E[Y_{i(t+1)}|E_{it}=1, N_{it}=1, M_{it}=1] - E[Y_{i(t+1)}|E_{it}=1, N_{it}=1, M_{it}=0]\big) = -\beta_5,\end{aligned} \tag{1.4}$$

[8] In a standard triple-differences regression, three indicator variables divide the sample into eight sub-groups. In my sample, though, there are only six sub-groups because of the restriction that the workers all lived in households where every member had insurance through an employer. This means that there are no individuals for whom E_{it} and N_{it} both equal zero, and hence, that $E_{it} = 0$ implies $N_{it} = 1$ and $N_{it} = 0$ implies $E_{it} = 1$. As a result, variables E_{it}, N_{it}, and $E_{it}N_{it}$ are collinear, as $E_{it} + N_{it} - E_{it}N_{it} = 1$. I therefore drop N_{it} and $N_{it}M_{it}$, since keeping E_{it} and $E_{it}M_{it}$ makes interpretation of the results more straightforward.

and

$$\lambda_2 \equiv E[Y_{i(t+1)}|E_{it}=1, N_{it}=0, M_{it}=1] - E[Y_{i(t+1)}|E_{it}=1, N_{it}=0, M_{it}=0]$$
$$- \big(E[Y_{i(t+1)}|E_{it}=0, N_{it}=1, M_{it}=1] - E[Y_{i(t+1)}|E_{it}=0, N_{it}=1, M_{it}=0]\big) = \beta_4. \tag{1.5}$$

Here the first lines of both equations are the same and represent the difference between the averages for the Medicaid eligible and ineligible individuals in the treatment group. The second lines both represent the same difference but for the two separate control groups, where the first equation uses the "preferred control" group (those with ECHI) and the second equation uses the "alternative control" group (those without ECHI). Job lock can be identified simultaneously via either method, λ_1 or λ_2, using the model in equation 1.3. Estimates for job lock using both identification methods, therefore, can be taken as $\hat{\lambda}_1 = -\hat{\beta}_5$ and $\hat{\lambda}_2 = \hat{\beta}_4$. Thus a positive coefficient estimate for β_4 and a negative estimate for β_5 are evidence consistent with job lock in this model.

From an experimental design perspective, $\hat{\lambda}_1$ is favored over $\hat{\lambda}_2$ because it is based on the preferred control group in which individuals all have ECHI, like the treatment group individuals. Individuals in the alternative control group, on which $\hat{\lambda}_2$ is based, do not have ECHI – implying an important difference from the treatment group members. From a practitioner's viewpoint, however, the ideal outcome would be for the estimators to produce similar estimates, especially since sample sizes for the alternative control group are much larger than the preferred control group. Thus, even though $\hat{\lambda}_1$ is expected to be more reliable, the estimates produced by $\hat{\lambda}_2$ are still valuable for comparison with the $\hat{\lambda}_1$ estimates.

While the simple specification given in equation 1.3 is convenient for establishing the rationale behind the dual DD specification, the model is limited since it does not consider other household members. Expanding from equation 1.3 to equation 1.2 is based on the idea of including a separate dual DD for each household member in the regression. The following

model illustrates this idea in a general form:

$$\begin{aligned} Y_{i(t+1)} = {} & \beta_1 E_{it} + \beta_2 M_{it} + \beta_3 E_{it} N_{it} + \beta_4 E_{it} M_{it} + \beta_5 E_{it} N_{it} M_{it} \\ & + \sum_{j=1}^{J_{it}} (\beta_{6j} E_{itj} + \beta_{7j} M_{itj} + \beta_{8j} E_{itj} N_{itj} + \beta_{9j} E_{itj} M_{itj} + \beta_{10j} E_{itj} N_{itj} M_{itj}) \\ & + X'_{it}\gamma + u_{it}. \end{aligned} \tag{1.6}$$

Here each coefficient on the second line of the model is indexed by j, implying that they differ by household member. In this model, all household members have their own dual DD models that produce two estimates for every household member's effect on the worker's job leaving rate. That is, two estimates of λ would be generated for each household member, and each household member's estimates could be different from all others. This specification is flexible, but implementation would be difficult, as would be interpretation, since it would depend on how household members are indexed across households. To address this, I make the assumption that each household member carries the same weight in determining the worker's job leaving behavior. This assumption implies, for example, that a father does not care more about his wife losing insurance coverage than his child, but wants both to have it and acts equally in response to both of their insurance statuses. To implement this assumption, I restrict all the coefficients on the cohabitant insurance status variables to be equal across cohabitants: $\beta_{hj} = \beta_{hk} = \beta_h$ for all j, and k, and for $h = 6, 7, 8, 9$, or 10. Given this assumption, equation 1.6 becomes the model I use in my analysis, equation 1.2.

The interpretation of the estimates in equation 1.2 are analogous to the interpretation for the simple model, equation 1.3. Estimates of λ are given for the household members by $\hat{\beta}_9$ and $-\hat{\beta}_{10}$ (where $-\hat{\beta}_{10}$ is the preferred estimator). These represent the incremental change of one household member becoming less dependent on the worker's ECHI for coverage. For the worker herself, the estimates of λ are given by $\hat{\beta}_4$ and $-\hat{\beta}_5$, as in equation 1.3, but this is only in the case for my female worker sample since adult men are not Medicaid eligible. For

regressions on my male sample, I drop the Medicaid status variables for the worker himself (M_{it}, $E_{it}M_{it}$, and $E_{it}N_{it}M_{it}$) from equation 1.2. Job lock estimates for men are therefore based only on their responses to insurance coverage for their household members.

1.3.2 Job push

The empirical strategy I rely on to detect job push is similar conceptually to the one I use for job lock, but requires several important changes. The first difference is that the outcome variable of interest is Z_{it} in this analysis, an indicator for an individual moving into a job with ECHI, regardless of whether it was from another job or from non-employment. If an individual moves to an ECHI job, then Z_{it} equals one, otherwise it is zero. I use this variable to attempt to identify job push, denoted as π, which I define as

$$\begin{aligned}\pi \equiv & E[Z_{it}|i \text{ seeks employer provided health insurance during } t] \\ & - E[Z_{it}|i \text{ does not seek employer provided health insurance during } t].\end{aligned} \tag{1.7}$$

A positive value of π means that individuals seeking employer health insurance are more likely to transition from their current states into jobs offering ECHI. Thus, positive estimates of π constitute evidence of job push.

The requirement that the individual moves to a job with ECHI has not been used previously by authors studying job push. Anderson (1997) and Hamersma and Kim (2009) studied job exits without specifying anything about where the workers went, while Cooper and Monheit (1993), the closest to my approach, studied job changes for those *predicted* to gain health insurance if they switched jobs. Requiring the new job to have ECHI, though, likely improves precision since movement into jobs without ECHI is inconsistent with insurance seeking behavior, which would make their inclusion a form of dependent variable measurement error.

Until very recently, job push authors did not include the population of people moving from non-employment into employment in their analyses. This population, though, should not be ignored since the mechanism behind job push is that jobs with ECHI are more attractive than similar (or even better) jobs without such insurance. This is true regardless of the individual's state before making the transition into a job. This point has been noted by previous authors with regards to job lock, as well (e.g., Madrian, 1994; Buchmueller and Valletta, 1996; Hamersma and Kim, 2009), and the inclusion of the non-employed in studying job push is simply the analog to the job lock practice of including voluntary job quits for any reason. In addition to this current analysis, though, two papers developed concurrently to this one by Dave et al. (2013) and Garthwaite et al. (2013) also study the effect of Medicaid on people moving into jobs.

Another change in comparison to my job lock analysis is the manner in which I restrict the SIPP data to obtain my estimation sample. As discussed in Section 1.3.1, for some individuals there is theoretical ambiguity as to whether one should expect to observe job lock or job push. In an effort to address this, I omit individuals that could potentially be affected by job lock, leaving job push as the only potential effect. To be included in my job push sample, an individual must not have held ECHI insurance (from his or her own employer), *or*, if the worker did have ECHI, then all household members (including the sample member) must have been covered by employer provided insurance originating from the employer of a household member besides the sample member. Individuals without ECHI obviously could not be affected by job lock, and if everybody in the family already had an alternative source of insurance that was high quality and affordable (as would be expected since it was employer provided), then there is no reason to think the workers needed to stay in their jobs out of insurance concerns.[9]

[9]My strategy of using different data restrictions to differentiate job lock and job push is similar to that of Hamersma and Kim (2009). Their samples were both more restrictive than mine since they split their samples strictly on ECHI status of the sample member. This is not a problem for their job push analysis, though their job lock regressions may have been confounded by the presence of some individuals influenced by job push rather than job lock. As shown by the example of the woman and child I gave in section 1.3.1,

In the final change versus my job lock analysis, I redefine the ECHI and Medicaid status variables to help maximize the symmetry between my job lock and job push analyses. Let $\tilde{E}_{it} \equiv 1 - E_{it}$ and $\tilde{M}_{it} \equiv 1 - M_{it}$, with analogous redefinitions applied to household members' insurance status variables. These variables indicate when the individuals *do not* have ECHI or Medicaid eligibility, respectively. These variables can be used to define my job push model, which is the analogue of the job lock model discussed above for the job push case. The simple version of the model is given by

$$Z_{i(t+1)} = \delta_1 \tilde{E}_{it} + \delta_2 \tilde{M}_{it} + \delta_3 \tilde{E}_{it} N_{it} + \delta_4 \tilde{E}_{it} \tilde{M}_{it} + \delta_5 \tilde{E}_{it} N_{it} \tilde{M}_{it} + X'_{it}\theta + \varepsilon_{it}. \tag{1.8}$$

As a consequence of the redefinition of the ECHI and Medicaid variables, this model can be used to identify two separate job push parameters in a manner that is analogous to the method used for the job lock case:

$$\begin{aligned} \pi_1 \equiv\ & E[Z_{i(t+1)} | \tilde{E}_{it} = 1, N_{it} = 0, \tilde{M}_{it} = 1] - E[Z_{i(t+1)} | \tilde{E}_{it} = 1, N_{it} = 0, \tilde{M}_{it} = 0] \\ & - \Big(E[Z_{i(t+1)} | \tilde{E}_{it} = 1, N_{it} = 1, \tilde{M}_{it} = 1] - E[Z_{i(t+1)} | \tilde{E}_{it} = 1, N_{it} = 1, \tilde{M}_{it} = 0] \Big) = -\delta_5, \end{aligned} \tag{1.9}$$

and

$$\begin{aligned} \pi_2 \equiv\ & E[Z_{i(t+1)} | \tilde{E}_{it} = 1, N_{it} = 0, \tilde{M}_{it} = 1] - E[Z_{i(t+1)} | \tilde{E}_{it} = 1, N_{it} = 0, \tilde{M}_{it} = 0] \\ & - \Big(E[Z_{i(t+1)} | \tilde{E}_{it} = 0, N_{it} = 1, \tilde{M}_{it} = 1] - E[Z_{i(t+1)} | \tilde{E}_{it} = 0, N_{it} = 1, \tilde{M}_{it} = 0] \Big) = \delta_4, \end{aligned} \tag{1.10}$$

which is detailed in Table 1.2.

Given the above identification methods, equation 1.8 can be used to produce dual difference-in-differences estimates of job push: $\hat{\pi}_1 = -\hat{\delta}_5$ and $\hat{\pi}_2 = \hat{\delta}_4$. Hence, a positive coefficient

requiring all workers to have ECHI is not sufficient to remove all possible influence of job push.

estimate for δ_4 and a negative estimate for δ_5 are evidence consistent with job push in this model. Both of these job push estimators are based on a treatment group composed of individuals with neither ECHI or non-ECHI. Lack of Medicaid eligibility represents the treatment since those without it are expected to to seek jobs with health insurance access more frequently than those who are eligible. The preferred control group is comprised of individuals without ECHI, like the treatment group, but who do have non-ECHI, while the alternative control group is individuals with both ECHI and non-ECHI coverage. For both of these control groups, Medicaid coverage is redundant because they all have non-ECHI. Therefore, eligibility would not be expected to cause demand for ECHI to vary.[10] As in the job lock case, the estimator based on the preferred control group, $\hat{\pi}_1$, is more credible than the one based on the alternative control group, $\hat{\pi}_2$, where individuals have insurance through their employers – a potentially important difference from the treatment group. Unlike the job lock case, though, sample size concerns also favor $\hat{\pi}_1$ since membership in the alternative control group is more restrictive – it requires both ECHI and non-ECHI coverage.

The full model used in my estimation, which includes terms for insurance status of household members, is derived using the same reasoning as the job lock case, and is given by the following:

$$\begin{aligned} Z_{i(t+1)} = {} & \delta_1 \tilde{E}_{it} + \delta_2 \tilde{M}_{it} + \delta_3 \tilde{E}_{it} N_{it} + \delta_4 \tilde{E}_{it} \tilde{M}_{it} + \delta_5 \tilde{E}_{it} N_{it} \tilde{M}_{it} \\ & + \delta_6 \left(\sum_{j=1}^{J_{it}} \tilde{E}_{itj} \right) + \delta_7 \left(\sum_{j=1}^{J_{it}} \tilde{M}_{itj} \right) + \delta_8 \left(\sum_{j=1}^{J_{it}} \tilde{E}_{itj} N_{itj} \right) + \delta_9 \left(\sum_{j=1}^{J_{it}} \tilde{E}_{itj} \tilde{M}_{itj} \right) \\ & + \delta_{10} \left(\sum_{j=1}^{J_{it}} \tilde{E}_{itj} N_{itj} \tilde{M}_{itj} \right) + X'_{it}\theta + \varepsilon_{it}. \end{aligned} \tag{1.11}$$

[10] It may be noticed that individuals in the alternative control group for the job push sample have the same insurance status as those in the preferred control group in the job lock sample. Similarly, individuals in the preferred control group of the job push sample have the same status as those in the alternative control group for the job lock sample. This is because the requirement for control groups in both analyses is the same: Medicaid should not be expected to influence demand for ECHI. As a result, 53,017 person-wave observations are common to both samples. Men constitute 15,951 of these, and women the remaining 37,066. The groups are not entirely the same, however, because the inclusion requirements regarding household members are different between the two samples.

As before, the full model is only estimated on my sample of women, with the variables $\tilde{M}_{it}$, $\tilde{E}_{it}\tilde{M}_{it}$, and $\tilde{E}_{it}N_{it}\tilde{M}_{it}$ dropped in my estimates for men. In this full model, $\hat{\pi}_1$ estimates are produced for household members by $-\hat{\delta}_{10}$, and for the sample member herself by $-\hat{\delta}_5$ (which is not produced for men). $\hat{\pi}_2$ estimates are given by $\hat{\delta}_9$ for household members, and $\hat{\delta}_4$ for the sample member personally. The coefficient signs that are consistent with a job push effect are the same as they were in my job lock analysis. That is, in both cases, negative coefficients on the triple-interaction variables indicate an job lock or job push. Positive signs on the ECHI and Medicaid eligibility interaction variables also indicate job lock or job push. This similarity makes the interpretation of the results more straightforward, and is a result of the redefinition of the ECHI and Medicaid variables for the job push case.

1.3.3 Estimation

I depart from the bulk of the previous literature in basing estimation on the linear probability model. Previous authors have used the probit model almost exclusively. This change affords me the ability to consistently estimate models with individual-level fixed effects and state-by-year dummies, neither of which have been used before in the literature. I view the individual-level fixed effects to be of particular importance since they account for individuals' unobserved propensities to transition between employment states and jobs. These could cause inconsistency if, for example, firm and industry specific skills and experience influence peoples' wages and abilities to get good jobs. Frequent transition behavior would therefore be correlated with lower earnings and hence Medicaid eligibility. To some extent, this issue is already addressed by the DD models described above since they are designed to address unobserved factors correlated with eligibility. The fixed effects, though, would help address differences in the distribution of these unobserved propensities across the treatment and control groups.

Previous authors were unable to use individual-level fixed effects because their analyses were based on sources of variation that have very little variation over time. My design, though, is particularly suited to the use of person-level fixed effects because of the Medicaid expansion, which created time-variation in eligibility that is not based on changes in person level characteristics. The Medicaid expansion, however, has an important state-by-time component, which creates an additional concern as it could be correlated with state-specific time-trends in job mobility. The inclusion of state-by-year dummy variables in some versions of my models are, therefore, intended to address this issue.

All models are estimated via the Within Estimator.[11] Other controls include dummies for education, marital status, five-year age group, earnings decile, household income decile, firm type, number of household members, and month-year. Indicators for having a child less than two years old, between two and five, and between six and 17 are also included, and these child dummies are fully interacted with each other. Descriptive statistics for all model dependent variables and controls can be found in Tables 1.7 and 1.8. All sample descriptive statistics and regression estimates are weighted using panel weights from the SIPP longitudinal files.[12]

Two-way, cluster-robust standard errors are reported for all models (Cameron et al., 2011). The use of two-way clustering is intended to address, at least in part, the problem of individuals who move between states over the course of a panel. For example, consider the case of a man who moves from California to South Carolina. Since he could be influenced by

[11] All regression estimates including standard errors are implemented using Stata module "xtivreg2" (Baum et al., 2010; Schaffer, 2010) via Stata/SE 12.1 for Windows (StataCorp, 2011).

[12] Since my analysis combines five SIPP panels (described in Section 1.4 below) which cover nearly eight full years, I adjust the weights to account for growth in the American population over this period. I adjust all weights such that for any particular wave, the sum of all weights in the pooled sample (before I restrict the data) is equal to the country population used as the base for the 1990 panel weights. So if A is the sum of the 1990 panel weights for any wave (all waves have the same sum), and B_p is the sum of the weights for a given wave of panel p, then I multiply all the weights for panel p by $(A/B_p) \times 0.2$. This adjustment factor for each panel is: 0.20789744 for 86, 0.20607208 for 87, 0.20414484 for 88, 0.2 for 90, and 0.19590615 for 91. The effect of this adjustment is to increase the weight for the earlier panels since, in comparison to individuals drawn in the 1990 panel, respondents in the earlier panels were more likely to be chosen to be panel members (because the population was smaller during the time of those panels). The unadjusted panel weights do not reflect this difference.

state-specific factors in both states that are not accounted for explicitly, his error term could be correlated with those of other Californians and South Carolinians both. This implies that clustering by one state only, say California, is deficient, since no correlation with residents of South Carolina is assumed.

To implement two-way clustering to address this issue, I create two variables where one equals California and the other South Carolina for each of the man's observations in the panel. More generally, for any worker moving between two states, one variable identifies the first state of residence and the other identifies the second. Workers who do not move have both variables equal to the same state where he or she lives. Both state variables are then used as cluster variables in calculating the two-way-clustered standard errors. Tables 1.3 and 1.4 present hypothetical data illustrating the implementation of these state variables.

Table 1.5 summarizes the types of observations that will be allowed to be correlated under this method. For a given pair of individuals, they are either both stayers or both movers, or there is one stayer and one mover. Table 1.5 divides the observations into these categories and lists them by whether the individuals have any values in common for either cluster variable. For those pairs that do have common values for a cluster variable, general forms of correlation are permitted, while for those with no common values, the correlation is assumed to be zero. As compared to the alternative of clustering on only one state, this method allows for correlation in two additional categories that would have been assumed to be zero. These additional categories of correlation (rows (4) and (8)) are identified in Table 1.5 by row shading.

The primary weakness of using this method of clustering is that one could argue that correlation should be allowed for the categories found in rows (9), (10), and (11), but because there is no common value between observations for either cluster variable, the correlation is assumed to be zero. For example, suppose Person One lived in California for waves one through four then moved to South Carolina, while Person Two lived in South Carolina the

first wave then moved to California for the rest of the panel (i.e. the case found in row (9)). These two people both lived in California at the same time for three waves, yet their error terms are assumed uncorrelated. Therefore, to the extent that this type of correlation is truly important in my sample, my standard error estimators will be inconsistent. However, as compared to available alternatives, this method would seem to be an improvement.[13]

1.4 Data

I perform my analysis using the 1986, 1987, 1988, 1990, and 1991 panels of the SIPP (U.S. Dept. of Commerce, Bureau of the Census, 2006, n.d.), an individual-level, longitudinal survey. The SIPP collected information on the resident population of the United States of America, excluding those who lived in military barracks or institutions. The panels each surveyed completely separate samples of individuals which were designed to be nationally representative. Hence, residents of all 50 states and the District of Columbia were surveyed, though Alaska, both Dakotas, Idaho, Iowa, Maine, Montana, Vermont, and Wyoming all were not separately identified in the data due to confidentiality concerns arising from their small populations. The SIPP selected its survey subjects by choosing homes and interviewing the people who lived there. Only individuals 15 years or older were eligible to be SIPP interviewees, though information about household children was collected through the interviews of household adults. When respondents moved to new addresses, the survey followed them (when possible) and continued to include them in the survey.

Individuals were interviewed every four months over the life of the panels, with each four-month-long subdivision known as a panel "wave". Each panel's sample was divided into

[13]Individuals who move more than once over the course of a panel are not included in my sample (see Section 1.4). However, these individuals could be included, in principle, and this method of clustering extended by creating variables for all of the states where individuals lived and using higher dimension clustering in the same fashion. The weakness regarding the handling of movers living in a place at the same time but without common values for any cluster variable would be compounded, though, so this method may not be justified in some cases.

four interview rotation groups which were interviewed separately on a rotating basis over the four months of a wave. This resulted in a survey schedule where every month during the span of the panel one of the rotation groups was in the process of being interviewed. The interviews are backwards looking, covering the four months immediately preceding the interview month. For example, for the individuals interviewed in February 1987, their interviews cover the period from October 1986 through January 1987, while respondents interviewed in May 1989 were questioned about the period January to April of the same year. Due to this backward looking design, for the people who remained in the survey over the entirety of the panel, it is possible to assemble an observation for every month over the entire period of the survey. However, the SIPP survey was not designed to track changes in an individual's source of health insurance over the course of a wave, and so I only use one observation per respondent, per wave, dropping all observations except those from the month that immediately preceded the individual's interview month. This has the additional advantages of helping to minimize error in the respondent's recollection and avoid the so-called "seam bias" (Gruber and Madrian, 1997; Ham and Shore-Sheppard, 2005).

Each panel followed respondents for at least two years. The 86 and 87 panels lasted seven waves,[14] while 88 lasted six, and 90 and 91 both lasted eight waves. The 1989 panel was not included in my analysis since it only lasted three waves.[15] Since a new panel started each year during this time period, the length of the surveys implied panel "overlap", meaning that waves from more than one panel occurred at the same time. For example, the last four waves of the 87 panel all occurred at the same time as one of the first four waves of the 88 panel. After combining all five panels, my data includes observations for every month from January 1986 through August of 1993.

[14]One of the rotation groups of the 1986 panel (i.e. a quarter of its entire sample) was only included in six waves due to budget constraints.

[15]Both the 1988 and 1989 panels were discontinued early as part of the Census Bureau's preparations for the 1990 SIPP which was larger and required more resources. See Appendix A of the 1991 (second edition) of the SIPP User's Guide.

I enforced a number of restrictions on the data to obtain my final analysis samples. Table 1.6 lists the impact of these restrictions on my sample sizes in detail. Only month four observations were used and sample members were required to be between the ages of 21 to 55 and have a valid interview for all waves. Since Medicaid eligibility is dependent on the individual's state of residence, I only included individuals who lived in one of the uniquely identifiable states for all waves. I also excluded individuals who moved between states more than once, since those moving frequently were probably different in important ways from other sample members (and they comprise only a very small portion of the SIPP sample). Individuals who reported receiving SSI were dropped since they likely had a disability that drastically alters labor force attachment.

Individuals with jobs were only included in my samples if they reported holding one job only at the end of a given wave *and* at the beginning of the next wave. The reason for requiring the individual to have only one job is that the SIPP survey did not observe which employer provided the respondent's ECHI insurance explicitly. If a worker reported working two jobs simultaneously, it is not clear which provided the insurance coverage.[16] Considering only workers with one job helps eliminate this ambiguity. This is also the motivation for requiring the report of one job at the beginning of the next wave. Since I observe state transitions in the wave following the one in which I observe ECHI status, requiring one job at the start of the following wave helps identify if the transition came from a job with ECHI or not.[17]

The job lock and job push samples are differentiated as already described above in Sections 1.3.1 and 1.3.2. Two additional restrictions were also made for estimation reasons: I excluded potential sample members who would only be observed in the sample one time (so that person

[16]The SIPP collected information on up to two jobs (plus self-employment, if any).

[17]This step would not be necessary in more recent panels of the SIPP, since employer ID numbers are consistent across waves. During the 1980s, however, this was not the case (Stinson, 2003). This step does have the benefit, though, of providing a check of the worker's survey responses. Since the end of a wave and beginning of the next are supposed to be the same time period, it shows that the respondent is providing employment status consistently for the same point in time at two separate interviews.

fixed effects could be estimated) and dropped respondents with zero panel weights.

1.4.1 Imputation of Medicaid eligibility

One of the prerequisites to implementing the analyses described in Sections 1.3.1 and 1.3.2 is that Medicaid eligibility must be observed for sample members. While the SIPP questionnaire does ask respondents about Medicaid enrollment, it does not explicitly observe eligibility for those not actually enrolled. This issue was previously addressed by authors working in different contexts, though. For my analysis, I follow Currie and Gruber (1996a,b), Cutler and Gruber (1996), Gruber and Yelowitz (1999), and Gruber and Simon (2008)[18] and impute Medicaid eligibility on the basis of observable data and detailed, state-level eligibility rules. I rely on the programs developed and used by Gruber and Yelowitz (1999) to impute eligibility for my study since their analysis used the SIPP during the same time period I use.[19] In my data, I code individuals as Medicaid eligible if they are imputed as eligible by the Gruber and Yelowitz program or are reported as actually enrolled in the SIPP.

I impute eligibility for children up to and inclusive of age 20, the highest possible age a person could be eligible as a child.[20] For women, I follow Gruber and Yelowitz (1999) and I impute pregnancy eligibility for the child bearing ages of 15 through 44. Since I include women up to age 55 in my analyses, the imputation of eligibility through 44 implies that I

[18] As a literature, this series of papers presents a very interesting analysis of the Medicaid expansion, and illustrates how government programs have both positive and negative consequences. Currie and Gruber (1996a,b) both focused on the positive effects of the expansion, arguing that it was responsible for the increased utilization of health care and a reduction in child and infant mortality and a lower incidence of low birth weights. Cutler and Gruber (1996) and Gruber and Simon (2008) focused on the arguably negative outcome that the expansion appears to have induced eligible individuals to substitute out of private health insurance into public health insurance. Gruber and Yelowitz (1999) likewise focus on an arguably negative consequence by suggesting that Medicaid eligibility was associated with reduced saving and increased consumption.

[19] The eligibility imputation programs developed by Gruber and Yelowitz were obtained from Professor Yelowitz.

[20] Eligibility for older children came through the "Ribicoff Children" program Congressional Research Service (1988). Gruber and Yelowitz (1999) only imputed child eligibility through age 18 for their analysis. This was the only change I made in using their imputation programs.

have assumed that there is no value for Medicaid eligibility for women 45 and older. To the extent that women above 44 do not expect to have pregnancy expenses, this is a reasonable assumption. For women from ages 15 through 20, I code the variable M_{it} equal to one if they are eligible either as a child or for pregnancy coverage. Since explicit enrollment in Medicaid implies eligibility, I also code individuals who report Medicaid coverage in the SIPP survey as eligible even if the imputation program did not impute them as eligible (as long as the individuals reporting Medicaid coverage satisfied the age limits used in my imputation of age 20 or younger for males and age 44 or younger for females).

1.5 Results

Tables 1.9 through 1.11 contain my main regression results, which are presented separately for job lock and job push, and for both men and women. It should be noted that all estimates are multiplied by 100 and all models included individual-level fixed effects and year-month dummies. Versions of all models were run with state-by-year interaction dummies as well, producing estimates that were all very similar to the results without them, and so are not reported. Each table contains estimates based on three samples: the full sample; one restricted to individuals who were married each time they were observed in the data; and one restricted to married people *and* excluding observations of individuals with household income or job earnings in the top decile. The marriage restriction has been common in the job lock literature given that dual insurance coverage is most likely among married couples. My models also involve children, providing additional reason to examine the results for married people. The most important reason for the top income exclusion is that such individuals are unlikely to be Medicaid eligible. Additionally, since incomes are top coded in the SIPP, there is potentially a very large range of incomes in the top decile. Lastly, it was previously argued by Hamersma and Kim (2009) that those with high incomes are unlikely to be affected by

job lock.[21]

1.5.1 Job lock

Table 1.9 presents my estimates of equation 1.2 for men in my job lock sample. As previously mentioned, since adult men are not Medicaid eligible, the model here drops the eligibility variables for the worker personally. Columns (1) through (3) contain the results for the main analysis in which voluntary job exits is the outcome variable. Column (1) shows the results for the full sample. The job lock estimate given by the preferred estimator, $\hat{\lambda}_1$ (the negative of the β_{10} coefficient), is 0.58, which means that an additional household member eligible for Medicaid results in an increase in the likelihood of the worker leaving his job in the next four months by a little more than half a probability point (that is, the change in probability times 100). This estimate is statistically significant at the five percent significance level. Compared to the voluntary exit rate of 1.7% for men in households where all members are dependent on his ECHI for coverage, this estimate represents an increase of approximately 34%. The estimates generated by $\hat{\lambda}_1$ for the restricted samples in Columns (2) and (3) are both larger than in Column (1). For the the sample of married men, the estimate is 0.63, while for married men excluding top earners it is 1.3 (with the first being significant at the five percent level and the second at the one percent level). These estimates imply increases in job mobility of 37% and 76%, respectively, for each additional eligible household member.

Turning next to the estimates generated by the alternative estimator, $\hat{\lambda}_2$ (given by the β_9 coefficient), we see that it produces an estimate of 0.44 in Column (1)'s full sample, 0.95 in the sample of married men in Column (2), and 0.88 for the married men excluding high earners in Column (3). Whereas these estimates also suggest large levels of job lock – increases of job exits of 26%, 56%, and 52%, respectively, due to Medicaid eligibility for one household

[21] This argument would seem questionable, though, given the tax-free nature of ECHI. As income increases, the tax benefit becomes larger, suggesting a greater importance of ECHI as a job benefit for higher income workers.

member – only the estimate in Column (2) is significant (at the ten percent level). The primary driver of the difference in significance as compared to the $\hat{\lambda}_1$ estimator is decreased precision, as all three standard errors for the $\hat{\lambda}_2$ estimates are at least 60% larger than the $\hat{\lambda}_1$ standard errors. Thus, the two different estimators in this case give fairly consistent results, even though they are not in perfect agreement with regard to statistical significance.

Columns (4) through (6) of Table 1.9 present a supplementary set of regressions on a placebo outcome, involuntary terminations (which was previously used by Hamersma and Kim, 2009). Here, the dependent variable is equal to one if the worker reports being laid-off or discharged over the next four months, and zero otherwise. One of the key arguments behind job lock is that the worker chooses to stay in his or her job to retain access to health insurance. The worker's choice is irrelevant though if the employer terminates the worker. Thus, we would not expect a job lock effect on involuntary terminations. However, using them for a placebo test is informative because it helps determine if unobserved trends or factors are biasing the results for voluntary terminations. For example, suppose the economy worsened causing employers to lay off and reduce hours for many workers. As a result, Medicaid eligibility might be correlated with the economic downturn (because of the hours reductions). If workers often report to survey takers that they quit when they were actually laid off (perhaps out of embarrassment), then the result would be a spurious positive estimate of job lock when using voluntary terminations as the outcome variable. However, a positive result would also be found when involuntary terminations was used as well (assuming some survey respondents tell the truth), meaning such a regression could be used to search for this sort of problem. Examining the estimates from my placebo regressions on my job lock sample of men, there is not any apparent source of bias affecting the results in Table 1.9. I find all are comparatively small in magnitude and are not statistically significant at conventional levels, even though standard errors are similar to those found in the voluntary exit regressions.

Table 1.10 presents estimates of equation 1.2 on my sample of women. Since women can be

eligible for Medicaid pregnancy coverage, I can estimate all of the parameters of equation 1.2, including the effect of coverage for the worker herself. In contrast to the estimates for men, the women's results are not consistent with a theory of job lock. Almost all of the estimates in Columns (1) through (3) have the "wrong" signs, and none of them are statistically significant. The magnitudes of the estimates, though, are relatively large. For example, the smallest estimate in magnitude comes from the $\hat{\lambda}_1$ estimator for the effect of coverage for the worker herself (the negative of the β_5 coefficient) for the sample of married women in Column (2), which suggests a *decrease* in the probability of a job exit by 0.25 probability points. The overall rate of voluntary job exits in my job lock sample for women who have no source of health insurance for themselves and their household members besides ECHI is 2.5%. Compared to this benchmark, the estimate of -0.25 represents a 10% reduction in job mobility.

Inspection of the results from the placebo regressions reported in Columns (4), (5), and (6) does not reveal any obvious biases that could be influencing the main regressions. The placebo estimates are generally smaller in magnitude than the main estimates, and even though they are estimated more precisely in nearly every case, none of the placebo estimates are statistically significant. Thus, if there is a true job lock effect for women, I am unable to find evidence of any unobserved trends that may have obscured it.

1.5.2 Job push

Table 1.11 presents my estimates of my job push model, equation 1.11, in which the dependent variable is moves to jobs with ECHI. Results for both men and women are reported in this table, since there is no available placebo outcome for the job push case.[22] Columns

[22]A valid placebo outcome would have to be something similar to transitions into jobs with ECHI, but one on which it is clear that eligibility for Medicaid would have no influence. In the job lock case, I relied on the fact that involuntary terminations are the employer's choice, not the employee. An analogous outcome in the job push case would require the observation of situations where an individual did not choose to transition

(1), (2), and (3) contain the results for men. For the full sample estimates in Column (1), the preferred estimator, $\hat{\pi}_1$ (the negative of the δ_{10} coefficient), produces a job push estimate of 0.45, which is statistically significant at the one percent significance level. This means that an additional household member eligible for Medicaid appears to decrease the man's likelihood of moving to a job with ECHI by a little less than half a percentage point. In my job push sample, for households where all members are without health insurance, men transition into jobs with ECHI over four month periods at an overall rate of 1.8%. Based on this benchmark, the estimate of 0.45 implies eligibility decreases job push by approximately 25%.

Unlike the $\hat{\pi}_1$ estimator in Column (1), the rest of the job push estimates for the sample of men are not statistically significant at conventional levels. Nevertheless, the rest of the estimates are otherwise consistent with the presence of job push. All the estimators have the expected signs, and the estimates are relatively large. For example, the smallest in magnitude is given by the alternative estimator, $\hat{\pi}_2$ (the δ_9 coefficient), for the full sample in Column (1). At 0.27 percentage points, this estimate suggests a 15% reduction in job push due to Medicaid eligibility for a household member. As compared to the estimate given by $\hat{\pi}_1$, the lack of statistical significance for this estimate is driven by the larger standard error, which is much more than twice as large. In fact, all the job push estimates for men in Table 1.11 would be significant at the ten percent level (or higher) if all the estimates had standard errors the same size as that for $\hat{\delta}_{10}$ in Column (1).

Columns (4) through (6) in Table 1.11 report job push results for my sample of women. As in the job lock analysis, for women I can estimate all of the parameters of equation 1.11 since women can be eligible for Medicaid pregnancy coverage. Considering first the effect of eligibility for the analysis sample member herself, we can see that the results are consistent, at least partly, with a job push effect. The estimates from all three samples are large in

into a job – *but did anyway*. Obviously such an observation would not be possible.

magnitude and have the expected signs. Of particular interest are the estimates generated by the alternative estimator, $\hat{\pi}_2$ (the δ_4 coefficient), which are extremely large in magnitude. For the full sample, I estimate that the likelihood of transitioning to jobs with ECHI falls by 1.6 percentage points when the worker herself is eligible for Medicaid, while for the married women sample the estimate is 3.1 (significant at the five percent level), and for the married women excluding top incomes sample it is 3.4 (which is also significant at the five percent level). Considering that the benchmark, four-month-period transition rate for women in households where no one has insurance is 1.3%, these estimates are problematic since they imply that eligibility reduces the transition rates by much more than 100%. One possible reason for results like this is that the women in the alternative control group may be too different from the women in the treatment group, leading to unrealistic estimates. Adding to the plausibility of this explanation is that the estimates given by the preferred estimator, $\hat{\pi}_1$ (the negative of the δ_5 coefficient), are much smaller in magnitude. The largest of these comes from the sample of married women, and at 0.49 it represents an effect of approximately 38%. While still a large estimate, it is much more realistic than the $\hat{\pi}_2$ estimate for this sample of an effect of almost 240%.

In contrast to the results for the effect due to coverage for the worker herself, the estimates for the effect of coverage for household members are not consistent with a job push effect. All of the estimates have the opposite of the expected sign, and none of the estimates are statistically significant. Some of the estimates, particularly those given by $\hat{\pi}_2$ (the δ_9 coefficient), are fairly large in magnitude despite that their signs indicate the opposite of a job push effect. For example, the full sample estimate of the δ_9 coefficient is -0.19, which indicates an *increase* in job push of about 15%.

Considering all of the estimates for the sample of women as a whole, it is hard to see the results as strong evidence of a job push effect for women. Although the estimates for the effect of the worker's own coverage are large in magnitude, the fact that the estimates seem

unrealistically large combined with the fact that the estimates for household members' coverage have the opposite signs gives sufficient reason to question the own coverage estimates. In particular, it seems hard to believe that women would behave differently when their children gain health insurance coverage than they would when they themselves gain coverage. And so, it would seem that the evidence presented here is weak at best regarding a job push effect for women.

1.5.3 Estimates using a state-level measure of eligibility

Despite that my regression models include several features intended to minimize bias due to endogeneity, there still might be concern about my estimates due to the fact that Medicaid eligibility can be chosen by an individual by working fewer hours or choosing a job that pays less. Perhaps more importantly, there is also concern that imputed Medicaid eligibility might be measured with error, something for which I have not made any attempt to compensate to this point. Since imputed eligibility relies on the measurement of individual characteristics via the SIPP, if those characteristics are mis-measured in the survey, the resulting imputation would also contain error. Moreover, this mis-measurement could be compounded due to the use of individual-level fixed effects.

To address both of these issues, I calculate a state-level measure of the generosity of Medicaid eligibility rules, and use it to replace individual-level eligibility in my regressions. Here state-level generosity is measured by the probability of eligibility in a given state conditional on individual-level characteristics that are more convincingly exogenous. This is essentially the strategy relied upon by Currie and Gruber (1996a), Currie and Gruber (1996b), Cutler and Gruber (1996), Gruber and Yelowitz (1999), Ham and Shore-Sheppard (2005), and Gruber and Simon (2008), who were working in other research areas, and has similarities to the approach of Hamersma and Kim (2009) in the job lock literature.

In my job lock models, I indicate the probability of eligibility as I_{it} (since it was used as an instrumental variable in most of the previous literature). The value of I_{it} varies on the state of residence, time (quarter), age, and education level (for kids education level is based on household adults). Before describing the calculation of I_{it} more carefully, the basic idea is to start with a static, national sample – static to eliminate the possibility of time trends in population characteristics, national to remove state-specific characteristics – and calculate the eligibility of the whole population *as if they all lived in a given state*, regardless of where the people actually lived. The probability of eligibility, then, is taken as the percentage of eligible individuals in this sample within the state, time, age, and education level categories.

The advantage of this strategy is that this state-level measure of eligibility no longer depends on characteristics that are often changed by the individual (state and education can vary in concept but seldom do for working age adults), and so it reduces the threat of endogeneity. Additionally, since the state-level calculation is based on the imputed eligibility for many individuals, the impact of mis-measurement for individuals plays a lesser role. The problem is transformed from one of measuring eligibility for particular individuals to one of measuring the policies of a state – which a fundamentally easier task. Thus, we would expect the errors-in-variables problem to be reduced by this approach. The major caveat, though, is that this approach will not address any mis-measurement introduced by errors in the Medicaid imputation program itself.[23]

The calculation of I_{it} starts with the entire first wave sample from the 1990 panel of the SIPP, which has the largest sample size of any wave in my data. For each state and each quarter in my panel, I impute eligibility for *almost* all individuals in the sample as if they were residents of the state in the given quarter. I write almost because no individuals were used in the calculation of the eligibility probability for the state in which they were actual residents during the first wave of the 1990 panel. So the measure of state-level eligibility is,

[23]Currie and Gruber (1996a), Currie and Gruber (1996b), and Cutler and Gruber (1996) have more extensive discussion about the exogeneity and measurement error characteristics of I_{it}.

in a sense, a type of "leave-one-out estimator". This is intended to reduce the influence of state-specific population characteristics on the calculation of I_{it} (and follows the method used by Ham and Shore-Sheppard, 2005). I adjust incomes used in the imputation process for inflation using the Consumer Price Index (so dollar amounts reflect prices prevailing during the given quarter, not 1990). Also, I adjust birth dates so that individuals are calculated to have the same age in the given quarter as they did in wave one of the 1990 panel.

Once this eligibility imputation process is complete, I compute the weighted average of imputed eligibility (using the SIPP final person weights) within the state, quarter, age, and education level categories. For children, there is a separate age category for each year from age zero through 20. The education level is taken as the highest education level of adults 18 years or older in the household (less than high school, high school graduate, some college, or four years of college or more). If there are no adults in the household, then the education level of the child him- or herself is used. For women aged 15 or older, the education level of the woman herself is used, and age groups are one for each year for those 22 or younger, and then two-year groups for those 23 and older. The full interaction of these age and education levels along with the state and quarter variables creates the average eligibility categories – with one important exception: education level is not used in creating the categories for women 22 or younger. The reason for this is that many people are still in the process of determining their final education level at that age. So instead of interacting age with education level, I used a different age category for each year. After 22, the age categories are two-year groups to ensure enough individuals in each age-by-education category. Finally, since that women age 15 through 20 could be eligible for Medicaid either as a child or for pregnancy coverage, for women in this age range I use the greater of the two probabilities as their state-level measures of eligibility.

Given this calculation of I_{it}, it replaces M_{it} in the models used to estimate job lock and job

push above. For job lock, the model becomes

$$\begin{aligned} Y_{i(t+1)} = {} & \beta_1 E_{it} + \beta_2 I_{it} + \beta_3 E_{it} N_{it} + \beta_4 E_{it} I_{it} + \beta_5 E_{it} N_{it} I_{it} \\ & + \beta_6 \left(\sum_{j=1}^{J_{it}} E_{itj} \right) + \beta_7 \left(\sum_{j=1}^{J_{it}} I_{itj} \right) + \beta_8 \left(\sum_{j=1}^{J_{it}} E_{itj} N_{itj} \right) + \beta_9 \left(\sum_{j=1}^{J_{it}} E_{itj} I_{itj} \right) \\ & + \beta_{10} \left(\sum_{j=1}^{J_{it}} E_{itj} N_{itj} I_{itj} \right) + X'_{it}\gamma + u_{it}. \end{aligned} \tag{1.12}$$

For job push, after defining $\tilde{I}_{it} = 1 - I_{it}$ and replacing $\tilde{M}_{it}$, the model becomes

$$\begin{aligned} Z_{i(t+1)} = {} & \delta_1 \tilde{E}_{it} + \delta_2 \tilde{I}_{it} + \delta_3 \tilde{E}_{it} N_{it} + \delta_4 \tilde{E}_{it} \tilde{I}_{it} + \delta_5 \tilde{E}_{it} N_{it} \tilde{I}_{it} \\ & + \delta_6 \left(\sum_{j=1}^{J_{it}} \tilde{E}_{itj} \right) + \delta_7 \left(\sum_{j=1}^{J_{it}} \tilde{I}_{itj} \right) + \delta_8 \left(\sum_{j=1}^{J_{it}} \tilde{E}_{itj} N_{itj} \right) + \delta_9 \left(\sum_{j=1}^{J_{it}} \tilde{E}_{itj} \tilde{I}_{itj} \right) \\ & + \delta_{10} \left(\sum_{j=1}^{J_{it}} \tilde{E}_{itj} N_{itj} \tilde{I}_{itj} \right) + X'_{it}\theta + \varepsilon_{it}. \end{aligned} \tag{1.13}$$

These models are then estimated on the same samples as the individual-level eligibility models.

In interpreting the results of the state-level measure models above, the focus should be on whether they tell the same story as the main regressions in terms of the signs and magnitudes of the point estimates. If endogeneity and measurement error are truly problems that cause faulty conclusions, then the coefficient estimates should be drastically different when using the state-level measure that is thought to suffer less from these problems. Whether or not the state-level estimates are statistically significant, however, should be viewed as less important because state-level measures vary less than individual-level measures, resulting in estimates that are less precise. This is acceptable for the purposes of this analysis, though, because the problems of endogeneity and measurement error are ones that cause inconsistency of coefficient estimates, not standard errors that are too small. Thus, if the state-level results tell the same story as the individual-level results – whether they are statistically

significant or not – this should give us confidence that the individual-level estimates are not suffering from inconsistency and can therefore be viewed to be as credible as the state-level estimates. If that is the case, then the appropriate estimates are the individual-level results, which are more precise by nature, since the issue of ECHI effects on job mobility is one of individual-level insurance coverage on individual-level behavior. Therefore, the state-level results are intended to be viewed as robustness-checks on the individual-level results, rather than primary analyses of their own.

Table 1.12 reports my estimates for equations 1.12 and 1.13. For men, the results using the state-level measure of eligibility suggest similar conclusions to the results from the individual-level measure. All estimates of job lock and job push, except one, have the expected sign for men, which is consistent with the individual-level results. Surprisingly, this pattern of similarity holds for the statistical significance of the estimates as well, despite the loss in precision.

Turning next to women, once again the estimates for job lock are not consistent with the theory. The sign pattern is the same for both the individual- and state-level analyses, but they are the "wrong" signs in almost all cases. In the job push case, we had seen that for women the strongest evidence of a job mobility effect came from the estimates for the effect of Medicaid eligibility for the woman herself. Using the state-level measure, however, these estimates have the opposite of the expected signs when estimated in the full sample. Despite that the estimates for own eligibility for the married women samples have similar estimates as the individual-level analysis, the full sample difference provides another reason to view those results with some skepticism. For the estimates for the effect of eligibility of household members, none have the expected sign, which is consistent with the results from the individual-level measure regressions.

1.6 Conclusion

The results of my analysis paint two different pictures about job mobility effects of ECHI for men and women. Among men, I find fairly strong evidence of both job lock and job push, with the strongest evidence consistent with the theory coming from the most credible estimators – those based on comparison groups with the same ECHI status as the treatment groups. My job lock results for men provide some validation of Madrian (1994) in relation to the critiques levied by Kapur (1997) (both of whom only analyzed men). The preferred estimators in my analysis, like Kapur's, are based on the comparison of groups that are more similar than those used by Madrian, but my source of variation in the demand for ECHI improves on Kapur's and allows me to further increase credibility through the inclusion of individual-level fixed effects and state-by-year interaction dummies. With this framework, and using data from the same time period as these authors, I estimate effects for men that are similar to those estimated by Madrian.

In contrast to my results for men, for women I do not find evidence of pervasive job lock effects, as none of my estimates are statistically significant and generally my estimates have the opposite of the signs one would expect if there is such an effect. This result is consistent with Holtz-Eakin (1994) and Berger et al. (2004) (and to a lesser extent, Hamersma and Kim (2009), who only found a small job lock effect for unmarried women). This result, however, conflicts most particularly with Buchmueller and Valletta (1996), who found consistently large job lock effects among women. For job push, I find weak evidence of an effect for women, which is at least partially in conflict with the results of Hamersma and Kim (2009), who found no job push effect for women. I describe the evidence as weak despite the fact that some of my estimates are large because the strongest evidence of an effect in my analysis comes from the less credible estimators in my regressions, and they produce estimates that are unrealistically high. Moreover, when state-level eligibility is used for identification, the estimates from my full sample of women change signs. Most troubling, though, is that the

estimates are inconsistent between personal Medicaid eligibility for women versus eligibility for their household members. Considering that, in most cases, the household members are children of the sample member, it is hard to reconcile this result with what people generally understand about relationships between women and their children. Thus, I view my results for job push for women suspiciously, and suggest that all of my results (both for job lock and job push) indicate that the ECHI effects of job mobility among women is an area in need of further research.

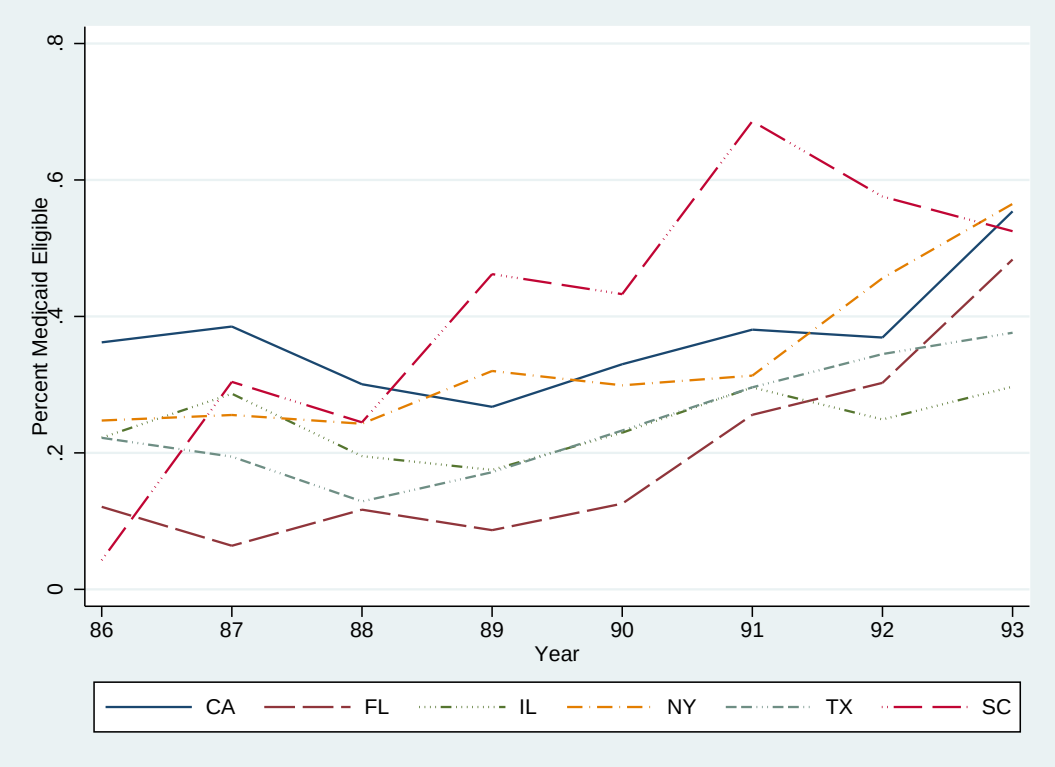

Figure 1.1: Expansion of Medicaid for children under 15 for the five largest states and the author's home state of South Carolina. Estimates are for June of each year and are weighted.

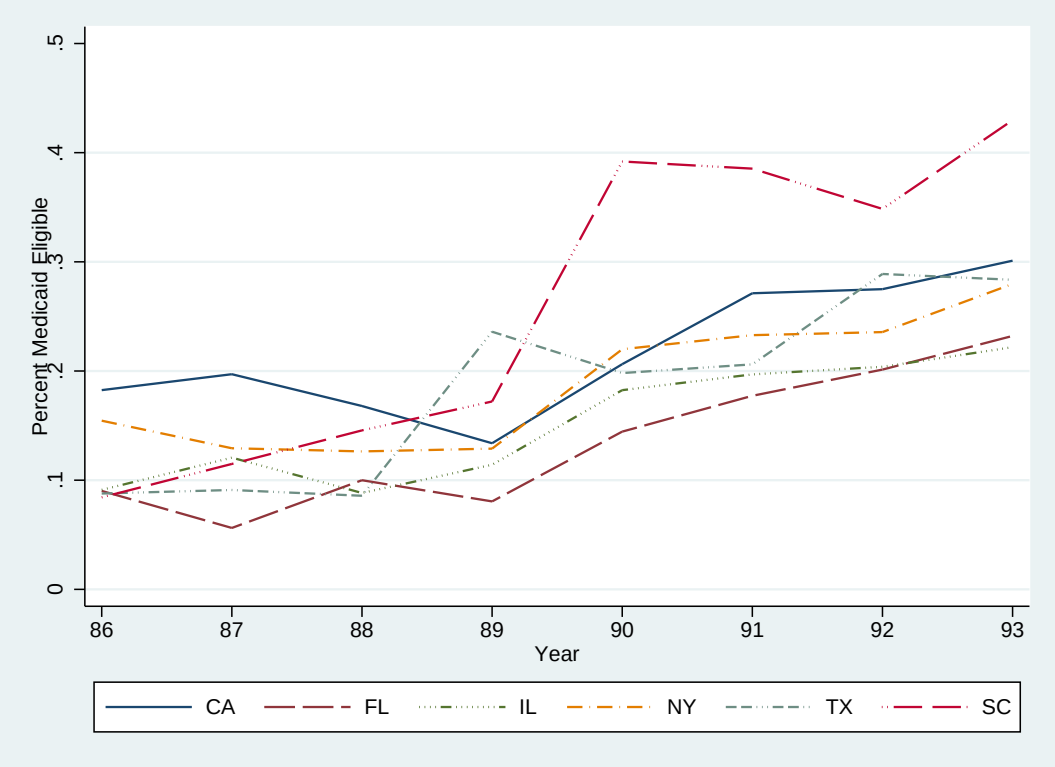

Figure 1.2: Expansion of Medicaid for women 21 or older for the five largest states and the author's home state of South Carolina. Estimates are for June of each year and are weighted.

Table 1.1: Identification of *job lock* parameters

Example Model: $Y_{i(t+1)} = \beta_1 E_{it} + \beta_2 M_{it} + \beta_3 E_{it} N_{it} + \beta_4 E_{it} M_{it} + \beta_5 E_{it} N_{it} M_{it} + u_{it}$

	ECHI (E_{it}) and Non-ECHI (N_{it}) Insurance Status		
Medicaid Eligible?	Treatment Group $E_{it} = 1, N_{it} = 0$	Preferred Control Group $E_{it} = 1, N_{it} = 1$	Alternative Control Group $E_{it} = 0, N_{it} = 1$
$M_{it} = 1$	$E[Y_{i(t+1)} \mid E_{it} = 1, N_{it} = 0, M_{it} = 1] = \beta_1 + \beta_2 + \beta_4$	$E[Y_{i(t+1)} \mid E_{it} = 1, N_{it} = 1, M_{it} = 1] = \beta_1 + \beta_2 + \beta_3 + \beta_4 + \beta_5$	$E[Y_{i(t+1)} \mid E_{it} = 0, N_{it} = 1, M_{it} = 1] = \beta_2$
$M_{it} = 0$	$E[Y_{i(t+1)} \mid E_{it} = 1, N_{it} = 0, M_{it} = 0] = \beta_1$	$E[Y_{i(t+1)} \mid E_{it} = 1, N_{it} = 1, M_{it} = 0] = \beta_1 + \beta_3$	$E[Y_{i(t+1)} \mid E_{it} = 0, N_{it} = 1, M_{it} = 0] = 0$
Column Difference	$= \beta_2 + \beta_4$	$= \beta_2 + \beta_4 + \beta_5$	$= \beta_2$

Job lock parameter 1: $\boldsymbol{\lambda_1} \equiv \beta_2 + \beta_4 - \beta_2 - \beta_4 - \beta_5 = \boldsymbol{-\beta_5}$

Job lock parameter 2: $\boldsymbol{\lambda_2} \equiv \beta_2 + \beta_4 - \beta_2 = \boldsymbol{\beta_4}$

Table 1.2: Identification of *job push* parameters*

Example Model: $Z_{i(t+1)} = \delta_1 \tilde{E}_{it} + \delta_2 \tilde{M}_{it} + \delta_3 \tilde{E}_{it} N_{it} + \delta_4 \tilde{E}_{it} \tilde{M}_{it} + \delta_5 \tilde{E}_{it} N_{it} \tilde{M}_{it} + \varepsilon_{it}$

	ECHI ($\tilde{E}_{it}$) and Non-ECHI (N_{it}) Insurance Status		
Medicaid Eligible?	Treatment Group $\tilde{E}_{it} = 1, N_{it} = 0$	Preferred Control Group $\tilde{E}_{it} = 1, N_{it} = 1$	Alternative Control Group $\tilde{E}_{it} = 0, N_{it} = 1$
$\tilde{M}_{it} = 1$	$E[Z_{i(t+1)} \mid \tilde{E}_{it} = 1, N_{it} = 0, \tilde{M}_{it} = 1]$ $= \delta_1 + \delta_2 + \delta_4$	$E[Z_{i(t+1)} \mid \tilde{E}_{it} = 1, N_{it} = 1, \tilde{M}_{it} = 1]$ $= \delta_1 + \delta_2 + \delta_3 + \delta_4 + \delta_5$	$E[Z_{i(t+1)} \mid \tilde{E}_{it} = 0, N_{it} = 1, \tilde{M}_{it} = 1]$ $= \delta_2$
$\tilde{M}_{it} = 0$	$E[Z_{i(t+1)} \mid \tilde{E}_{it} = 1, N_{it} = 0, \tilde{M}_{it} = 0]$ $= \delta_1$	$E[Z_{i(t+1)} \mid \tilde{E}_{it} = 1, N_{it} = 1, \tilde{M}_{it} = 0]$ $= \delta_1 + \delta_3$	$E[Z_{i(t+1)} \mid \tilde{E}_{it} = 0, N_{it} = 1, \tilde{M}_{it} = 0]$ $= 0$
Column Difference	$= \delta_2 + \delta_4$	$= \delta_2 + \delta_4 + \delta_5$	$= \delta_2$

Job push parameter 1: $\boldsymbol{\pi_1 \equiv \delta_2 + \delta_4 - \delta_2 - \delta_4 - \delta_5 = -\delta_5}$

Job push parameter 2: $\boldsymbol{\pi_2 \equiv \delta_2 + \delta_4 - \delta_2 = \delta_4}$

*Note that here $\tilde{E}_{it} = 1$ implies *no* ECHI and $\tilde{M}_{it} = 1$ implies *not* Medicaid eligible.

Table 1.3: State variable creation example – stayer

ID Variable	Wave	Actual State of Residence	First State	Second State
101	1	CA	CA	CA
101	2	CA	CA	CA
101	3	CA	CA	CA
101	4	CA	CA	CA

Example data setup for a hypothetical four-wave panel.

Table 1.4: State variable creation example – mover

ID Variable	Wave	Actual State of Residence	First State	Second State
101	1	CA	CA	SC
101	2	CA	CA	SC
101	3	SC	CA	SC
101	4	SC	CA	SC

Example data setup for a hypothetical four-wave panel.

Table 1.5: Summary of error term correlation assumptions

Type of Observation Pair	Row Number	Person i First State	Person i Second State	Person j First State	Person j Second State	Correlation Allowed	Correlation of Zero Assumed
Stayer & Stayer	(1)	h	h	h	h	✓	
	(2)	h	h	k	k		✓
Stayer & Mover	(3)	h	h	h	k	✓	
	(4)	h	h	k	h	✓	
	(5)	h	h	k	m		✓
Mover & Mover	(6)	h	k	h	k	✓	
	(7)	h	k	h	m	✓	
	(8)	h	k	m	k	✓	
	(9)	h	k	k	h		✓
	(10)	h	k	m	h		✓
	(11)	h	k	k	m		✓
	(12)	h	k	m	l		✓

All regression standard errors are calculated using two-way clustering, whicht allows for error term correlation structures as summarized above. Letters i and j indicate distinct individuals and h, k, l, and m represent distinct states of residence. Shaded rows indicate categories of observations in which correlation is allowed using the two-way-cluster method suggested here, but where correlation would be assumed to be zero if one were to cluster only on the first state of residence. More detail regarding the calculation of standard errors can be found in Section 1.3.3.

Table 1.6: Restrictions to SIPP sample data (1986 - 1988, 1990, & 1991 panels)

	Women		Men	
Sample restrictions	Person-wave records	Unique individuals	Person-wave records	Unique individuals
All month four records, excluding the last interview of each panel*	601,181	111,481	554,742	104,768
Age 21 to 55 during all waves	279,016	51,814	259,573	49,541
Valid interview for all panel waves	214,349	34,350	188,594	30,249
Uniquely identifiable state all waves	205,233	32,879	179,913	28,851
At most one move between states	204,278	32,728	178,972	28,700
Not an SSI recipient	200,294	32,105	176,664	28,334
Job lock sample				
All household members have employer provided coverage	122,950	24,523	114,890	22,667
One job at end of wave and start of next	84,580	18,800	96,565	19,811
Observed in at least two waves	82,294	16,514	94,821	18,067
Positive panel weight	82,272	16,510	94,809	18,064
Job push sample				
Sample member does not have ECHI *or* the entire household has employer coverage from a household member besides the sample member	133,904	25,320	71,645	16,291
No job *or* one job at end of wave and start of next	126,745	25,028	67,399	16,022
Observed in at least two waves	124,703	22,986	64,690	13,313
Positive panel weight	124,673	22,981	64,664	13,309

*Also excludes ten records which did not have an assigned gender in the SIPP longitudinal files.

Table 1.7: Job lock sample: sample averages and standard deviations (in parentheses)
All averages presented as percentages (multiplied by 100), except where noted by *

	Women					Men		
		Medicaid eligible?		At least one household member Medicaid eligible?			At least one household member Medicaid eligible?	
Variable	All	Yes	No	Yes	No	All	Yes	No
Quit/retired	3.316	4.591	3.201	2.841	3.354	2.160	2.032	2.177
	(17.91)	(20.93)	(17.60)	(16.62)	(18.00)	(14.54)	(14.11)	(14.59)
Laid-off/fired	0.910	1.634	0.845	1.192	0.888	1.208	1.351	1.189
	(9.498)	(12.68)	(9.155)	(10.85)	(9.381)	(10.92)	(11.54)	(10.84)
ECHI	70.76	74.85	70.39	67.59	71.01	91.92	91.59	91.96
	(45.49)	(43.39)	(45.66)	(46.81)	(45.37)	(27.25)	(27.76)	(27.18)
Non-ECHI	46.96	30.59	48.44	46.60	46.99	18.37	15.46	18.75
	(49.91)	(46.08)	(49.98)	(49.89)	(49.91)	(38.73)	(36.15)	(39.03)
Medicaid eligible	8.274			33.07	6.283			
	(27.55)			(47.05)	(24.27)			
ECHI & non-ECHI	17.71	5.433	18.82	14.19	18.00	10.30	7.046	10.72
	(38.18)	(22.67)	(39.09)	(34.90)	(38.42)	(30.39)	(25.59)	(30.93)
ECHI & Medicaid eligible	6.193			24.60	4.715			
	(24.10)			(43.07)	(21.20)			
Non-ECHI & Medicaid eligible	2.531			10.17	1.917			
	(15.71)			(30.23)	(13.71)			
ECHI, non-ECHI, & Medicaid eligible	0.449			1.703	0.349			
	(6.689)			(12.94)	(5.896)			
Age*	36.20	31.13	36.66	38.98	35.98	36.81	37.38	36.74
	(8.441)	(6.167)	(8.468)	(8.801)	(8.371)	(8.412)	(8.687)	(8.373)
Earnings decile*	5.559	3.342	5.759	4.663	5.631	7.507	6.218	7.673
	(2.499)	(1.617)	(2.467)	(2.494)	(2.485)	(2.207)	(2.450)	(2.118)
Household income decile*	6.888	4.216	7.129	6.376	6.929	6.915	5.645	7.079
	(2.333)	(2.348)	(2.176)	(2.916)	(2.275)	(2.265)	(2.694)	(2.149)
Married	71.34	44.17	73.80	66.76	71.71	76.15	87.21	74.72
	(45.22)	(49.66)	(43.97)	(47.11)	(45.04)	(42.62)	(33.40)	(43.46)
Previously married	13.59	24.44	12.61	19.56	13.11	8.650	4.309	9.211
	(34.26)	(42.97)	(33.19)	(39.67)	(33.75)	(28.11)	(20.31)	(28.92)
Never married	15.07	31.39	13.60	13.68	15.18	15.20	8.484	16.07
	(35.77)	(46.41)	(34.28)	(34.36)	(35.88)	(35.90)	(27.87)	(36.72)
Less than HS	6.643	10.78	6.270	10.84	6.306	9.565	16.75	8.638
	(24.90)	(31.01)	(24.24)	(31.09)	(24.31)	(29.41)	(37.34)	(28.09)
High School	39.01	47.79	38.22	44.66	38.56	35.96	42.75	35.09
	(48.78)	(49.95)	(48.59)	(49.72)	(48.67)	(47.99)	(49.47)	(47.72)
Some college	26.13	28.70	25.89	24.80	26.23	22.83	20.58	23.12
	(43.93)	(45.24)	(43.81)	(43.19)	(43.99)	(41.97)	(40.43)	(42.16)
College	28.22	12.74	29.61	19.70	28.90	31.64	19.92	33.16

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Table 1.7 – continued from previous page

	(45.01)	(33.34)	(45.65)	(39.78)	(45.33)	(46.51)	(39.94)	(47.08)
Private job	76.90	82.69	76.37	75.02	77.05	82.43	84.64	82.14
	(42.15)	(37.83)	(42.48)	(43.30)	(42.05)	(38.06)	(36.05)	(38.30)
Public job	23.10	17.31	23.63	24.98	22.95	17.57	15.36	17.86
	(42.15)	(37.83)	(42.48)	(43.30)	(42.05)	(38.06)	(36.05)	(38.30)
Counts of household members								
All*	2.024	2.435	1.987	3.034	1.943	2.174	3.332	2.024
	(1.299)	(1.422)	(1.281)	(1.222)	(1.271)	(1.404)	(1.339)	(1.341)
Over 14 years*	1.231	1.156	1.238	2.127	1.160	1.259	1.904	1.175
	(0.894)	(1.040)	(0.880)	(1.204)	(0.823)	(0.889)	(1.134)	(0.815)
Under 15 years*	0.793	1.279	0.749	0.907	0.784	0.915	1.428	0.849
	(0.982)	(1.146)	(0.953)	(1.105)	(0.970)	(1.083)	(1.301)	(1.033)
ECHI*	0.824	1.145	0.795	1.278	0.787	1.639	2.566	1.519
	(1.229)	(1.438)	(1.204)	(1.522)	(1.195)	(1.550)	(1.691)	(1.489)
Non-ECHI*	1.467	1.383	1.475	2.020	1.423	0.784	0.979	0.759
	(1.410)	(1.648)	(1.386)	(1.745)	(1.370)	(1.188)	(1.592)	(1.123)
Medicaid eligible*	0.0960	0.480	0.0613	1.291		0.174	1.524	
	(0.383)	(0.878)	(0.276)	(0.657)		(0.597)	(1.031)	
ECHI & non-ECHI*	0.267	0.0927	0.282	0.264	0.267	0.249	0.214	0.254
	(0.783)	(0.521)	(0.800)	(0.849)	(0.777)	(0.725)	(0.750)	(0.722)
ECHI & Medicaid eligible*	0.0474	0.307	0.0240	0.638		0.140	1.223	
	(0.287)	(0.750)	(0.180)	(0.855)		(0.557)	(1.179)	
Non-ECHI & Medicaid eligible*	0.0551	0.184	0.0435	0.742		0.0442	0.386	
	(0.276)	(0.569)	(0.228)	(0.718)		(0.255)	(0.662)	
ECHI, non-ECHI, & Medicaid eligible*	0.00656	0.0112	0.00615	0.0883		0.00970	0.0849	
	(0.0942)	(0.135)	(0.0895)	(0.335)		(0.113)	(0.325)	
Observations	82,272	7,111	75,161	6,539	75,733	94,809	11,633	83,176

All estimates are weighted using panel weights adjusted as described in footnote 12. Average counts of household members exclude the analysis sample member (the person whose job mobility is modeled in the regressions). The categories for firm type and marital status have been combined to save space for presentation of descriptive statistics. Complete categories were used in the regressions.

Table 1.8: Job push sample: sample averages and standard deviations (in parentheses)
All averages presented as percentages (multiplied by 100), except where noted by *

	Women					Men		
		Medicaid eligible?		At least one household member Medicaid eligible?			At least one household member Medicaid eligible?	
Variable	All	Yes	No	Yes	No	All	Yes	No
Moved to ECHI	1.000	1.608	0.776	0.961	1.013	1.713	1.877	1.658
	(9.951)	(12.58)	(8.776)	(9.755)	(10.01)	(12.98)	(13.57)	(12.77)
ECHI	10.75	1.068	14.31	2.588	13.29	12.93	3.682	16.09
	(30.97)	(10.28)	(35.02)	(15.88)	(33.94)	(33.55)	(18.83)	(36.74)
Non-ECHI	72.74	39.07	85.14	41.37	82.50	60.32	38.91	67.63
	(44.53)	(48.79)	(35.57)	(49.25)	(38.00)	(48.92)	(48.76)	(46.79)
Medicaid eligible	26.92			67.82	14.20			
	(44.36)			(46.72)	(34.90)			
ECHI & non-ECHI	10.75	1.068	14.31	2.588	13.29	12.93	3.682	16.09
	(30.97)	(10.28)	(35.02)	(15.88)	(33.94)	(33.55)	(18.83)	(36.74)
ECHI & Medicaid eligible	0.288			0.372	0.261			
	(5.356)			(6.092)	(5.105)			
Non-ECHI & Medicaid eligible	10.52			18.66	7.988			
	(30.68)			(38.96)	(27.11)			
ECHI, non-ECHI, & MEdicaid eligible	0.288			0.372	0.261			
	(5.356)			(6.092)	(5.105)			
Age*	36.35	30.58	38.48	35.22	36.71	35.71	34.96	35.97
	(8.977)	(6.358)	(8.866)	(9.229)	(8.868)	(9.150)	(8.961)	(9.200)
Earnings decile*	4.129	2.240	4.503	3.087	4.314	5.588	4.143	5.979
	(2.489)	(1.172)	(2.510)	(2.111)	(2.505)	(2.727)	(2.325)	(2.696)
Household income decile*	5.688	3.119	6.634	3.609	6.335	5.501	3.804	6.080
	(2.940)	(2.366)	(2.537)	(2.912)	(2.632)	(2.951)	(2.689)	(2.810)
Married	77.22	45.06	89.07	54.59	84.26	65.30	70.56	63.50
	(41.94)	(49.76)	(31.20)	(49.79)	(36.42)	(47.60)	(45.58)	(48.14)
Previously married	11.16	22.09	7.137	23.42	7.351	9.317	7.824	9.827
	(31.49)	(41.49)	(25.74)	(42.35)	(26.10)	(29.07)	(26.86)	(29.77)
Never married	11.61	32.85	3.791	21.99	8.388	25.39	21.62	26.67
	(32.04)	(46.97)	(19.10)	(41.42)	(27.72)	(43.52)	(41.17)	(44.23)
Less than HS	17.73	28.76	13.67	32.52	13.13	20.09	32.08	16.00
	(38.19)	(45.26)	(34.35)	(46.85)	(33.77)	(40.07)	(46.68)	(36.66)
High School	42.46	41.74	42.72	42.18	42.54	36.23	37.34	35.85
	(49.43)	(49.31)	(49.47)	(49.38)	(49.44)	(48.07)	(48.37)	(47.96)
Some college	22.21	20.51	22.84	17.13	23.80	22.73	18.66	24.12
	(41.57)	(40.38)	(41.98)	(37.68)	(42.58)	(41.91)	(38.96)	(42.78)
College	17.60	8.986	20.77	8.173	20.53	20.96	11.92	24.04
	(38.08)	(28.60)	(40.57)	(27.40)	(40.40)	(40.70)	(32.40)	(42.73)
Private job	82.04	88.84	80.69	84.74	81.56	83.67	88.32	82.41

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Table 1.8 – continued from previous page

	(38.39)	(31.48)	(39.47)	(35.96)	(38.78)	(36.97)	(32.12)	(38.08)
Public job	17.96	11.16	19.31	15.26	18.44	16.33	11.68	17.59
	(38.39)	(31.48)	(39.47)	(35.96)	(38.78)	(36.97)	(32.12)	(38.08)
Counts of household members								
All*	2.630	2.973	2.503	3.365	2.401	2.390	3.423	2.037
	(1.447)	(1.699)	(1.320)	(1.658)	(1.292)	(1.558)	(1.685)	(1.341)
Over 14 years*	1.464	1.356	1.504	1.713	1.387	1.523	1.963	1.372
	(1.005)	(1.183)	(0.928)	(1.371)	(0.845)	(1.077)	(1.273)	(0.956)
Under 15 years*	1.165	1.617	0.999	1.652	1.014	0.867	1.460	0.665
	(1.207)	(1.390)	(1.085)	(1.412)	(1.092)	(1.128)	(1.354)	(0.960)
ECHI*	0.139	0.0150	0.185	0.0414	0.169	0.224	0.0815	0.273
	(0.610)	(0.224)	(0.695)	(0.372)	(0.664)	(0.765)	(0.524)	(0.826)
Non-ECHI*	1.909	1.325	2.125	1.435	2.057	1.560	1.399	1.615
	(1.513)	(1.671)	(1.390)	(1.758)	(1.396)	(1.450)	(1.710)	(1.346)
Medicaid eligible*	0.464	1.332	0.144	1.956		0.519	2.040	
	(1.045)	(1.524)	(0.509)	(1.298)		(1.144)	(1.428)	
ECHI & non-ECHI*	0.139	0.0150	0.185	0.0414	0.169	0.224	0.0815	0.273
	(0.610)	(0.224)	(0.695)	(0.372)	(0.664)	(0.765)	(0.524)	(0.826)
ECHI & Medicaid	0.00353	0.00210	0.00406	0.0149		0.00776	0.0305	
eligible*	(0.0696)	(0.0586)	(0.0732)	(0.142)		(0.104)	(0.205)	
Non-ECHI &	0.129	0.286	0.0718	0.546		0.156	0.614	
Medicaid eligible*	(0.492)	(0.789)	(0.299)	(0.891)		(0.552)	(0.957)	
ECHI, non-ECHI, &	0.00353	0.00210	0.00406	0.0149		0.00776	0.0305	
Medicaid eligible*	(0.0696)	(0.0586)	(0.0732)	(0.142)		(0.104)	(0.205)	
Observations	124,673	33,247	91,426	29,766	94,907	64,664	16,925	47,739

All estimates are weighted using panel weights adjusted as described in footnote 12. Average counts of household members exclude the analysis sample member (the person whose job mobility is modeled in the regressions). The categories for firm type and marital status have been combined to save space for presentation of descriptive statistics. Complete categories were used in the regressions.

Table 1.9: Job lock regression estimates for men (estimates multiplied by 100)

	Voluntary Job Exits			Involuntary Job Exits (Placebo Outcome)		
		Married Only			Married Only	
Coefficient – Variable	Full Sample (1)	(2)	Excludes Top Incomes (3)	Full Sample (4)	(5)	Excludes Top Incomes (6)
$\beta_1 - E_{it}$	-0.398	0.746	0.174	1.198**	0.983**	1.455**
	(0.80)	(0.83)	(1.04)	(0.47)	(0.49)	(0.60)
	[0.62]	[0.37]	[0.87]	[0.01]	[0.04]	[0.02]
$\beta_3 - E_{it}N_{it}$	-0.268	-0.394	-0.096	-0.749**	-0.755***	-0.614*
	(0.53)	(0.62)	(0.77)	(0.33)	(0.25)	(0.32)
	[0.62]	[0.53]	[0.90]	[0.02]	[0.00]	[0.06]
Counts of Household members						
$\beta_6 - \sum_j E_{itj}$	-0.231	-0.400	-0.245	0.141	0.125	0.099
	(0.24)	(0.27)	(0.35)	(0.19)	(0.20)	(0.21)
	[0.34]	[0.14]	[0.49]	[0.45]	[0.53]	[0.64]
$\beta_7 - \sum_j M_{itj}$	-0.517	-1.073*	-1.004	-0.266	-0.476	-0.597
	(0.42)	(0.58)	(0.66)	(0.34)	(0.40)	(0.36)
	[0.22]	[0.06]	[0.13]	[0.43]	[0.23]	[0.10]
$\beta_8 - \sum_j E_{itj}N_{itj}$	0.179	0.209	0.220	0.175	0.193*	0.101
	(0.21)	(0.22)	(0.28)	(0.13)	(0.11)	(0.14)
	[0.40]	[0.35]	[0.44]	[0.17]	[0.09]	[0.46]
$\beta_9 - \sum_j E_{itj}M_{itj}$ ($\hat{\lambda}_2$ estimator)	0.437	0.946*	0.878	-0.006	0.288	0.417
	(0.44)	(0.56)	(0.65)	(0.34)	(0.37)	(0.36)
	[0.32]	[0.09]	[0.17]	[0.99]	[0.44]	[0.25]
$\beta_{10} - \sum_j E_{itj}N_{itj}M_{itj}$ ($-\hat{\lambda}_1$ estimator)	-0.576**	-0.628**	-1.266***	-0.269	-0.299	-0.400
	(0.27)	(0.27)	(0.39)	(0.34)	(0.34)	(0.43)
	[0.03]	[0.02]	[0.00]	[0.43]	[0.38]	[0.35]
Observations	94,809	70,252	49,359	94,809	70,252	49,359

Estimates are weighted using panel weights adjusted as described in footnote 12. Two-way-clustered standard errors in parentheses, p-values in brackets. Statistical significance for two-sided t-tests indicated by * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$. Both cluster variables indicate state of residence, and can be different for movers. See Section 1.3.3 for details. Samples for Columns (2), (3), (5), and (6) include only those who were married every time they were observed. Columns (3) and (6) also exclude any observations for individuals with either job earnings or household income in the top decile. All regressions include person-level fixed effects and year-month dummies. The inclusion of state-by-year dummies results in very similar estimates, and so are not reported. Additional, unlisted control variables include dummies for: child age (indicators for a child under two, two through five, and six through 17, and these are fully interacted); education (less than high school, high school diploma, some college, and four years of college or more); marital status (married – spouse present, married – spouse absent, widowed, divorced, separated, never married); five-year-age group, meaning 21-24, 25-29, 30-34, and so on; job earnings and household earnings deciles; firm type (not working, private, non-profit, federal, state , or local government, armed forces, or unpaid family business or farm); and number of household members, with five or more treated as one group.

Table 1.10: Job lock regression estimates for women (estimates multiplied by 100)

	Voluntary Job Exits			Involuntary Job Exits (Placebo Outcome)		
		Married Only			Married Only	
Coefficient – Variable	Full Sample (1)	(2)	Excludes Top Incomes (3)	Full Sample (4)	(5)	Excludes Top Incomes (6)
$\beta_1 - E_{it}$	-1.538**	-0.603	-0.678	0.040	-0.354	-0.606
	(0.65)	(0.63)	(0.69)	(0.31)	(0.38)	(0.53)
	[0.02]	[0.34]	[0.33]	[0.90]	[0.36]	[0.26]
$\beta_2 - M_{it}$	1.626*	-0.571	-0.388	0.489	0.518	0.374
	(0.85)	(0.88)	(0.89)	(0.72)	(0.71)	(0.69)
	[0.05]	[0.52]	[0.66]	[0.50]	[0.47]	[0.59]
$\beta_3 - E_{it}N_{it}$	0.678	0.122	0.387	-0.221	0.013	0.162
	(0.52)	(0.43)	(0.37)	(0.23)	(0.30)	(0.41)
	[0.20]	[0.78]	[0.30]	[0.34]	[0.97]	[0.69]
$\beta_4 - E_{it}M_{it}$ ($\hat{\lambda}_2$ estimator)	-1.120	1.077	1.033	-0.223	-0.249	-0.076
	(0.93)	(1.15)	(1.12)	(0.75)	(0.88)	(0.91)
	[0.23]	[0.35]	[0.35]	[0.77]	[0.78]	[0.93]
$\beta_5 - E_{it}N_{it}M_{it}$ ($-\hat{\lambda}_1$ estimator)	0.336	0.249	0.418	-0.173	0.001	-0.097
	(1.09)	(1.27)	(1.36)	(0.65)	(0.76)	(0.76)
	[0.76]	[0.84]	[0.76]	[0.79]	[1.00]	[0.90]
Counts of Household members						
$\beta_6 - \sum_j E_{itj}$	0.630**	0.470*	0.570***	0.081	0.143	0.208
	(0.26)	(0.25)	(0.22)	(0.12)	(0.16)	(0.23)
	[0.01]	[0.06]	[0.01]	[0.51]	[0.37]	[0.36]
$\beta_7 - \sum_j M_{itj}$	-0.064	0.368	0.079	0.201	0.056	0.054
	(0.43)	(0.43)	(0.50)	(0.26)	(0.36)	(0.44)
	[0.88]	[0.39]	[0.87]	[0.44]	[0.88]	[0.90]
$\beta_8 - \sum_j E_{itj}N_{itj}$	-0.478*	-0.331	-0.495*	-0.172	-0.226	-0.288
	(0.24)	(0.23)	(0.29)	(0.16)	(0.19)	(0.27)
	[0.05]	[0.15]	[0.09]	[0.30]	[0.24]	[0.28]
$\beta_9 - \sum_j E_{itj}M_{itj}$ ($\hat{\lambda}_2$ estimator)	-0.430	-0.906	-0.486	-0.139	0.748	0.700
	(0.62)	(0.78)	(0.87)	(0.36)	(0.52)	(0.57)
	[0.49]	[0.25]	[0.58]	[0.70]	[0.15]	[0.22]
$\beta_{10} - \sum_j E_{itj}N_{itj}M_{itj}$ ($-\hat{\lambda}_1$ estimator)	0.383	0.384	0.021	0.283	-0.503	-0.093
	(0.58)	(0.73)	(0.72)	(0.38)	(0.46)	(0.53)
	[0.51]	[0.60]	[0.98]	[0.45]	[0.28]	[0.86]
Observations	82,272	55,453	44,483	82,272	55,453	44,483

Estimates are weighted using panel weights adjusted as described in footnote 12. Two-way-clustered standard errors in parentheses, p-values in brackets. Statistical significance for two-sided t-tests indicated by * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$. Both cluster variables indicate state of residence, and can be different for movers. See Section 1.3.3 for details. Samples for Columns (2), (3), (5), and (6) include only those who were married every time they were observed. Columns (3) and (6) also exclude any observations for individuals with either job earnings or household income in the top decile. All regressions include person-level fixed effects and year-month dummies. The inclusion of state-by-year dummies results in very similar estimates, and so are not reported. Additional, unlisted control variables include dummies for: child age (indicators for a child under two, two through five, and six through 17, and these are fully interacted); education (less than high school, high school diploma, some college, and four years of college or more); marital status (married – spouse present, married – spouse absent, widowed, divorced, separated, never married); five-year-age group, meaning 21-24, 25-29, 30-34, and so on; job earnings and household earnings deciles; firm type (not working, private, non-profit, federal, state , or local government, armed forces, or unpaid family business or farm); and number of household members, with five or more treated as one group.

Table 1.11: Job push regression estimates for men and women (estimates multiplied by 100)
Outcome Variable = Moves to ECHI Jobs

	Male Sample			Female Sample		
		Married Only			Married Only	
Coefficient – Variable	Full Sample (1)	(2)	Excludes Top Incomes (3)	Full Sample (4)	(5)	Excludes Top Incomes (6)
$\delta_1 - \tilde{E}_{it}$	4.136***	3.184***	3.346***	2.985**	0.224	-0.102
	(1.09)	(0.90)	(1.10)	(1.49)	(1.32)	(1.34)
	[0.00]	[0.00]	[0.00]	[0.05]	[0.87]	[0.94]
$\delta_2 - \tilde{M}_{it}$				-1.981	-3.142**	-3.429**
				(1.38)	(1.53)	(1.54)
				[0.15]	[0.04]	[0.03]
$\delta_3 - \tilde{E}_{it}N_{it}$	-1.045***	-1.437***	-1.373***	-0.736***	-0.304	-0.334
	(0.36)	(0.43)	(0.46)	(0.26)	(0.41)	(0.43)
	[0.00]	[0.00]	[0.00]	[0.00]	[0.45]	[0.44]
$\delta_4 - \tilde{E}_{it}\tilde{M}_{it}$ ($\hat{\pi}_2$ estimator)				1.611	3.118**	3.406**
				(1.51)	(1.44)	(1.43)
				[0.28]	[0.03]	[0.02]
$\delta_5 - \tilde{E}_{it}N_{it}\tilde{M}_{it}$ ($-\hat{\pi}_1$ estimator)				-0.225	-0.492	-0.480
				(0.34)	(0.31)	(0.31)
				[0.51]	[0.11]	[0.13]
Counts of Household members						
$\delta_6 - \sum_j \tilde{E}_{itj}$	0.245	0.228	0.317	-0.086	0.110	0.135
	(0.25)	(0.36)	(0.41)	(0.12)	(0.11)	(0.14)
	[0.33]	[0.53]	[0.44]	[0.47]	[0.32]	[0.32]
$\delta_7 - \sum_j \tilde{M}_{itj}$	-0.130	-0.254	-0.167	0.115	0.244	0.227
	(0.36)	(0.34)	(0.45)	(0.24)	(0.25)	(0.31)
	[0.72]	[0.45]	[0.71]	[0.63]	[0.33]	[0.47]
$\delta_8 - \sum_j \tilde{E}_{itj}N_{itj}$	-0.038	-0.060	-0.052	-0.063	0.083	0.072
	(0.18)	(0.25)	(0.27)	(0.11)	(0.11)	(0.12)
	[0.83]	[0.81]	[0.85]	[0.57]	[0.47]	[0.55]
$\delta_9 - \sum_j \tilde{E}_{itj}\tilde{M}_{itj}$ ($\hat{\pi}_2$ estimator)	0.274	0.494	0.398	-0.185	-0.302	-0.306
	(0.33)	(0.36)	(0.47)	(0.20)	(0.25)	(0.30)
	[0.41]	[0.17]	[0.40]	[0.36]	[0.22]	[0.31]
$\delta_{10} - \sum_j \tilde{E}_{itj}N_{itj}\tilde{M}_{itj}$ ($-\hat{\pi}_1$ estimator)	-0.447***	-0.276	-0.278	0.136	0.093	0.148
	(0.14)	(0.18)	(0.20)	(0.13)	(0.12)	(0.12)
	[0.00]	[0.12]	[0.16]	[0.29]	[0.45]	[0.20]
Observations	64,664	42,347	35,424	124,673	93,601	79,841

Estimates are weighted using panel weights adjusted as described in footnote 12. Two-way-clustered standard errors in parentheses, p-values in brackets. Statistical significance for two-sided t-tests indicated by * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$. Both cluster variables indicate state of residence, and can be different for movers. See Section 1.3.3 for details. Samples for Columns (2), (3), (5), and (6) include only those who were married every time they were observed. Columns (3) and (6) also exclude any observations for individuals with either job earnings or household income in the top decile. All regressions include person-level fixed effects and year-month dummies. The inclusion of state-by-year dummies results in very similar estimates, and so are not reported. Additional, unlisted control variables include dummies for: child age (indicators for a child under two, two through five, and six through 17, and these are fully interacted); education (less than high school, high school diploma, some college, and four years of college or more); marital status (married – spouse present, married – spouse absent, widowed, divorced, separated, never married); five-year-age group, meaning 21-24, 25-29, 30-34, and so on; job earnings and household earnings deciles; firm type (not working, private, non-profit, federal, state , or local government, armed forces, or unpaid family business or farm); and number of household members, with five or more treated as one group.

Table 1.12: Estimates using a state-level measure of eligibility (estimates multiplied by 100)

MALE SAMPLE

Job Lock – Voluntary Job Exits				Job Push – Moves to ECHI jobs			
		Married Only				Married Only	
Coefficient – Variable	Full Sample (1)	(2)	Excludes Top Incomes (3)	Coefficient – Variable	Full Sample (4)	(5)	Excludes Top Incomes (6)
β_9 $(\hat{\lambda}_2)$ – $\sum_j E_{itj} I_{itj}$	0.931	0.960	-0.163	δ_9 $(\hat{\pi}_2)$ – $\sum_j \tilde{E}_{itj} \tilde{I}_{itj}$	0.412	0.865	0.834
	(0.68)	(0.70)	(0.88)		(0.43)	(0.64)	(0.85)
	[0.17]	[0.17]	[0.85]		[0.34]	[0.18]	[0.33]
β_{10} $(-\hat{\lambda}_1)$ – $\sum_j E_{itj} N_{itj} I_{itj}$	-0.594	-1.330**	-1.746**	δ_{10} $(-\hat{\pi}_1)$ – $\sum_j \tilde{E}_{itj} N_{itj} \tilde{I}_{itj}$	-0.863***	-0.544	-0.497
	(0.58)	(0.56)	(0.72)		(0.33)	(0.39)	(0.44)
	[0.31]	[0.02]	[0.01]		[0.01]	[0.16]	[0.26]
Observations	94,809	70,252	49,359	Observations	64,664	42,347	35,424

FEMALE SAMPLE

Job Lock – Voluntary Job Exits				Job Push – Moves to ECHI jobs			
		Married Only				Married Only	
Coefficient – Variable	Full Sample (1)	(2)	Excludes Top Incomes (3)	Coefficient – Variable	Full Sample (4)	(5)	Excludes Top Incomes (6)
β_4 $(\hat{\lambda}_2)$ – $E_{it} I_{it}$	-2.945	3.634	4.174	δ_4 $(\hat{\pi}_2)$ – $\tilde{E}_{it} \tilde{I}_{it}$	-0.970	1.631	2.788*
	(1.82)	(2.79)	(2.88)		(1.55)	(1.60)	(1.61)
	[0.11]	[0.19]	[0.15]		[0.53]	[0.31]	[0.08]
β_5 $(-\hat{\lambda}_1)$ – $E_{it} N_{it} I_{it}$	2.179	0.152	1.120	δ_5 $(-\hat{\pi}_1)$ – $\tilde{E}_{it} N_{it} \tilde{I}_{it}$	0.189	-0.453	-0.398
	(1.54)	(2.09)	(1.94)		(0.69)	(0.59)	(0.62)
	[0.16]	[0.94]	[0.56]		[0.79]	[0.44]	[0.52]
β_9 $(\hat{\lambda}_2)$ – $\sum_j E_{itj} I_{itj}$	-0.270	-1.711	-1.336	δ_9 $(\hat{\pi}_2)$ – $\sum_j \tilde{E}_{itj} \tilde{I}_{itj}$	-0.154	-0.582*	-0.645*
	(1.04)	(1.31)	(1.44)		(0.32)	(0.32)	(0.39)
	[0.80]	[0.19]	[0.35]		[0.63]	[0.07]	[0.09]
β_{10} $(-\hat{\lambda}_1)$ – $\sum_j E_{itj} N_{itj} I_{itj}$	1.560	1.921*	1.574	δ_{10} $(-\hat{\pi}_1)$ – $\sum_j \tilde{E}_{itj} N_{itj} \tilde{I}_{itj}$	0.034	0.300	0.394
	(1.11)	(1.11)	(1.34)		(0.24)	(0.24)	(0.25)
	[0.16]	[0.08]	[0.24]		[0.89]	[0.22]	[0.11]
Observations	82,272	55,453	44,483	Observations	124,673	93,601	79,841

Two-way-clustered standard errors in parentheses, p-values in brackets. Statistical significance for two-sided t-tests indicated by * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$. All models are the same as in Tables 1.9, 1.10, and 1.11, except the Medicaid eligibility indicator variable has been replaced with a state-level measure of eligibility, I, as described in Section 1.5.3. See notes for Tables 1.9, 1.10, and 1.11 for model and standard error details.

Chapter 2

Does Regulation of Physicians Reduce Health Care Spending?

2.1 Introduction

This chapter investigates whether malpractice risk faced by physicians, which originates primarily through state-level licensing boards and the civil litigation system, induces changes in the overall cost of health care. Vocal segments of the physician and politician populations have long argued that fear of malpractice accusations drives up health care costs by inducing physicians to increase the amount of medical services they recommend. This argument, however, overlooks other possible responses to this type of legal risk. For example, increased use of preventative services could result in fewer costly illnesses. Alternatively, physicians could reduce their use of risky procedures. The net effect of either of these alternatives could be a reduction in spending, implying that the true impact of malpractice fears on spending is theoretically ambiguous. This point has been borne out in the empirical literature on the topic thus far, as results have been mixed.

My analysis introduces new evidence on the matter by exploiting a previously untapped measure of risk, the state-level frequency of sanctions taken against physicians by industry oversight groups, such as medical boards and hospitals. Also known as *adverse actions*, these sanctions are monitored by the National Practitioner Data Bank (NPDB), which also tracks malpractice payments made by (or on behalf of) physicians. Federal law mandates reporting of both adverse actions and malpractice payments to the NPDB, so this unique government database allows observation of the entire universe (or close to it) of these measures. The availability of the adverse action data offers the opportunity for insight on the regulatory influence of medical boards and industry groups, a source of risk to physicians that is of

particular interest because doctors do not insure against it. This is in contrast to the more often studied civil litigation risk, against which nearly all physicians carry insurance.

Combining the NPDB data with data from several other sources, I measure the effect of changes in the frequency of malpractice payments and adverse actions over a 16 year period on health care expenditures at the state level. I find evidence suggesting that increases in adverse actions against physicians are associated with lower health care expenditures. One additional action taken against a physician each year is estimated to decrease average state hospital expenditures by as much as 1.6 hundredths of a percent. For spending on prescription drugs, the reduction is as much as 1.8 hundredths of a percent. At the same time, I estimate that malpractice payments generally have much smaller, statistically insignificant effects, a result that is consistent with the argument that physician incentives for responding to malpractice litigation risk are muted due to insurance. To the extent that doctors do in fact respond to this type of risk, though, my results suggest that it also decreases spending since in most cases I estimate coefficients with negative signs.

2.2 The legal risk faced by physicians

Modern physicians in America face malpractice risk from both the court system and centralized regulators. Litigation arising under malpractice law, a subset of tort law, incentivizes physicians to provide high quality care via the threat of monetary penalties for injuries to patients caused by substandard care (Cooter and Ulen, 2012). Centralized oversight comes primarily from state-level medical licensing boards, as well as some professional societies, like the American Medical Association (AMA), and other entities such as hospitals. These organizations motivate physicians to maintain high quality via the threat of adverse actions. Adverse actions are actions taken by regulators such as medical boards as punishment against physicians for professional misconduct, such as incompetent care or breaking certain types of laws. Actions taken by state medical boards generally involve restrictions on the physician's license (e.g. probation, suspension, revocation), and actions taken by other organizations are similar conceptually though smaller in scope (e.g. a hospital may suspend clinical privileges). Although the civil litigation system and industry oversight groups are separate and have different histories and scopes[1], they do overlap: a malpractice allegation could result in a physician facing both a lawsuit and punishment from oversight groups.

The existence of both sources of malpractice risk stems from the recognition of two asymmetries between physicians and patients. First, only the patient bears the costs associated with

[1]Tort law is part of the broader societal legal system, but state medical licensing and the AMA developed in this country specifically in response to a long history of poor quality medical care provided by generally low skilled and poorly trained health care workers (Shryock, 1967).

receiving substandard health care, such as a delayed diagnosis or improperly dosed medication. Thus, if physicians, who make recommendations for levels of treatment, do so on the basis of their own, private incentives (that is, free of intervention by regulators) they may make recommendations that are different than would be preferred by their patients. Secondly, there is an information gap between physicians and patients, resulting in a difficulty on patients' part to know the true quality of health care they receive from their doctors. Therefore, if physicians do indeed make sub-optimal recommendations, patients may not recognize it and end up accepting lower levels of care than they would if they knew what the physician knew. By adding costs to physicians' objective functions, regulation and the legal system seek to more closely align physicians' incentives with those of patients. Thus, malpractice risk is intended, at least in theory, to protect the interests of the patient, and does so at the expense of physician.

A potential problem for this is that the realignment of physicians' incentives induced by legal risk may be too strong, resulting in physicians recommending levels of care that are too high – a phenomenon often referred to as *defensive medicine*. In fact, such is the predominant belief among physicians. For example, a national survey in 2009 of physicians from all specialties by Bishop et al. (2010) found that 91% of respondents agreed with the question: "Do Physicians Order More Tests and Procedures Than Patients Need to Protect Themselves From Malpractice Suits?" Another 2009 national survey of physicians performed by Sirovich et al. (2011), which focused on primary care physicians, found that 76% of respondents cited malpractice concerns as causing them to practice more aggressively (by testing and referring more frequently). Physicians in high-risk specialties have reported practicing defensively at rates above 90%, as well (Studdert et al., 2005). Supporting its constituency, the AMA has also been a vocal proponent of this view, beginning one of its medical liability policy documents as follows:

> The broken medical liability system remains one of the most vexing issues for physicians today. It places a wedge between physicians and their patients. It forces physicians to practice defensive medicine. (American Medical Association, 2012b, p. 3)

These views have led physicians to call for protection from legal liability in the practice of medicine. For example, the survey by Bishop et al. (2010) also found that 91% of physicians agreed with the statement: "Unnecessary use of diagnostic tests will not decrease without protections for physicians against unwarranted malpractice suits". Furthermore, formal policy of the AMA categorizes liability reform as its "highest legislative priority" (American Medical Association, 2012a).

Such calls have found a receptive audience in the public sector, likely because it is believed that defensive medicine adds significantly to national health care spending. For example,

a 2009 estimate of the effect of tort reforms prepared by the Congressional Budget Office (CBO) estimated that national health care spending would be reduced by "about 0.5 percent (about $11 billion in 2009)" if national reforms were enacted, and that the the federal budget deficits would be reduced by $54 billion over the decade from 2010 through 2019 (Elmendorf, 2009). Because of numbers such as these, support for malpractice law reform to limit malpractice risk for physicians at the national level has become common. For example, the House of Representatives passed a bill in March 2012 that would reform health care litigation nationally in a number of ways, including limits on noneconomic and punitive damages in health care lawsuits[2]. Though the idea of national reform is not yet popular enough to become law (as this bill has not yet been passed by the Senate), a number of states have been induced to take action by adopting reforms. For example, 26 states had adopted laws limiting malpractice damages (usually noneconomic damages, which does not limit payment of medical bills) as of 2006 (Mello, 2006a).

Despite all of this attention the topic of defensive medicine has received, there are two aspects that have received comparatively little discussion. The first is that defensive medicine could theoretically lead to either increased or decreased spending. This possibility was explicitly acknowledged by U.S. Congress, Office of Technology Assessment (1994), which defined positive defensive medicine as when physicians order extra medical services to avoid legal liability, and negative defensive medicine as when they avoid patients or services out of liability fears[3]. The second aspect is that it is actually not obvious why we should even expect doctors to respond to malpractice litigation risk in the first place, given that nearly all physicians are insured for it (Mello, 2006b). Malpractice insurance is unlike some other forms of insurance in that it is not experience rated, meaning the claims experiences of individual physicians do not affect the rates they pay for insurance. Instead, malpractice insurance is generally priced based on specialty and geography (Sloan, 1990; Fournier and McInnes, 2001; Mello, 2006b), so from the perspective of individual physicians, malpractice insurance premiums are fixed costs, because their own choices regarding services recommendations have negligible influence on their own insurance rates. Some authors (e.g. Kessler and McClellan, 1996) have suggested that the indirect or noneconomic costs associated with malpractice litigation, like reputation effects, stress, or time spent defending against the allegation, are strong motivators for physicians. This certainly could be the case, yet a physician facing an adverse action from the state medical board (for example) would face all these same costs *in addition* to potential economic consequences, since no insurance is available for adverse actions. Thus, if physicians do respond to malpractice litigation risk that they carry insurance for, it would then seem reasonable to expect that they would also respond to malpractice risk originating from regulators.

[2]Protecting Access to Healthcare Act, H.R. 5, 112th Congress (2012).

[3]Additionally, Currie and MacLeod (2008) develops a formal theoretical model to emphasize the ambiguity of defensive medicine outcomes.

2.3 Previous economics literature on defensive medicine

The previous economics literature on defensive medicine can be delineated by strategies used to measure variation in the malpractice risk levels that doctors face. Two approaches have been particularly common. The first has relied on variation in state-level malpractice laws, while the second used malpractice insurance premiums or payments variation. The path breaking paper in the first category was written by Kessler and McClellan (1996), who used a difference-in-differences framework to estimate the effect of reforms of state tort laws on hospital expenditures and health outcomes from 1985 to 1990. Their analysis focused on Medicare patients with two types of heart conditions and found that reforms that directly reduced the amount of malpractice awards (like caps on damages) were associated with statistically significant reductions in hospital expenditures of five and nine percent for the two conditions. They also found no evidence of an effect on health outcomes. Thus, their results were consistent with conventional views that spending is positively affected by malpractice risk, with no health benefits.

Currie and MacLeod (2008), however, argued that both negative and positive outcomes are possible, depending on the context of the procedure. Their empirical analysis, which used a state-level tort law identification strategy like Kessler and McClellan (1996), punctuated this point by estimating that damages caps increased the use of C-section procedures by about five percent (a statistically significant result). This result suggests a negative relationship between malpractice risk and spending for an important sub-population of medical patients, a conclusion in direct contrast to Kessler and McClellan (1996).

While both Kessler and McClellan (1996) and Currie and MacLeod (2008) employ credible empirical frameworks, their contrasting results underscore the narrow focus of their analyses. Broader measures of the effect of defensive medicine are therefore attractive because they allow for estimates of net effects (which are particularly of interest for policy makers). Examples of papers of this type that rely on state law variation for identification include White and Hage (2006) and Sloan and Shadle (2009). White and Hage modeled state-level, healthcare spending by Medicare and overall[4], while Sloan and Shadle used total, individual-level, Medicare spending following hospitalizations. Both papers generally found insignificant effects of direct reforms except in specific cases of more limited types of spending, in particular Medicare spending in hospitals (for White and Hage) and spending for heart conditions (for Sloan and Shadle, which was on the margin of statistical significance).

[4]The state-level spending data used by White and Hage was obtained from the Centers for Medicare and Medicaid Services, which was also the source for the data that used in my empirical analysis below. The series used in their paper was slightly different from the one I used in that it allocated spending on the basis of state of residence of the patient, whereas the spending measure used in my analysis measures spending by the location of the provider. Additionally, their panel has an earlier time frame than mine, though there is nine years of overlap between then two.

One of the drawbacks of the use of tort laws to measure physicians' perceived malpractice risk is that these laws are distant measures of such risk, in the sense that their effects are filtered through the general patient populations that make malpractice accusations, the civil litigation system which adjudicates such claims, and the insurance companies which make payments and charge premiums. Given this, it is not always clear that physicians' perceptions of risk changes resulting from law changes would be the same as an economist might think after the fact. This point was underscored by Kessler and McClellan's result for "indirect reforms" like abolishment of joint-and-several liability and caps on contingency fees. They found statistically significant *positive* effects on spending were associated with such reforms, a result that is seemingly the opposite of what was expected[5]. As later pointed out by White and Hage (2006) and Currie and MacLeod (2008), one of the important types of indirect reforms, joint-and-several liability, may actually raise the risk of litigation faced by physicians, not reduce it. Thus, Kessler and McClellan's result for indirect reforms is, in fact, consistent with the conventional defensive medicine theory. Nevertheless, the initial misunderstanding regarding joint-and-several liability illustrates the importance of measuring risk levels close to the level where it is perceived by subjects.

The second category of papers in the economics defensive medicine literature has sought closer measures of physician risk levels, using one or both of average malpractice premiums and payments. Authors using these measures have consistently found small or no statistically significant effects (Dubay et al., 1999, 2001; Baicker et al., 2007; Kim, 2007; Thomas et al., 2010). Each of these papers, however, faces concerns regarding reverse causality to some extent. Since the main purpose of monetary malpractice payments are to compensate injured patients for economic loss, it is likely that plaintiffs' medical bills are important determinants of the size of their malpractice payments[6]. Furthermore, since the price of malpractice insurance reflects, in part, the cost to insurers of making malpractice payments, insurance premiums also likely reflect plaintiff medical bills. Thus, there is a plausibly causal link between the prevailing prices and usage rates of medical care and the size of malpractice payments and malpractice insurance premiums, which could lead to biased coefficient estimates when using these variables as measures of malpractice risk.

In the context of this critique of malpractice premiums and malpractice payments measured in dollar amounts, Kim (2007) becomes a particularly interesting analysis because of his use of frequency counts of malpractice payments as one of his measures of malpractice risk. This measure does not directly reflect prevailing prices of medical procedures or the frequency of

[5]Similar results were also found in each of the other papers discussed above.

[6]To my knowledge, there is no direct evidence on the effect of medical bill size on the amount of a malpractice payment resulting from a settlement, but there is suggestive evidence presented by Sloan and Hsieh (1990), who show that payments made by insurers increase with the severity of injury. In the case of jury awards, Sloan and Hsieh also show that they are positively related to estimated economic losses. Similar evidence, though linking claimed losses specifically identified as for medical costs and jury awards, can also be found in Lakdawalla and Seabury (2012).

their use, and thus is not susceptible to the threat of reverse causality by trends in prices or utilization[7]. This reasoning underlies the use of frequency counts of malpractice payments instead of dollar amounts in my empirical analysis described below.[8]

Viewing all of the papers discussed above as a whole, they seem to suggest that there is some response to malpractice litigation risk, though the directions of the effects are often contradictory, and in many cases no response is found. Additionally, no one has yet studied the impact of the presence of regulators in this industry, despite their important role in the market. Thus, the existing evidence is not clear cut on the question of the effect of malpractice risk on the use of health care services or health care spending.

2.4 Empirical investigation

2.4.1 Econometric model

My empirical investigation follows the strain of literature studying broad measures of spending by modeling the influence of malpractice risk on state-level health care spending categories. In particular, I estimate linear panel data models of the form:

$$\ln(SPEND_{st}) = \alpha_s + \widetilde{\alpha}_s t + \beta_t + X'_{st}\gamma + NPDB'_{st}\delta + \epsilon_{st}. \tag{2.1}$$

Indexes s and t indicate the state and year, while $\ln(SPEND_{st})$ represents the natural log of state-level, real health care expenditures. Column vectors $NPDB_{st}$ and X_{st} contain measures of the malpractice risk environment and other control variables. State fixed-effects are represented by α_s and $\widetilde{\alpha}_s t$ indicates state-specific, linear time-trends (accounting for long-term, state-specific, growth-differences in spending). Year dummy variables are represented by β_t, while γ and δ are column vectors of parameters representing marginal effects on health spending. The X_{st} vector is comprised of the number of non-federally employed medical doctors in the state, the state population, and the number of state residents with private health insurance, Medicaid, and Medicare (all of which are expressed in thousands). Additionally, it contains the number of practicing lawyers in the state (also in thousands), which serves as a measure of the costliness of accessing the state legal system.

[7]This is not to say, however, that the use of frequency counts eliminates all possible sources of reverse causality. For example, high spending on medical care in a state might induce more malpractice allegations. The specific influence that prices and utilization have on malpractice payments because malpractice payments are related to medical bills, however, is addressed.

[8]For an alternative strategy to address this issue, see Lakdawalla and Seabury (2012), which offers a compelling argument that their identification strategy, which is based on jury awards for non-economic damages in malpractice cases, also avoids the bias implied by the influence of medical costs on malpractice payments.

The $NPDB_{st}$ vector is comprised of the count of physicians who received adverse actions and the number of physicians associated with malpractice payouts. These variables serve as proxies for the actual probabilities faced by physicians of adverse actions and malpractice payments, and are the primary variables of interest. I construct these variables using data from the NPDB, and so, for brevity, I will at times refer to these jointly as the "NPDB variables".

I estimate equation (2.1) for each of four categories of expenditures: hospital care, physician and clinical services, prescription drugs, and other non-durable medical products (the last category being a placebo category). All models are estimated using the within estimator, with state-level, cluster-robust standard errors to account for potential serial correlation. Note that since the dependent variable is in logarithms, the reported estimates for all coefficients are interpreted as percentage changes in spending due to one unit changes in the independent variables. All reported estimates, though, are multiplied by 100 for presentation purposes in the tables below.

2.4.2 Identification

One of the primary advantages of using the NPDB variables for this type of analysis is that they reflect input from the entire system that oversees physicians, including bureaucratic processes, behaviors of juries and judges, and the litigiousness of the population. As compared to a tort law identification strategy, the benefits of this can be seen in a simple example. Suppose a state enacts a damages cap for malpractice lawsuits, but that before the law was passed, there were few allegations of malpractice made in the state in the first place. Upon observing little change in spending, one might conclude that the damages cap had little effect on spending. This conclusion would be faulty, however, because what really happened was there was no real change in the risk level faced by physicians. There was little enforcement in the first place, so making enforcement even less likely had no effect. The NPDB variables, though, would reflect the minimal presence of regulation both before and after the law change, and therefore would more fully account for the risk levels in the state.

Despite this advantage of the NPDB variables, the use of these variables as measures of malpractice risk requires careful discussion of the basis for identification. Of first order concern is the threat of endogeneity, since the NPDB variables result from the interaction of the regulatory, litigation, and medical environments in a state. One step I took to address part of these concerns was to rely a frequency count of malpractice payments instead of a measure of the dollar value of payments or other dollar denominated measures like malpractice insurance premiums. This addresses the possibility of bias caused by trends in health care prices and utilization, which obviously play roles in determining health care spending, but that also contribute to the dollar value of malpractice payments and insurance premiums.

For malpractice payments, this is because payments are often set to cover some or all of the medical bills of the accusing party. In the case of malpractice insurance premiums, the link comes from the fact that most malpractice payments are actually made by insurers, and premiums are set to help cover the cost of making these payments.

Avoiding dollar based measures of risk only addresses a portion of the endogeneity concerns, though. Another threat is that the NPDB variables may not only reflect pure enforcement, but may also, to some extent, reflect the rate that malpractice occurs (or, inversely, the quality level of care) in the state. For example, it may be the case that when there is more malpractice, there are more accusations and stronger cases against physicians, resulting in more observed payments or adverse actions. Basing estimation on measures of regulatory risk that are contaminated in this way could possibly cause misleading results. To reduce the severity of this threat, I include state-level fixed effects in my regressions to account for time-invariant, unobserved heterogeneity at the state-level, including state-specific malpractice rates. Furthermore, I also include state-specific, linear time trends to account for time-varying heterogeneity that occurs in a linear fashion (state by year interactions are not possible in my state by year panel). These steps would still, however, leave the possibility of non-linear time variation in malpractice rates influencing my results. The nature of the NPDB variables, however, provides a solution to this issue. Each malpractice payment and adverse action is the end result of a legal process that begins in year t_1 with the original incident or injury of the accusing patient. The original incident is possibly a reflection of the malpractice rate since the patient may have received substandard care resulting in injury, but my analysis does not count the incident until the end of the legal process in year t_2, when a payment is made or an action taken against a physician by an oversight group. These measures are then paired via regression with outcome measures of spending for the corresponding year, t_2. Thus, the NPDB variables used in my analysis are implicitly lag variables: to the extent that they measure the malpractice rate in the state, they measure it as of $t_2 - t_1$ years before the measurement of the dependent variable in my regressions, state health care spending. In the case of malpractice payments, the NPDB data shows that $t_2 - t_1$ is usually quite a long time, as the median payment occurred four years after the incident from which it originated, with the tenth percentile being two years after, and the ninetieth percentile, eight years after. The NPDB does not have the information necessary to determine the time lag for adverse actions, but statistics posted to the website of the Medical Board of California (2010) indicate that the legal process for actions on a physician's license is also quite long, with the sum of the median number of days for the stages of the complaint process exceeding 600 days (and rising) since fiscal year 1999/2000. Category averages for each stage of the medical board legal process also exceed their medians, usually significantly so.

Given this lag aspect of the NPDB variables, the malpractice rate in a state could only influence my estimates if the malpractice rate in year t_1 has a direct influence on health

spending several years after the fact, in year t_2. Thus, the validity of my estimates relies on the assumption that malpractice rates of years past do not influence spending in the current year. This assumption can be argued to be reasonable because previous research has suggested that the effect of *actual* malpractice – as in injuries to patients due to medical errors – on health spending is minimal, regardless of the year. A paper from a large study of medical injuries in Utah and Colorado by Thomas et al. (1999)[9] estimated that "adverse events" – injuries caused by medical management, but not necessarily reflecting negligence – accounted for about 4% of national health expenditures in 1996. Only about half of these costs, though, were due to spending on health care (the rest was lost wages and home production). The amount of adverse events attributible to negeligence (i.e. malpractice) was estimated by Brennan et al. (1991)[10] and Thomas et al. (2000) to be on the order of 30%, bringing the overall estimate of the direct effect of malpractice on health spending to less than 1% of national health expenditures.

I have presented, therefore, several arguments suggesting that coefficient estimates based on the NPDB variables could be considered credible even if they partially reflect the malpractice rate of the state. These arguments, however, are only necessary in the first place if the NPDB variables do truly reflect the malpractice rate – a possibility that is *not* supported by previous research. In fact, two large studies of the connection between medical negligence (i.e. malpractice) in American hospitals and malpractice allegations – the first step of the relationship between actual malpractice and adverse actions or malpractice payments – have shown that the link between the two is quite weak. The first study, performed in New York hospitals by Localio et al. (1991), found that a malpractice claim followed an actual example of malpractice only 1.5% of the time. The second study, by Studdert et al. (2000) and performed in Colorado and Utah hospitals, found a similar estimate of 2.5%. Furthermore, both studies found that claims of malpractice were frequently made in cases where it was likely no malpractice occured (83% of claims in the New York sample and 78% in the Colorado and Utah sample). As a result, the authors of the Colorado and Utah study drew the following conclusions:

> "The poor correlation between medical negligence and malpractice claims that was present in New York in 1984 is also present in Utah and Colorado in 1992. Paradoxically, the incidence of negligent adverse events exceeds the incidence of malpractice claims but when a physician is sued, there is a high probability that it will be for rendering nonnegligent care." (p. 250)

[9]Thomas et al. (1999) was a part of a series of papers from the Utah and Colorado study, which also includes Studdert et al. (2000) and Thomas et al. (2000), both cited below.

[10]Brennan et al. (1991) was one of the papers generated by the "Harvard Medical Practice Study", a large study of medical injuries and malpractice in New York during the mid-1980s. This study also generated the paper by Localio et al. (1991), which I discuss below.

Another study, national in scope, by Studdert et al. (2006), focused on the relationship between claims and outcomes of malpractice litigation – the second step of the relationship between malpractice and adverse actions or malpractice payments. Using data on closed claims from medical liability insurers, the authors found that 27% of malpractice claims regarding incidents in which it was judged that malpractice did actually occur resulted in no payment at all being made to the claimant. Conversely, in cases where no malpractice occured, payment was still made 28% of the time. Thus, in cases where a claim is made, the wrong outcome – i.e. a payment made when it should not be or no payment made when it should be – is quite a common occurance. Overall, once a malpractice accusation has been made, a payment is far from certain. Studdert et al. calculated that 56% of malpractice claims resulted in a payment being made. Another study by Jena et al. (2011) which had a much larger sample size, however, estimated this rate to be much lower at less than 22%[11]. The current processes of accusations, negotiations, and legal mechanisms, therefore, imposes a strong filter between the actual occurrence of malpractice and the observation of an outcome in the NPDB. Incidents of actual malpractice rarely result in malpractice claims, and even when claims are made, payment is estimated to take place as little as 22% of the time. Furthermore, claims and payments are commonly associated with incidents in which no malpractice occured. These facts suggest that if the NPDB variables reflect the influence of the state's rate of malpractice, it is a very diluted effect.

2.4.3 Data

Equation (2.1) is estimated using a year-by-state (including Washington DC) panel data set that covers the years 1992 through 2008 and is compiled from several sources. Its cornerstone is the data field I use as the dependent variable in my regressions: health expenditures by the state of the provider, estimated by the Centers for Medicare and Medicaid Services (2011) as part of their National Health Expenditure Accounts program. The Consumer Price Index[12] was used to express these expenditures in 2011 dollars, thereby adjusting for inflation in the overall economy, though the spending estimates still reflect inflation that was particular to the health care industry. The expenditure data is categorized into ten different categories, four of which I use in my analysis: hospital care, physician and clinical services, prescription drugs, and other non-durable medical products. Hospital care, accounting for 37% of all expenditures, is the largest and most interesting category since the highest risk and most costly medical care takes place in hospitals, making them likely settings for incidents that

[11] One of the main drawbacks of the work of Jena et al. (2011), though, is that it relies on records of only one insurer. The insurer was a large one that insured physicians in all states.

[12] More precisely, I used the Consumer Price Index for All Urban Consumers: All Items, with the index value for June taken as the value for the whole year (Bureau of Labor Statistics, 2012).

result in malpractice payments or adverse actions[13]. Physician and clinical services is the second largest category at 25% of spending, and, broadly speaking, covers services provided in outpatient settings. The third largest category I use, at 10% of the total, is prescription drugs, which tallies prescription drug sales in retail settings (drugs administered in hospitals or physician offices and included in hospital or office revenue, are included in the hospital care or physician and clinical services categories). The last category I use in my empirical study is other non-durable medical products, which I make use of for a placebo analysis. Though this group is much smaller than the other categories at 2.5% of costs, it is unique in that spending in this category is for retail purchases of medical products that do not require prescriptions or other physician input for purchase. In fact, purchases in this category are almost entirely paid for out of pocket (Medicare pays a small portion) since they are at the consumer's discretion. Examples of products in this category include over-the-counter drugs like cough and allergy medicines and medical sundries such as bandages and thermometors. Legal risks faced by physicians, therefore, would be expected to have no influence on this category of spending[14].

The NPDB variables are calculated using the Public Use Data File of the NPDB, a project overseen by the U.S. Department of Health and Human Services (Health Resources and Services Administration, Bureau of Health Professions, 2011b). By law, the NPDB collects information on all adverse actions taken against and malpractice payments made by (or on behalf of) physicians in the country[15]. Data collection by the NPDB began in fall of 1991, but collection of some types of adverse actions did not begin until the start of 1992, which is the first year of data I use in my analysis. I use the NPDB to count the number of unique physicians associated with malpractice payments and adverse actions by state and year. This includes payments associated with both judgments and settlements, and all types of adverse actions, without restriction with regard to the reason given for why the payment was made or adverse action taken (even if it was not necessarily related to medical competence)[16]. However, two important restrictions are made in order to avoid double counting. In the case of malpractice payments, I do not include payments identified as made by "State Funds",

[13]In fact, a recent study of the NPDB by Bishop et al. (2011), using data no longer available to the public, found that roughly half of all malpractice payments originated in a hospital setting, despite that there are almost 30 times more outpatient visits than hospital discharges.

[14]See Centers for Medicare and Medicaid Services (2010) for detailed discussion of the categoriztion of the National Health Expenditure Accounts.

[15]The National Practitioner Data Bank was established during the presidency of Ronald Reagan by Title IV of Public Law 99-660 (known as the Health Care Quality Improvement Act of 1986), with final regulations found in 45 CFR Part 60 (Health Resources and Services Administration, Bureau of Health Professions, 2001).

[16]Two reasons underlie my reasoning for not eliminating any observations based on the reason given in the NPDB for the malpractice payment or adverse action. First, many records do not have any reason given for the adverse actions or malpractice payments – 29% in the case of adverse actions, and 23% for malpractice payment records. Secondly, and more importantly, it is likely that all actions convey information about the regulatory regime regardless of the basis for the action (e.g. what types of actions lead to adverse actions or payments, and how costly the punishment or payment could be).

since, according the the NPDB documentation[17], these payments are usually made only when a payment has also been made by an insurer. In the case of adverse actions, I do not count actions that are identified as modifying previous actions[18].

The NPDB collects several types of adverse actions, including state licensure actions, exclusions from Medicare and Medicaid, and more localized actions such as hospital clinical privileges restrictions. Licensure and Medicare/Medicaid actions represent more than 83% of the records, indicating that the bulk of adverse actions reflect the actions of centralized regulators. Malpractice payments include both those made after trials and settlements. The inclusion of settlements is key to the representativeness of the NPDB since settlements make up a huge portion of the malpractice risk faced by physicians. Judgments and jury awards, in fact, represent less than 3% of the records in my sample of the NPDB. Thanks to this aspect of the NPDB and the public availability of portions of its data, numerous researchers have made use of it before (for example Baicker and Chandra, 2005; Baicker et al., 2007; Bishop et al., 2011).

Despite the above, the NPDB is not completely free of problems, and as pointed out by United States General Accounting Office (2000), it does suffer from issues with tardiness of reporting and possible under-reporting, particularly in the case of malpractice payments and clinical restrictions (as distinct from licensure actions, which may be reported late to some degree, but are generally thought to be complete). One important possible source of under-reporting was identified by the GAO as the "corporate shield", a loophole in which malpractice by a physician may go unreported if the physician is not identified as a party on the final settlement (on which only a hospital or other corporation is identified as responsible), even if the physician had initially been identified in the malpractice accusation. Because of the lack of data available on this type of situation, it is not known to what extent this "loophole" might be obscuring the malpractice picture reflected in the NPDB. However, to some degree, the situation identified by the GAO may not represent under-reporting but instead be an example of the difficulty of categorizing settlements as malpractice or not malpractice, or of indentifying the true liable party. For example, suppose an individual sues a hospital, a medical device manufacturer, and a physician after being injured during a procedure in which a defective instrument was used. During the course of the legal pro-

[17]From the p. 41 of the NPDB documentation: "Nine States – Florida, Indiana, Kansas, Louisiana, Nebraska, New Mexico, Pennsylvania, South Carolina, and Wisconsin – have or had State funds which make malpractice payments in addition to the payment made by a practitioner's primary malpractice insurance carrier if the total amount of the settlement or judgment is more than a maximum amount set in State law for payments by a primary insurance carrier. If such payments are made, there are in most cases two reports to the NPDB, one from the primary malpractice insurance carrier and one from the State fund, for a single malpractice incident."

[18]More precisely, I drop adverse action records classified (via any of the variables AACLASS1 through AACLASS5) with any of the following codes: 1280, 1282, 1283, 1285, 1295, 1296, 1297, 1480, 1482, 1496, 1497, 1515, 1680, 1681, 1682, 1689, 1690, 1695, 1696, 1699, 1780, 1781, 1789, 1790, 1795, 1796, 1799, 3281, 3296, or 3297.

cess, evidence may show that the physician was not aware of the faulty instrument and is dropped from the allegation before a settlement is reached by the injured individual and the manufacturer or hospital. In this case, the GAO's critique would suggest that the physician had benefited from a loophole when she should have been reported to the NPDB, despite that the facts of the situation suggest there was no fault on her part. Thus, there is reason to think that the corporate shield may not be as serious an issue as suggested by the GAO. One recent paper by Jena et al. (2011), in fact, supports the notion that the NPDB is representative, since they compared the size and frequency of malpractice payments as reflected in the NPDB over the period 1991 to 2005 to data they obtained from a large, national malpractice liability insurer and found that differences between the two sources were small. Thus, despite the critiques of the GAO, the NPDB remains the best available data on malpractice and adverse actions.

Besides the NPDB variables, I use figures from the American Bar Association (2012, 2009) for the number of active attorneys in the state. Additionally, I rely on several sources of data for proxies for the production cost of medical services. State level population figures are obtained from Population Division, U.S. Census Bureau (2002, 2009). The number of active medical doctors (excluding those employed by the federal government) comes from the Area Resource File (Health Resources and Services Administration, Bureau of Health Professions, 2011a). Frequency counts for the number of state residents with private insurance, Medicare, or Medicaid, were tabulated by the author from IPUMS CPS data for march of each year (Miriam King et al., 2010).

Summary statistics for all the variables used in my analysis can be found in Tables 2.1 through 2.3. Table 2.1 presents statistics on the categories of spending used as dependent variables in terms of millions of (real) dollars. Table 2.2 presents the same statistics for the natural log of these spending categories, which is the actual form of the variables that are used in my estimations. Table 2.3 reports statistics for the right hand side variables in my regressions. Note that in this table, the statistics for lawyers and non-federally employed physicians are presented in their true levels, as opposed to their use in my regressions, where the values are in thousands.

2.4.4 Results

Table 2.5 presents the results of my estimation of equation (2.1) on my sample of state health expenditures. Each category of health spending has estimates for each of two versions of (2.1), one with coincidently timed control variables and one in which all controls are all lagged one period (that is, $NPDB_{s,t-1}$ and $X_{s,t-1}$ replace their period t versions in equation (2.1)). The lag version of the model is intended as a check for reverse causality between health care spending and the coincidently timed control variables (especially the non-NPDB

control variables, since they do not share the lag feature of the NPDB variables discussed in section 2.4.2).

Focusing first on the hospital care spending category, column (1) presents the model with coincident independent variables. I estimate that, on average, one additional physician suffering an adverse action in a year results in a reduction of state hospital expenditures by 1.3 hundredths of a percent, a result that is statistically significant at the 1% significance level. Relative to the average annual state-level spending on hospital care over my sample period (approximately $11.5 billion), this estimate represents a decrease of about $1.5 million. For a change equal to the average, year-to-year, within-state change of 16, this estimate represents a reduction of almost 21 hundredths of a percent, or nearly $24 million. In contrast to this result, though, the coefficient for malpractice payments is not statistically significant and is quite close to zero. In fact, the estimate for malpractice payments is more than 225 times smaller than the adverse actions estimate. Given the conventional wisdom in the medical field that fear of malpractice lawsuits is an important determinant of cost, this result is surprising. That said, it is consistent with the view that broad use of malpractice insurance leaves little reason for physicians to alter their behavior out of fear of litigation.

Column (2) presents my estimates for the hospital care regression using lagged values of the right hand side variables. Here, the pattern is nearly identical to column (1), with adverse actions statistically significantly estimated (at the 1% significance level) to reduce spending by 1.6 hundredths of a percent, a decrease of approximately $1.8 million (for a change equal to the average, year-to-year change: decrease of 25.0 hundredths or almost $29 million). Malpractice payments are estimated to have a negligible effect that is insignificant at conventional levels. Even the upper bound of the 95% confidence interval for the malpractice payments coefficient is only 40% of the magnitude of the estimate for adverse actions. One interesting point to take away from these regressions is that, even though the effect of adverse actions is small relative to the size of hospital care spending in a state, it is large relative to the estimate for the population effect, with estimates for adverse actions in both versions of the model more than doubling the estimates for increasing state population by a thousand residents.

Turning to my estimates for spending on physician and clinical services in columns (3) and (4), I find that adverse actions are again estimated to reduce spending. The estimate for the coincident model is -0.7 hundredths and is not significant, though the lagged model is -1.2 hundredths and is significant at the 10% level. These coefficient estimates correspond to dollar valued marginal effects of -$9 million and -$15 million, respectively, for an average-level increase in adverse actions and when evaluated at the category's sample average annual state-level spending of $7.8 billion. Although the point estimates for this category are somewhat smaller than the estimates for the hospital care category, an important reason for the change in significance between categories is that these estimates are less precise, with standard

errors increasing by a third and a half for the coincident and lagged models, respectively. Comparison with the population effect point estimates for this category still suggests that these estimates have meaningful size, since one additional physician suffering an adverse action results in a change in spending of approximately the same magnitude as increasing the state population by a thousand.

The estimates for malpractice payments for the physician and clinical services category are notably different from the estimates for the other two categories. The coefficient estimate of -0.8 hundredths in the coincidental controls model is statistically significant at the 5% significance level, a decrease of 0.20% or nearly $16 million for an increase of 26 malpractice payments, the average, year-to-year, within-state change in malpractice payments. The lagged model estimate is similar in magnitude at -0.5 basis points (-$0.4 million), though in this case it is not statistically significant. These estimates are considerably larger (in percentage terms) than those for the hospital care and prescription drug categories, and the coincident model result is the only case where I find a statistically significant change in response to malpractice payments. As I will discuss below, however, the regressions I run which include leads for the NPDB variables suggest that this outcome may be influenced by endogeneity bias, which could help explain the incosistency of these estimates with the other categories.

The next set of regression estimates, for the prescription drug category, can be found in columns (5) and (6) of Table 2.5. The pattern of estimates in this case is quite similar to that of the hospital care category. The model with coincident regressors estimates a statistically insignificant 1.1 hundredths of a percent reduction due to adverse actions. For an average-level change, this corresponds to a reduction of 17 hundredths or $5.5 million (relative to the spending category sample average of $3.2 billion). The lagged model estimates this same coefficient as -1.8 hundredths (-0.29% or -$9.3 million for average-level change), which is significant at the 5% level. These point estimates are nominally close to the estimates from the hospital care category, and, also like the hospital care category, are large relative to the effect of population on spending. In both models in this category, the adverse action estimate is more than four times larger than the population estimate. Thus, the differences in statistical significance between the hospital care category and this one seem to be primarily driven by a drop in the precision of the estimates (standard errors are more than 60% larger than the hospital care category).

Focusing next on the malpractice payment estimates, I again find that the coefficients are negative, though neither case is statistically significantly different from zero. The estimated reductions of 0.2 and 0.4 hundredths of a percent in columns (5) and (6), respectively, both correspond to drops of approximately $2 to $3 million in category spending for average-level increases of 26 payments. As in the case of the hospital care category, the magnitudes of these estimates are much smaller (at about a quarter in size) than those for the adverse action

estimates. This is again consistent with the reasoning that the availability of malpractice insurance reduces physician responsiveness to the likelihood of malpractice payments[19].

Placebo analysis

Regression results for a final spending category, other non-durable medical products, are presented in columns (7) and (8) of Table 2.5. These results serve as a placebo analysis, since we should not expect to observe an effect of physician risk avoidance behavior on this type of health care spending. If there is no endogeneity bias, we would expect to estimate coefficients for adverse actions and malpractice payments equal to zero. On the other hand, if endogeneity is causing bias (and spurious statistically significant estimates in my main regressions) then we would expect to see significant, non-zero estimates when the placebo category is the dependent variable as well[20].

This analysis is a credible check of endogeneity to the extent that the dependent variables in my main regressions and the placebo regressions share the same unobserved factors that may bias coefficient estimates. Figures 2.1 through 2.3 show that average state spending on other non-durable medical products shares a similar time trend with the other spending categories. Furthermore, state-by-year correlations between other non-durable medical products and the other categories are high: 0.94, 0.97, and 0.87 for hospital care, physician and clinical services, and prescription drugs, respectively (0.17, 0.19, and 0.33 for first-differences).

Despite these correlations between other non-durable medical products and the other categories, the effects of the NPDB variables observed in the spending categories discussed above does not appear to carry over. As columns (7) and (8) show, neither model estimates statistically significant effects for these variables, casting doubt on the possible influence of unobserved factors. Additionally, unlike the estimates for the other categories, which were generally negatively signed for the NPDB variable coefficients, the estimates in this case are positive, suggesting that if there are unobserved factors, they would seem to be working in the opposite direction of my results.

[19]The prescription drug category models have noticeably larger estimates for the effect of the physician population on spending. While it is hard to be sure why this differs so much from the other categories, there has been previous research documenting a link between physician geographical concentration and increased sale of prescription drugs, e.g. Bruckner et al. (2012).

[20]Examples of previous uses of placebo analyses can be found in Hamersma and Kim (2009) and Bitler and Carpenter (2012).

Checking for endogeneity of the regulatory environment

Panel A of Table 2.6 presents results for the estimation of a variation of equation (2.1) which includes one-period-forward leads for the adverse action and malpractice payments variables (in addition to the coincidently timed versions of those variables). This is intended to address the possibility that the malpractice risk environment itself is a response to conditions in the state. If the NPDB variables are *not* endogenous, then one would expect that their lead values would estimate as insignificantly different from zero, and estimates of the coincident values of the NPDB variables would be similar to when the models are estimated without leads[21].

The results for the hospital care and prescription drug spending categories (columns (1) and (3)) have a similar pattern. The coefficient estimates for the coincident version of the adverse actions variable are both statistically significant (1% level for hospital care and 10% level for prescription drugs) and have magnitudes that are similar to my main estimates in Table 2.5. Additionally, the estimates for the adverse actions lead are not significant and are small in comparison to the coincident version estimates. For hospital care, the lead variable estimate is 34% of the coincident estimate, while for prescription drugs that ratio is 14%. For malpractice payments, neither the coincident nor lead versions have statistically significant estimates in these categories. The estimates are also comparatively small relative to the adverse action estimates, a finding that is consistent with the results of Table 2.5. For the placebo category, other non-durable medical products, the addition of lead variables does not lead to any interpretive changes. All estimates for this category are statistically insignificant, and magnitudes are similar Table 2.5 estimates. All of these facts undercut the possibility that the results for these categories found in Table 2.5 are driven by an endogenous response of the malpractice risk environment to health care spending or other conditions in a state.

The physician and clinical services category, however, has a different story. As is immediately noticeable, the lead variable estimate for malpractice payments is statistically significant (1% level) and comparatively large at approximately -1 hundredth of a percent. Moreover, the coincident version is not significant and has a much smaller magnitude than the estimates found in Table 2.5. Additionally, the point estimate for the lead version of the adverse actions is about 50% larger in terms of magnitude than the coincident version (though neither version of the adverse actions variable has a significant coefficient). These results suggest that there might be some remaining issues with endogeneity in this spending category that my models are not completely eliminating. In the case of malpractice payments, this would seem to explain the statistically significant estimate in Table 2.5, a result that at first glance seemed out of place due to its dissimilarity with the other categories.

[21]Two notable previous uses of this type of strategy to address endogeneity can be found in Gruber and Hanratty (1995) and Friedberg (1998).

The effect of big states

To check whether the effects estimated in Table 2.5 are being driven by the large states, Panel B of Table 2.6 presents results for the re-estimation of (2.1) after excluding the largest states in my panel. The excluded states were California, New York, Texas, Florida, Illinois, Pennsylvania, and Ohio, all of which had average populations over the period of my sample of more than 10 million. Each state also exceeded $66 billion in average annual health care spending.

The pattern of point estimates is similar to that of Table 2.5. The most notable magnitude differences are that the adverse action estimate for hospital care spending is about 40% smaller than it was in Table 2.5, while the malpractice payments estimates for physician and clinical services and prescription drugs increased by more than 40% and 300%, respectively. The estimate for malpractice payments in the placebo category, other non durable medical products, also increased in magnitude, rising to 0.9 hundredths, though its adverse action estimate fell to less than a third of the Table 2.5 estimate. The dropping of observations, though, also resulted in a much lower level of precision for the estimates, so none of the estimates are statistically significant at conventional levels. Nevertheless, the pattern of results seem to suggest that health care spending effects of regulation are not drastically different between the smaller and larger states.

2.5 Conclusion

I have presented evidence suggesting that regulation of physicians, as measured by frequency counts of adverse actions by oversight groups, decreases overall average health care spending at the state-level. In particular, I find that rising frequencies of adverse actions against physicians are associated with statistically significant spending decreases in the hospital care and prescription drug categories. My estimates for effects of increasing frequencies of malpractice payments are smaller in comparison to the adverse action estimates and, except for one case, are all statistically insignificant at conventional levels. The one significant estimate indicated a negative effect on spending in the physician and clinical services category, though there is some evidence that there might be endogeneity issues influencing the estimates for this category. These results are particularly interesting in light of the fact that physicians are insured against malpractice litigation risk, but are not insured against adverse actions such as a medical license suspension. Since impediments to their free ability to practice are very costly to physicians, it is not hard to believe that they would be sensitive to changes in the adverse action threat levels. Additionally, finding that malpractice risk emanating from centralized regulators associated with decreases spending is notable given that the medical and political communities have long argued that such risk (malpractice litigation, in particular)

leads to higher health care costs.

There are several mechanisms through which greater fear of punishment or litigation for physicians could result in lower spending levels. Doctors could increase their use of preventative services, leading to fewer cases of more serious (and expensive) illnesses. Alternatively, increased regulation could induce physicians to take greater precaution when providing services, leading to fewer errors that require costly additional care. Yet another possibility is doctors may reduce their use of risky services that have low marginal value to the health of the patient (or, more cynically, reduce their use of risky procedures regardless of the value to the health of the patient). The true explanation could even be a combination of these mechanisms and others. This analysis, though, makes no attempt to distinguish between mechanisms, and given that previous research has been mixed on the issue of how varying levels of risk influence physician behavior, there is room for additional research on this point.

Table 2.1: Dependent variable summary statistics, by expenditure category
(Millions of June 2011 dollars)

	Mean	Std. Dev.	Minimum	Maximum
Hospital care	11,523	12,556	620	75,931
Physician & clinical services	7,818	9,671	297	67,725
Prescription drugs	3,159	3,745	123	24,588
Other non-durable medical products	795	925	49	5,044
All four categories combined	23,296	26,435	1,093	172,212
Observations	867			

Table 2.2: Log-scale dependent variable summary statistics, by expenditure category
(Log of millions of June 2011 dollars)

	Mean	Std. Dev.	Minimum	Maximum
Hospital care	8.8556	1.0191	6.4301	11.2376
Physician & clinical services	8.3948	1.0933	5.6934	11.1232
Prescription drugs	7.4512	1.1617	4.8112	10.1100
Other non-durable medical products	6.1145	1.1074	3.8851	8.5259
All four categories combined	30.8160	4.2931	20.9071	40.8882
Observations	867			

Table 2.3: Independent variable summary statistics

	Mean	Std. Dev.	Minimum	Maximum
Physicians suffering adverse actions	66	77	0	632
Physicians with malpractice payments	240	338	10	1,838
Practicing lawyers	19,512	26,124	1,093	150,542
Non-Federally Employed Physicians (M.D.s)	13,654	16,454	640	95,906
Population ('000s)	5,515	6,130	466	36,580
Privately insured residents ('000s)	3,870	4,041	324	23,382
Medicare residents ('000s)	737	780	25	4,240
Medicaid residents ('000s)	652	880	31	6,398
Observations	867			

Table 2.4: Additional summary statistics: absolute values of year-to-year changes

	Mean	Std. Dev.	Minimum	Maximum
Physicians suffering adverse actions	16	29	0	344
Physicians with malpractice payments	26	43	0	410
Observations	816			

Table 2.5: Regression results by health care spending category
Dependent variable is the log of category health care spending
All estimates are multiplied by 10,000 (i.e. they are expressed in hundredths of a percent)

	Hospital Care		Physician & Clinical Services		Prescription Drugs		Other Non-Durable Medical Products (Placebo Category)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Timing of regressors (relative to dependent variable)	Coincident	Lagged	Coincident	Lagged	Coincident	Lagged	Coincident	Lagged
Physicians suffering adverse actions	-1.295***	-1.561***	-0.715	-1.202*	-1.083	-1.840**	0.889	0.778
	(0.461)	(0.463)	(0.615)	(0.702)	(0.756)	(0.746)	(0.939)	(0.920)
Physicians with malpractice payments	0.00574	0.0195	-0.779**	-0.528	-0.249	-0.392	0.142	0.0747
	(0.303)	(0.305)	(0.384)	(0.434)	(0.618)	(0.558)	(0.929)	(0.808)
Practicing lawyers ('000s)	-6.617	1.343	-12.51	-10.23	30.13***	17.81**	27.19***	27.70***
	(6.089)	(6.685)	(9.915)	(9.346)	(9.906)	(7.926)	(8.937)	(8.344)
Non-federally employed physicians ('000s)	77.32	42.96	8.317	16.39	277.0***	224.6***	-14.38	-4.511
	(59.33)	(55.19)	(59.63)	(63.96)	(87.70)	(74.99)	(85.61)	(89.19)
Population ('000s)	0.600*	0.565**	-0.873	-1.049	0.267	0.417	0.00400	-0.603
	(0.324)	(0.274)	(0.874)	(0.855)	(0.864)	(0.731)	(0.880)	(0.744)
Privately insured residents ('000s)	-0.0330	0.0920	-0.0355	0.0700	0.241*	0.277*	0.331	0.273
	(0.0990)	(0.0903)	(0.134)	(0.164)	(0.131)	(0.149)	(0.210)	(0.214)
Medicare insured residents ('000s)	0.0745	0.0145	-0.262	-0.184	0.0743	0.343	0.0955	-0.122
	(0.207)	(0.255)	(0.346)	(0.314)	(0.362)	(0.436)	(0.469)	(0.525)
Medicaid insured residents ('000s)	0.193	-0.0461	0.370*	0.544***	0.293	-0.109	0.101	0.208
	(0.132)	(0.148)	(0.203)	(0.197)	(0.234)	(0.251)	(0.284)	(0.278)
Observations	867	816	867	816	867	816	867	816
Adjusted R^2	0.978	0.979	0.961	0.959	0.991	0.990	0.837	0.830

Notes: Standard errors (clustered at state-level) in parentheses. Conventional levels of statistical significance (for two-tailed tests) indicated as * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. All regressions also included year and state fixed effects and state-specific, linear time-trends that are not reported.

Table 2.6: Robustness checks of regression results
Dependent variable is the log of category health care spending
All estimates are multiplied by 10,000 (i.e. they are expressed in hundredths of a percent)

	Panel A: Including Leads				Panel B: Excluding Big States			
	Hospital Care (1)	Physician & Clinical Services (2)	Prescription Drugs (3)	Other Non-Durable Medical Products (Placebo Category) (4)	Hospital Care (5)	Physician & Clinical Services (6)	Prescription Drugs (7)	Other Non-Durable Medical Products (Placebo Category) (8)
Physicians suffering adverse actions	-1.107**	-0.398	-1.190*	0.576	-0.761	-0.867	-1.781	-0.265
	(0.431)	(0.582)	(0.693)	(0.806)	(0.838)	(1.253)	(1.161)	(1.764)
Physicians with malpractice payments	0.102	-0.356	-0.285	0.0307	0.164	-1.096	-0.795	0.935
	(0.239)	(0.296)	(0.474)	(0.621)	(0.459)	(0.846)	(0.674)	(0.707)
Physicians suffering adverse actions (Lead variable)	-0.377	-0.607	-0.169	0.594	--	--	--	--
	(0.413)	(0.657)	(0.882)	(0.753)				
Physicians with malpractice payments (Lead variable)	-0.195	-0.968***	0.253	0.210	--	--	--	--
	(0.305)	(0.280)	(0.537)	(0.738)				
Observations	816	816	816	816	748	748	748	748
Adjusted R^2	0.977	0.960	0.991	0.856	0.979	0.960	0.991	0.842

Notes: Standard errors (clustered at state-level) in parentheses. Conventional levels of statistical significance (for two-tailed tests) indicated as * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Regressions also included all other control variables, fixed effects, and time-trends that were included in the models reported in Table 2.5. Regressions in Panel B excluded records for California, New York, Texas, Florida, Illinois, Pennsylvania, and Ohio, which all had average populations over my sample period greater than 10 million. These seven states also had the highest average annual health care spending, each exceeding $66 billion.

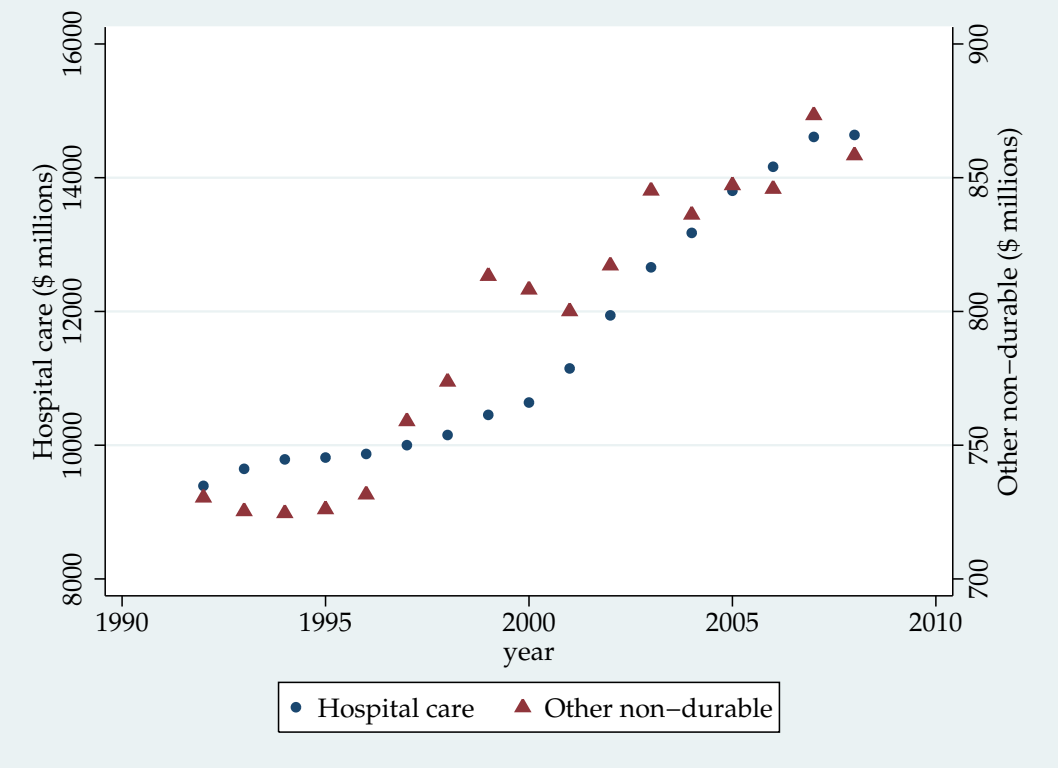

Figure 2.1: Comparison of average state expenditures on hospital care and other non-durable medical products over time

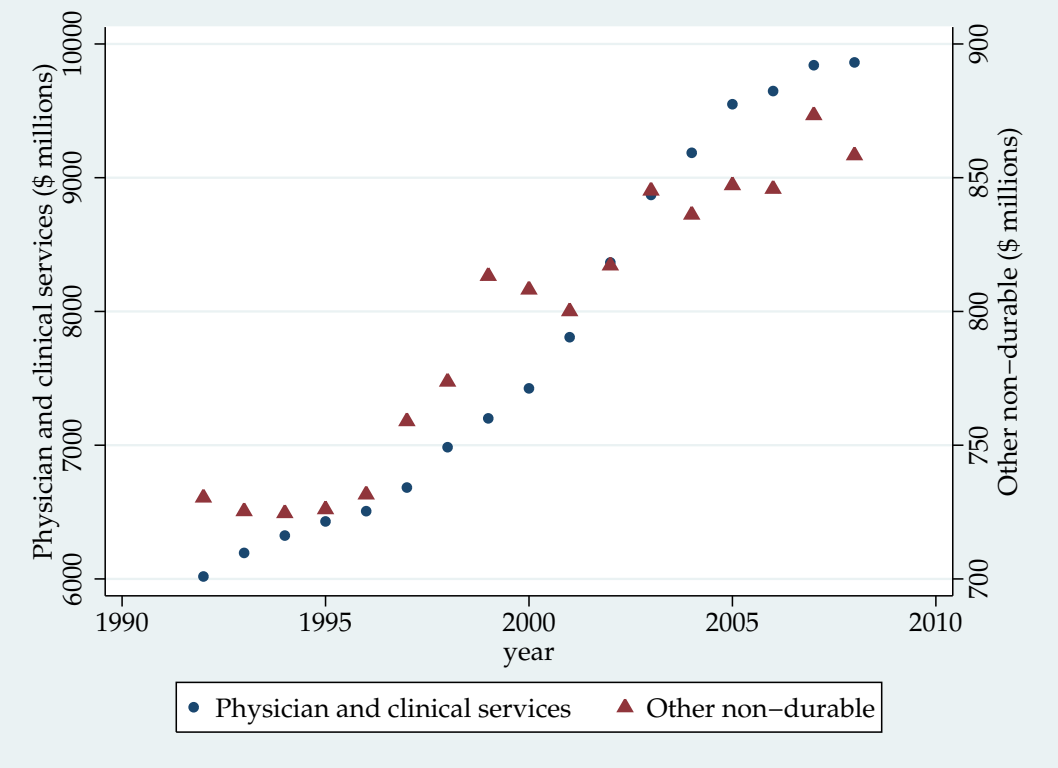

Figure 2.2: Comparison of average state expenditures on physician and clinical services and other non-durable medical products over time

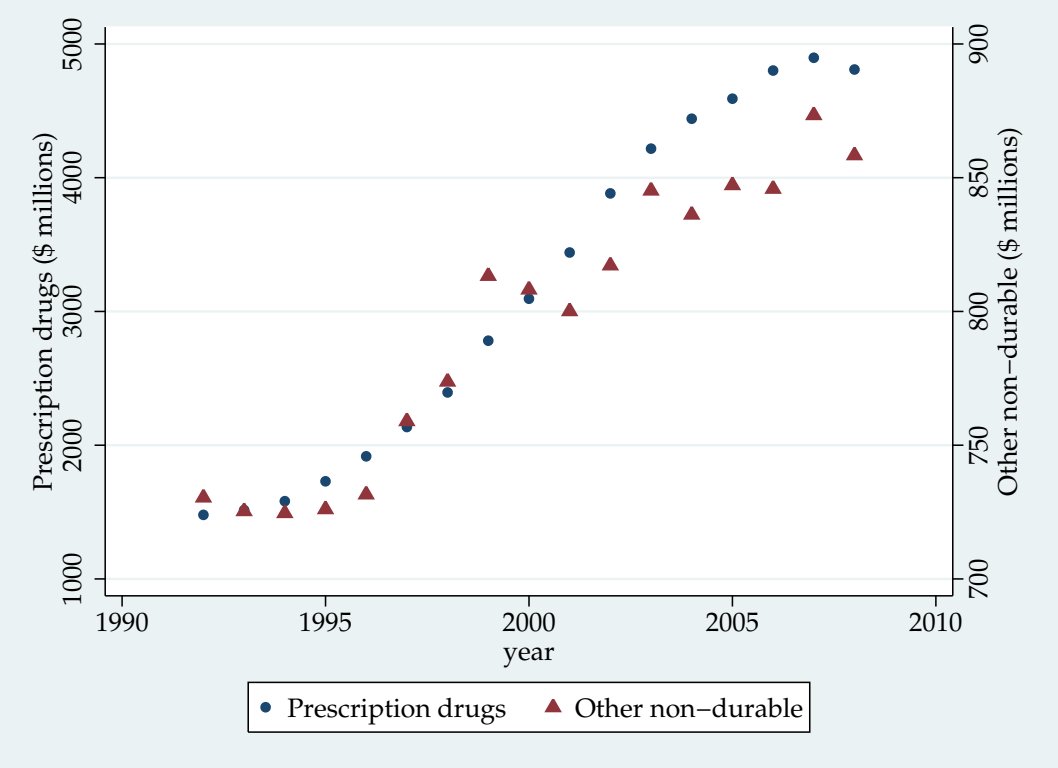

Figure 2.3: Comparison of average state expenditures on prescription drugs and other non-durable medical products over time

Chapter 3

Preliminary Results On The Effect of Specialist Cost Information on Primary Care Physician Referral Patterns

3.1 Introduction

Economists have long argued that cost information plays an important role in the efficient allocation of resources (e.g. Hayek, 1945), but this argument relies on the assumption that cost or price information is available to decision makers at the time of their decision. In the American health care system, this assumption is quite often not true. In the course of everyday practice, physicians often make decisions regarding drugs to prescribe, tests to order, and referrals to make in the near total absence of the comparative costs of the options available. This phenomenon has to some extent been documented in the medical literature (e.g. Shulkin, 1988; Tierney et al., 1990; Reichert et al., 2000; Feldman et al., 2013), and also anecdotally to me personally by physicians of my acquaintance.

In this chapter, I examine the importance of cost information in the physician referral process. In modern health care provision, physicians are highly specialized workers who are often loosely organized in teams when a patient's medical care requires input from physicians in more than one area of specialized knowledge. Such a team is typically led by a primary care physician (PCP), who refers the patient to specialists with relevant expertise (Starfield, 1998). Since specialists may not all sell their services at the same rate, the patient's health care will only be cost efficient if the referral is made to the specialist who provides the

required service (at the required level of quality) at the lowest price. In practice, however, the PCP usually does not have access to (and so cannot consider) information on the relative cost of available specialists, and so referrals are likely made inefficiently with respect to cost.

To investigate these issues, I partnered with a local group of physician practices – an Independent Practice Association ("the IPA") – to perform a field experiment testing whether providing information on the costliness of specialist physicians to PCPs alters their referral behavior. After first randomly assigning the IPA's primary care practices to treatment or control groups, I provided the treatment group of PCPs a list of average costs for several ophthalmology practices that were part of the IPA. After allowing a short, one-month period to pass for observation, data collected by the IPA was used to compare changes between the treatment and control group referral rates to the ophthalmologists of interest. Since the analysis is based on such a short period, the results reported herein are preliminary. The experiment is ongoing, and future analyses will be performed on longer post-periods and reported in a more complete article on the project.

Analysis of the currently available data has so far not found any statistically significant effects of cost information on PCP referral patterns. However, point estimates are relatively large when compared to sample averages of referral share variables. Additionally, coefficient estimate patterns are suggestive that physicians are applying the appropriate cost information when effects are broken down by patient types, even though they are not statistically significant.

This project was reviewed by the UCI Human Research Protections staff, and it was confirmed that this project does not qualify as human subjects research since no identifying information was available to UCI researchers as part of the project.

3.2 Background

The IPA is an association of otherwise independent medical practices that exists primarily to make the process of contracting with health insurance companies easier. It consists of approximately 150 PCPs and 350 specialists, and provides services to roughly eighty thousand patient members on behalf of their insurance companies. More than 91% of the IPA's claims are generated by patients with HMO insurance coverage, with the remainder being mainly covered under Point-of-Service plans. The experiment herein restricts its attention to the HMO coverage patients of the IPA. These patients are split into two types: about three-quarters are standard, non-Medicare HMO patients (which I call "HMO patients"), while the remaining quarter is comprised of Medicare Advantage HMO patients (which I refer to as "SrHMO patients"). Despite that standard HMO patients outnumber them by

approximately three-to-one, SrHMO patients are responsible for more than 45% of all HMO coverage claims.

When a physician practice joins the IPA, it agrees to accept the reimbursement rates for services the practice provides for the patients who are members of the IPA. These rates are those that the IPA has separately negotiated with patient insurance companies.[1] In the case of HMO patients, there is usually a set of the most common services provided by the practices which they agree to provide at a flat rate (per patient, per month). In the language of the medical field, these services are "capitated". Thus, when a PCP refers a patient to a specialist for a service that is capitated, the amount paid to the specialist is fixed, regardless of how intensively the specialist treats the patient. However, there is often also a set of services that are not capitated (even for HMO patients). These services are paid on a per-service basis, called fee-for-service (FFS) in the medical field. The payments received by physician practices for FFS services are true marginal costs for these services for the IPA. The services which are paid via capitation versus FFS vary by specialty and patient plan type. For example, for Ophthalmology, HMO patients are all paid on a FFS basis, but for SrHMO patients, about three-quarters of services are capitated. Thus for Ophthalmology, increases in treatment intensity directly result in increased costs for the IPA for either patient type. In the case of SrHMO patients, though, the IPA's exposure is limited due to the capitation arrangement.

As part of their association with the IPA, the PCPs hold financial stakes in the cost efficiency of the IPA. Bonuses paid to the PCPs are based in part on the financial results of the IPA, so if PCPs reduce the cost of their patients' care by (for example) referring to less expensive specialists, then they could see larger bonuses. Thus, there is a financial incentive for the PCPs to refer to cost effective specialists. However, according to IPA management, PCP awareness of this fact is not high. PCP bonuses are also affected by several types of care quality measures which are the primary focus of the PCPs' attention. To the extent that the PCPs do perceive a financial incentive that affects their referral choices, though, this incentive would be thought to be stronger for services that are paid on a FFS basis than those that are capitated.

3.3 Experiment Description

The full experimental plan follows a variation of the standard treatment-control, before-after design, in which the control group also receives that treatment after a period when

[1] A practice could directly contract with the insurance companies on its own, but partnership with the IPA allows the practice to "outsource" that responsibility. Negotiation as a group of practices instead of individually also provides bargaining advantages.

only the treatment group had access to it. This paper, however, reports preliminary results for a portion of the experiment when only the treatment group had received the treatment. Hence, in this preliminary draft of the analysis, the traditional experimental protocol is followed.

The subjects of the experiment were the IPA PCPs, who practiced in either the Family Practice or Internal Medicine specialties. The experimental treatment received by members of the treatment group was a report listing six busy Ophthalmology practices, along with two numbers for each practice: risk-adjusted, 180-day cost averages for newly referred patients to Ophthalmology for both standard HMO patients and SrHMO patients separately. Along with the cost report, the treatment group PCPs also received a cover letter from the CEO of the IPA, briefly explaining the reason for receiving the report and a description of how the costs were calculated. Anonymous facsimiles of the report and cover letter are attached as Figures 3.1 and 3.2.

Ophthalmology was chosen to be the specialty for which cost information was distributed to PCPs for three main reasons. First, Ophthalmology as a specialty receives a large number of referrals from the IPA's PCPs. During the twelve month period from March 2012 through February 2013, the Ophthalmology specialty received 3,467 referrals from Family Practitioners and 2,461 from Internists. For both types of PCPs, these were enough to make Ophthalmology their fourth most often referred to specialty in the IPA. Second, before this experiment, the IPA PCPs had never previously received any information about ophthalmologist costs, allowing for the measurement of the effect of completely new information. Third, as the medical specialty of physicians who treat and study diseases and functions of the eye, Ophthalmology is a particularly highly specialized area of medicine, and PCPs typically cannot substitute their own services, or the services of other specialists, for those of ophthalmologists. Ideally, the introduction of cost information to the PCPs would not affect the likelihood of a referral to the specialty of interest. That is, for a given patient, the probability he will be referred to the specialty of interest will be the same both before and after the introduction of cost information. Since Ophthalmology is so specialized, it seems likely that the only margin of response available to PCPs would be to which ophthalmologist the referral is made, *not* whether or not to refer to Ophthalmology at all.

All of the IPA's active PCPs at the time of the treatment distribution were included as subjects in the experiment if they satisfied minimal criteria: they must have had at least ten HMO claims during each calendar month from August 2013 through January 2014, and made at least one HMO patient referral to Ophthalmology during that period (total, not each month). It should be noted that for all purposes in this experiment, only adult HMO referrals are counted, and self-referrals and referrals to physicians in the same specialty as the referring physician are not counted. Besides excluding those PCPs that did not satisfy these criteria, one additional practice of three PCPs who were social contacts of me personally

were excluded. In the end, a total of 93 Internists and Family Practitioners were included in the experiment, with their assignments into treatment or control groups being determined via computer generated random numbers. Since the PCPs were organized into 55 separate practices, randomization took place at the practice level, so that either all physicians in a practice were assigned to the treatment group, or none were. This feature of the experimental procedure was intended to minimize control group contamination via discussion between PCPs, since if the subjects were going to discuss the specialist cost information, it seems likely that it would take place within the practice. It should be noted, though, that when I refer to a PCP "practice", I am referring to the office address of the PCPs, not a legal entity. So a PCP practice consists of one or more PCPs with the same office address listed with the IPA.

During the experiment, the IPA collected data on PCP referrals as part of its normal operations. This data was generated by the physicians' activities of seeing and treating patients as part of their usual medical practices in their regular offices. The IPA regularly collects all of this data, and all of the physicians are aware of this data collection. However, none of the physicians were made aware of the fact that the distribution of the cost information was related to an experiment or that an outside researcher was involved. Thus, interpretation of the PCPs behavior observed in the experiment is plausibly not obscured by the so-called "Hawthorne Effect", since no unusual observation was apparent to the subjects.

For this preliminary analysis, the experiment consisted of pre- and post-periods. The pre-period took place during the six months from November 2013 through April 2014. The post-period consists of the month from May 15th through June 15th, 2014. The ophthalmologist cost reports were mailed by the IPA to the treatment group subjects via regular U.S. Postal Service mail on May 5th, 2014. Referrals from the period starting May 1st, 2014 through May 14th, 2014 are excluded so that the pre-period could be based on six complete calendar months and to allow time for the reports to be delivered and then opened and read by the PCPs. The short post-period in this analysis is a weakness since one month may not be long enough for the treatment to have a detectable effect. Follow-up versions of this paper will address this issue by analyzing longer time periods after treatment.

Regular mail was used to distribute the treatment for two main reasons. First and most importantly was that IPA management and I wanted to implement a real-world type method that would be feasible for the IPA or other groups to replicate outside of an experimental setting. An important aspect of this is that this method put little burden on IPA staff. Secondly, in its normal operations IPA management often used mail for communication with its physicians, especially for important information, so sending the treatment in this manner made it less likely to seem unusual to the physicians or to suggest outside involvement. The use of regular mail, though, had an important weakness in that it was not possible to observe whether the PCPs actually received and read the cost reports. If, in the end, no effect on

referrals were observed, then it would not be possible to differentiate between a situation where physicians did not respond to the cost information and one where they somehow did not actually receive the information. Nevertheless, it was judged that the advantages of distribution via mail outweighed this disadvantage.

Another important issue regarding the design of the treatment is whether the PCPs actually understood and internalized the cost information (conditional on reading it). Given the many demands on the time of the PCPs, it seemed to be a real danger that if the information was not easily read and comprehended it would be ignored or discarded. In an effort to reduce the chance of this outcome, the cost report was designed to be as simple as possible. The information for only one specialty was presented, and the number of specialists practices included (six) was limited to only the busiest. The number of averages presented for each specialist was also limited to the minimum of two, and the explanation given on the reports was minimized as well. Still, since I could not observe PCP comprehension, my analysis implicitly assumes comprehension in the event that a PCP read the cost report. This assumption is a weakness that should be kept in mind when interpreting the results of this experiment.

3.4 Econometric Model

The primary outcome of interest for this experiment is the share of a PCP practice's Ophthalmology referrals that an Ophthalmology practice receives. Letting $p \in \{1, 2, ..., P\}$ and $s \in \{1, 2, ..., S\}$ index the PCP and specialist practices, respectively, I notate the referral share using $\theta_{ps} \equiv REFS_{ps}/TOTREFS_p$, where $REFS_{ps}$ is the number of Ophthalmology referrals between p and s, and $TOTREFS_p$ is the total Ophthalmology referrals made by p. The use of the referral share helps make the dependent variable more comparable between practices that may have different numbers of physicians and patients.

The basis for the econometric model is the notion that the costliness of the specialist can influence θ, so in its simplest form, a model would have θ as a function of C, the cost of the services of the specialist. Making the assumption that C enters in log form, such a model would look like the following (suppressing the constant):

$$\theta_{ps} = \beta \ln(C_s) + u_{ps}. \tag{3.1}$$

As a consequence of modeling the referral share θ as a function of the natural log of C, the coefficient of interest is interpreted as the change in referral share due to percentage changes in C_s: $\beta = d\theta_{ps}/(dC_s/C_s)$. For example, a ten percent change in the average cost for an ophthalmologist implies a change in referral share of $\beta/10$.

Equation 3.1 also allows for something to be said about the relevant elasticity for those PCP-specialist pairs that have positive referrals (a general elasticity is not possible since elasticities are not defined for pairs without any referrals). Restricting the outcome variable θ_{ps} to positive values only, and taking the ratio of the coefficient with θ_{ps}:

$$\begin{aligned}\frac{\beta}{\theta_{ps}} &= \frac{d\theta_{ps}}{dC_s} \times \frac{C_s}{\theta ps} \\ &= \frac{\left(\frac{d(REFS_{ps})}{TOTREFS_p}\right)}{dC_s} \times \frac{C_s}{\left(\frac{REFS_{ps}}{TOTREFS_p}\right)} \\ &= \frac{d(REFS_{ps})}{dC_s} \times \frac{C_s}{REFS_{ps}} \equiv \epsilon_{ps}\end{aligned}$$

This implies a one percent change an ophthalmologist's average cost is associated with a (β/θ_{ps}) percent change in the number of referrals the specialist receives from the PCP. Following the traditional methods of describing elasticities in economics, ϵ_{ps} would be called a "specialist cost elasticity of PCP referrals". It should be noted that this interpretation relies on the assumption that $TOTREFS_p$ is fixed, meaning that changes in θ_{ps} come only from changes in $REFS_{ps}$. This seems feasible given the highly specialized nature of Ophthalmology. If a PCP has a patient in need of ophthalmologic services, he or she does not have alternatives beyond referring to some ophthalmologist.

Given that ϵ_{ps} is specific to PCP-specialist practice pairs, to say something more generally about all pairs, one could look at the expected value,

$$\epsilon \equiv E[\epsilon_{ps}|\theta_{ps} > 0] = E\left[\frac{\beta}{\theta_{ps}}\middle|\theta_{ps} > 0\right].$$

Using Jensen's Inequality, this average elasticity can be bounded from below by

$$\epsilon > \frac{\beta}{E[\theta_{ps}|\theta_{ps} > 0]}.^2$$

This lower bound of the average elasticity for positive referral pairs can be consistently estimated by substituting the coefficient estimate $\hat{\beta}$ and the sample average of the positive values of θ_{ps}.

Adapting this simple model for use in the experiment requires first differentiating time periods. Only one month of post-period data is available for this preliminary analysis, so I measure the outcome variables in monthly intervals, and index them chronologically by $t \in \{1, 2, ...7\}$. Since the experiment follows a traditional before-and-after, treatment-and-control design, a straightforward modification of equation 3.1 to accommodate this is to nest it within a difference-in-differences (DD) framework. I indicate the post-period by A_t, where

$A_t = 1$ if $t = 7$, but $A_t = 0$ otherwise. Similarly, $I_p = 1$ identifies PCP practices that receive the information treatment, and $I_p = 0$ those that do not. The DD version of the model would then look like the following:

$$\begin{aligned}\theta_{pst} = {} & \beta_1 \ln(C_s) A_t I_p + \beta_2 A_t I_p + \beta_3 \ln(C_s) A_t + \beta_4 \ln(C_s) I_p \\ & + \beta_5 \ln(C_s) + \beta_6 A_t + \beta_7 I_p + u_{pst}.\end{aligned} \tag{3.2}$$

The primary coefficient of interest in equation 3.2 is β_1, the DD version of the effect of cost on referral share.[3]

Since the cost report that the PCPs actually received listed average costs for two separate types of patients, those on the standard HMO and those that were part of the Medicare Advantage HMO, the actual model implemented in my regressions incorporates both cost measures. Using C_s^{HMO} to indicate the average cost for Ophthalmology practice s for standard HMO patients, and C_s^{SrHMO} to indicate the cost for Medicare Advantage HMO patients, the full empirical model takes the following form:

$$\begin{aligned}\theta_{pst} = {} & \beta_1 \ln(C_s^{HMO}) A_t I_p + \beta_2 \ln(C_s^{SrHMO}) A_t I_p + \beta_3 A_t I_p \\ & + \beta_4 \ln(C_s^{HMO}) A_t + \beta_5 \ln(C_s^{SrHMO}) A_t + \beta_6 \ln(C_s^{HMO}) I_p + \beta_7 \ln(C_s^{SrHMO}) I_p \\ & + \beta_8 \ln(C_s^{HMO}) + \beta_9 \ln(C_s^{SrHMO}) + \alpha_p + \gamma_t + u_{pst}.\end{aligned} \tag{3.3}$$

Here β_1 and β_2 are interpreted as the separate effects for HMO and SrHMO patients. Additionally, this version of the model replaces the I_p main effect with PCP practice fixed effects, indicated by α_p. Since the cost report did not feature every Ophthalmologist associated with the IPA, only the busiest six Ophthalmology practices, the PCP practice fixed effects help compensate for differences between PCP practices in the proportion of total Ophthalmology referrals they allocate to the Ophthalmology practices on the cost report. This version also replaces the A_t main effect with month dummies, indicated by γ_t, allowing for the capture of PCP wide time trends in the allocation of referrals between those on the cost report and those that are not.

[3]The partial derivative of equation 3.2 produces $\partial\theta_{pst} / (\partial C_s / C_s) = \beta_1 A_t \times I_p + \beta_2 A_t + \beta_3 I_p + \beta_5$. Taking the double difference,

$$\begin{aligned}& [\partial\theta_{pst} / (\partial C_s / C_s) \,|I_p = 1, A_t = 1] - [\partial\theta_{pst} / (\partial C_s / C_s) \,|I_p = 0, A_t = 1] \\ & - ([\partial\theta_{pst} / (\partial C_s / C_s) \,|I_p = 1, A_t = 0] - [\partial\theta_{pst} / (\partial C_s / C_s) \,|I_p = 0, A_t = 0]),\end{aligned}$$

yields β_1.

3.5 Experiment Results

3.5.1 Randomization

Stratified randomization was used to assign PCP practices to treatment or control groups. Practices were stratified on the basis of five pre-period dummy variables: whether the practice was an Internal Medicine practice; whether the per-physician count of SrHMO referrals exceeded the pre-period median of all the PCP practices, and the same for HMO referrals; and whether the per-physician count of SrHMO claims exceeded the pre-period median of all the PCP practices, and the same for HMO claims. These practice-level measures were created using all IPA claims and referrals data for the six-month period from August 2013 through January 2014 (January was the latest month of data available to me before the distribution of the treatment).[4] Re-randomization was not performed; the seed for the random number generator used to produce the assignment was set to the date that the randomization was implemented, 20140430.[5]

The randomization process appears to have been successful. Table 3.1 presents practice-level, pre-period sample averages by treatment status. These averages were calculated using the same IPA data used in the randomization, and are measured for the whole six-month period, not monthly. As far as notable differences, the control group was assigned more internal medicine practices than the treatment group, and the control group practices had more SrHMO claims per-PCP than treatment group practices. However, neither of these differences is statistically significant at conventional levels, and none of the other characteristics had significant differences either. Table 3.2 presents the pre-period distribution of PCP practices by number of physicians, which is similar between both treatment and control groups. A chi-squared test for distribution differences between groups failed to reject at conventional significance levels. Table 3.3 presents sample averages at the PCP-level, where the PCPs took their own assignments from the assignments of their practices. While there is still some difference between the groups in terms of the number of internists and the number of SrHMO claims, these are less pronounced than when calculated at the practice-level. As before, though, none of the characteristics has a statistically significant difference in means at conventional levels.

Pre-period regressions also find no evidence of important differences between the treatment and control groups. Table 3.4 presents the estimates of these regressions, which use the

[4] All of the IPA's referrals and claims data was made available for the preparation of the analysis of the experimental results. However, except for physician specialty, these claims and referrals provide the only information observed about the physicians. Hence, it is not possible to control for other types of physician characteristics, which is a limitation that should be kept in mind.

[5] The function runiform() in Stata/SE 12.1 for Windows was used for random number generation (StataCorp, 2011).

same referrals data relied upon for the randomization and the descriptive statistics discussed above, but measured in monthly intervals. In this case, since the data is all pre-period, the econometric model in equation 3.3 resolves to

$$\begin{aligned}\theta_{pst} =&\beta_1 \ln(C_s^{HMO})I_p + \beta_2 \ln(C_s^{SrHMO})I_p \\ &+ \beta_3 \ln(C_s^{HMO}) + \beta_4 \ln(C_s^{SrHMO}) + \alpha_p + \gamma_t + u_{pst}.\end{aligned} \tag{3.4}$$

The top panel of the table shows the results for the regression when θ_{ps} is calculated using all Ophthalmology referrals. Point estimates for the cost variables interacted with the treatment group dummy have the sign one might expect – negative – but they are small in magnitude and not statistically significant. Moreover, the cost variables that are not interacted are not significant and are small in magnitude as well. The middle panel of Table 3.4 presents the estimates when θ_{ps} is calculated using only standard HMO patient referrals. As before, neither cost-by-treatment interactions are significant, although the estimate for the SrHMO cost interaction variable is moderate in size, estimating an elasticity of approximately -0.04. The cost variable for HMO patients that is not interacted is the only coefficient in any of these pre-period regressions that is statistically significant, being so at the five percent level. The bottom panel of Table 3.4 has no significant coefficients and all of the point estimates are very close to zero.

3.5.2 Model Estimates

Table 3.5 presents sample averages for the model outcome variables for the relevant groups. It shows that, on average, an Ophthalmologist only receives about two percent of a PCP's Ophthalmology referrals in a one month period. This small average is driven by a substantial number of zeros in the data, since many Ophthalmologists receive no referrals at all from a given PCP in a given month. This is not surprising since a PCP is likely not familiar with all specialists in an area, and only has so many referrals to distribute in a month. This fact, though, could indicate that the relationship (if any) between specialist cost and referrals is not linear. In future work on this project, I will explore the feasibility of a multinomial model designed to handle choices between multiple options as a robustness check for the primary regression results.

All regression model coefficients are estimated using least squares methods, and all standard errors are estimated using cluster-robust methods, with the PCP practice defining the clusters. Given that PCPs that have referred to one specialist in the past are likely to do so again in the future, and are not likely to refer to specialists that they have not done so previously, standard errors estimated without clustering are likely to be inconsistent due to serial correlation (Bertrand et al., 2004). Beyond this, though, clustering in this case also addresses the fact that referrals are a zero-sum process – if one specialist gets a referral, all

others do not. Thus, one would expect that for a given PCP, error term covariances between specialists may be less than zero.

For consistency's sake, I discuss model estimates in terms of ten percent changes in specialist cost. In the context of this experiment, a ten percentage change in cost is a fairly moderately sized change, given the spread in averages on the cost report. For SrHMO patients, the largest average is more than 27% larger than the smallest, while for HMO patients, it is more than 126% larger.

Results from estimating equation 3.3 can be found in Table 3.6. The top panel presents the estimates when the dependent variable is, θ_{pst}, which reflects all referrals to Ophthalmology. The estimate for the first coefficient of interest, on the variable $\ln(C_s^{HMO})A_tI_p$, implies that an Ophthalmologist's referral share would increase by 0.001 in response to a ten percent cost increase. In contrast, the estimate for the coefficient on $\ln(C_s^{SrHMO})A_tI_p$ has the sign one would expect, and is comparatively large at a 0.007 decrease in referral share in response to a ten percent cost increase. Given that PCPs refer about twice as many SrHMO patients to Ophthalmology as standard HMO patients, it is not surprising that the magnitude of the estimate on the SrHMO cost variable is larger than the HMO cost variable (though the fact that it is larger by a factor of seven is unexpected). Neither estimate is statistically significant at conventional levels. That said, the effect for SrHMO patients, in particular, is large relative to the sample average of θ_{ps} of 0.02 (which is based on the control group during the pre-period). Relative to this benchmark, the estimate implies a 35% increase in the expected referral share. Furthermore, comparing the estimate to 0.17, which is the sample average of positive values of θ_{ps}, produces a lower bound of 0.4 for the average elasticity of those with positive referrals.

The middle panel of Table 3.6 presents the estimates when the dependent variable reflects HMO patients only, θ_{pst}^{HMO}. The estimate for the HMO patient effect, the coefficient on $\ln(C_s^{HMO})A_tI_p$, suggests that a ten percent increase in Ophthalmologist cost leads to a reduction of 0.005 in referral share. The estimate for the SrHMO effect is much smaller at 0.0003. As in the case for all referrals, neither estimate is significant at conventional levels. Despite that, the estimate for the HMO effect is relatively sizable. Compared to the control group's pre-period sample average of θ_{pst}^{HMO}, which is approximately 0.02, a ten percent cost increase implies a 25% decrease in expected referral share. The lower bound for the elasticity for positive-referral PCP and Ophthamology pairs is estimated as about 0.17, given that $\left(\bar{\theta}_{pst}^{HMO}\middle|\theta_{pst}^{HMO}>0\right)\approx 0.3$. The most notable aspect of this regression is the contrast between the HMO and SrHMO effects, which is consistent with behavior on the part of the PCPs where they base their HMO patient referral decisions on the HMO patient average costs, not the SrHMO costs.

Turning now to the bottom panel of Table 3.6, estimates for the case when the outcome variable is based on SrHMO patients only, θ_{pst}^{SrHMO}, are reported. For the HMO patient

effect (the coefficient on $\ln(C_s^{HMO})A_tI_p$), the estimate suggests that a ten percent increase in Ophthalmologist cost leads to an increase in referral share of 0.0026. The SrHMO effect (the $\ln(C_s^{SrHMO})A_tI_p$ coefficient) is estimated as a decrease of 0.004 in response to a ten percent cost increase. Once again, neither of the main coefficients of interest are statistically significant, though the contrast of effects (SrHMO effect larger and having a negative sign) is again at least somewhat consistent with PCP behavior of basing referral decisions for a given patient on the relevant cost information. Additionally, as before, the larger estimate here is comparatively large. The pre-period sample average of the outcome variable for the control group is again 0.2. Relative to this, the estimate of 0.004 represents a 20% decrease in expected referral share in response to a ten percent cost increase. Also, the lower bound of the elasticity for those PCP and ophthalmologist pairs with positive referrals is estimated at 0.2 for the SrHMO patients.

3.6 Conclusion

The experiment described in this paper is still at a preliminary stage. Only one month of post-period data was available for analysis at the time of its writing. As a consequence, all results discussed are preliminary and may very well change in future versions of the experimental analysis. Nevertheless, the currently available data suggests several things. First of all, the randomization seems to have been successful. Analysis of the pre-period data suggests that the treatment and control groups were similar with regard to observable characteristics. This was true using sample averages for six-month-period-measured characteristics, and regression on monthly-measured data.

Secondly, using the data so far collected, there is no statistically significant evidence of an effect of the cost reports on the referral patterns of PCPs. That said, point estimates have magnitudes that are moderate to large when compared to baseline measures of expected referral shares. Given that the precision of the experiment will likely increase as more data is collected, the large magnitudes may become statistically significant in later analyses of the experimental data.

Lastly, although the estimates are not currently significant, the estimate patterns are suggestive that physicians have paid attention to and applied the appropriate information to the relevant patients. If this result strengthens in future analyses of the data, this would be impressive, as anecdotal reports have suggested that the IPA's PCPs have many important concerns competing for their attention. This would suggest they rate the importance of the cost information as high enough for them to pay attention to it and incorporate it into their practice. Future work on this experiment will pay close attention to this portion of the analysis.

May 1, 2014

Dear Doctor,

As requested by our primary care physicians, we are continuing our efforts to share information on specialty costs by rendering physician. To that end, please find the included report on average costs per patient for GNP Ophthalmology practices. These costs are based on actual claims from encounters with patients who were newly referred to Ophthalmology, and who had their first encounters with Ophthalmologists during the twelve-month period from July 2012 through June 2013. All claims over the 180-day period following the first encounter were used in the calculations.

Since our goal was to produce a broad measure of cost, we calculated the averages using claims for patients across a range of diagnoses. However, in order to increase the comparability of the averages, we only used diagnoses that were common across practices, and adjusted diagnosis proportions to reflect GNP-wide prevalence instead of individual practice level prevalence. As a result, for the patients included in this analysis, cataract diagnoses were the most common, occurring roughly 50% of the time. Since cataract conditions are relatively costly to treat, these patients accounted for almost 71% of the average costs reported.

Lastly, to further improve comparability, only practices that saw more than 300 newly referred patients and had patient satisfaction scores above 80% were included.

Sincerely,

Chief Executive Officer

Figure 3.1: Cover Letter for Ophthalmologist Cost Report

Average 180-Day Cost for Newly Referred Patients to Ophthalmology

For patients with first encounters with Ophthalmology during the twelve-month period from July 2012 through June 2013

Practice / Physician Name	HMO Patients	SrHMO Patients
101 *(Dr. A, Dr. B)*	$147	$450
102 *(Dr. C, Dr. D)*	$215	$502
103 *(Dr. E, Dr. F)*	$230	$456
104	$270	$575
105 *(Drs. G,H, I, J, K)*	$292	$561
106	$333	$470

Notes:

(1) Ophthalmology is paid fee-for-service for HMO patients. For SrHMO patients, approximately 75% of procedure codes are capitated, with the rest being fee-for-service. Costs for capitated codes are based on the Medicare fee schedule for claims submitted.

(2) Averages have been adjusted for observable differences in the underlying health of specialist patient populations, and rounded to the nearest dollar.

(3) Newly referred patients are those that had not had a claim in Ophthalmology for the previous 180-days and were referred to Ophthalmology by an internist or FP during the previous 180-days.

(4) Ophthalmology practices included on this report all had at least 300 newly referred patients, had patient satisfaction scores for all practice ophthalmologists exceeding 80%.

Figure 3.2: Ophthalmologist Cost Report

Table 3.1: Comparison of practice-level averages before experiment start

	Sample Averages			
	Control Group	Treatment Group	T-statistic	P-value
Internal Medicine Practice	0.5185	0.3571	1.2003	0.2354
Total SrHMO referrals per-PCP $\geq$ all-practice median	0.5185	0.5714	-0.3873	0.7001
Total HMO referrals per-PCP $\geq$ all-practice median	0.5185	0.6071	-0.6530	0.5166
Total SrHMO claims per-PCP $\geq$ all-practice median	0.5556	0.4643	0.6672	0.5075
Total HMO claims per-PCP $\geq$ all-practice median	0.5185	0.5000	0.1348	0.8932
Total referrals per-PCP (all types)	335.1759	301.4583	0.8553	0.3962
Total practice SrHMO Ophthalmology referrals	34.5185	34.0357	0.0615	0.9512
Total practice HMO Ophthalmology referrals	17.4444	20.4286	-0.6053	0.5476
Total practice Ophthalmology referrals	51.9630	54.4643	-0.2099	0.8345
Number of claims per-PCP	707.9877	629.1250	1.0352	0.3053
Number of HMO claims per-PCP	380.1574	365.5565	0.2581	0.7974
Number of SrHMO claims per-PCP	327.8302	263.5685	1.5898	0.1178
PCP's share of referrals that are male patients	0.4194	0.4109	0.2813	0.7795
PCP's share of referrals that are patients 60 or older	0.5886	0.5593	0.7675	0.4462
PCP's share of referrals that are patients ages 40 to 59	0.2927	0.3056	-0.5273	0.6002
PCP's share of referrals that are patients ages 18 to 39	0.1187	0.1352	-0.9423	0.3503

The sample includes 55 primary care physician practices total: 27 in the control group and 28 in the treatment group. T-statistic calculation assumes equal variances. P-value is for the two-sided test that there is no difference in means. Calculated using all IPA claims and referrals data for the six-month period from August 2013 through January 2014.

Table 3.2: Practice size distribution by treatment status

Number of PCPs in practice	Control Group	Treatment Group	Any Treatment Status
1	16	17	33
	59.26%	60.71%	60.00%
2	7	4	11
	25.93%	14.29%	20.00%
3	3	5	8
	11.11%	17.86%	14.55%
4	1	1	2
	3.70%	3.57%	3.64%
6	0	1	1
	0.00%	3.57%	1.82%
All Practice Sizes	27	28	55
	100.00%	100.00%	100.00%

Percentages are relative to column totals. Pearson's chi-square statistic equals 2.3311 and P-value equals 0.675.

Table 3.3: Comparison of PCP-level averages before experiment start

	Sample Averages			
	Control Group	Treatment Group	T-statistic	P-value
Total referrals (all types)	347.3488	322.6600	0.7827	0.4358
Total SrHMO Ophthalmology referrals	21.6744	19.0600	0.9639	0.3376
Total HMO Ophthalmology referrals	10.9535	11.4400	-0.3736	0.7096
Total Ophthalmology referrals	32.6279	30.5000	0.6483	0.5184
Total claims	724.7907	657.0400	1.0507	0.2962
Total HMO claims	396.8372	374.0000	0.4840	0.6295
Total SrHMO claims	327.9535	283.0400	1.2367	0.2194
Internal Medicine Specialty	0.4186	0.3400	0.7742	0.4408
Share of referrals that are male patients	0.4156	0.4034	0.4072	0.6848
Share of referrals that are patients 60 or older	0.5736	0.5665	0.2218	0.8249
Share of referrals that are patients ages 40 to 59	0.2950	0.3037	-0.4602	0.6465
Share of referrals that are patients ages 18 to 39	0.1315	0.1297	0.1105	0.9122

The sample includes 93 PCPs total: 43 in the control group and 50 in the treatment group. T-statistic calculation assumes equal variances. P-value is for the two-sided test that there is no difference in means. Calculated using all IPA claims and referrals data for the six-month period from August 2013 through January 2014.

Table 3.4: Pre-period regression results – based on practice-level data

Variable	Coefficient Estimate	Standard Error	T-statistic	P-value
Dependent variable = θ_{pst} *(all referrals)*				
$\ln(C_s^{HMO})I_p$	-0.0007	0.0157	-0.04	0.965
$\ln(C_s^{SrHMO})I_p$	-0.0151	0.0400	-0.38	0.708
$\ln(C_s^{HMO})$	0.0146	0.0113	1.30	0.201
$\ln(C_s^{SrHMO})$	-0.0189	0.0287	-0.66	0.513
Dependent variable = θ_{pst}^{HMO} *(only HMO referrals)*				
$\ln(C_s^{HMO})I_p$	0.0071	0.0177	0.40	0.690
$\ln(C_s^{SrHMO})I_p$	-0.0358	0.0461	-0.78	0.442
$\ln(C_s^{HMO})$	0.0239	0.0118	2.03	0.047
$\ln(C_s^{SrHMO})$	-0.0363	0.0341	-1.07	0.292
Dependent variable = θ_{pst}^{SrHMO} *(only SrHMO referrals)*				
$\ln(C_s^{HMO})I_p$	-0.0031	0.0162	-0.19	0.849
$\ln(C_s^{SrHMO})I_p$	-0.0005	0.0420	-0.01	0.991
$\ln(C_s^{HMO})$	0.0073	0.0111	0.66	0.512
$\ln(C_s^{SrHMO})$	-0.0052	0.0268	-0.19	0.848

All regressions had 1980 observations (one observation for each combination of month, PCP practice, and Ophthalmology practice). Standard errors are clustered on PCP practice, and p-values are for two-sided tests. Dummies for each PCP practice and month are included but not reported. The dependent variables were calculated using all IPA referrals data for the six-month period from August 2013 through January 2014. Specialist average costs were calculated using IPA claims data for new patient referrals to Ophthalmology during the period July 2012 to June 2013.

Table 3.5: Experimental results: dependent variable sample averages

	Group		
	Control	Treatment	Both
Dependent variable = θ_{pst} *(all referrals)*			
Pre-period	0.0207	0.0193	0.0200
Post-period	0.0202	0.0201	0.0201
Both	0.0206	0.0194	0.0200
Dependent variable = θ_{pst}^{HMO} *(only HMO referrals)*			
Pre-period	0.0223	0.0215	0.0219
Post-period	0.0248	0.0274	0.0262
Both	0.0227	0.0224	0.0225
Dependent variable = θ_{pst}^{SrHMO} *(only SrHMO referrals)*			
Pre-period	0.0205	0.0195	0.0200
Post-period	0.0177	0.0163	0.0170
Both	0.0201	0.0190	0.0196

Based on 2,310 month by PCP-practice by Ophthalmology-practice observations. Individual cells have: 972 observations for the pre-period control group; 1,008 for pre-period treatment group; 162 for post-period control group; and 168 for post-period treatment group.

Table 3.6: Model estimation results – based on practice-level data

Variable	Coefficient Estimate	Standard Error	T-statistic	P-value
Dependent variable = θ_{pst} (all referrals)				
$\ln(C_s^{HMO})A_tI_p$	0.0127	0.0197	0.65	0.521
$\ln(C_s^{SrHMO})A_tI_p$	-0.0711	0.0517	-1.38	0.175
A_tI_p	0.3735	0.2505	1.49	0.142
$\ln(C_s^{HMO})A_t$	-0.0038	0.0140	-0.27	0.786
$\ln(C_s^{SrHMO})A_t$	0.0348	0.0404	0.86	0.393
$\ln(C_s^{HMO})I_p$	-0.0163	0.0174	-0.94	0.353
$\ln(C_s^{SrHMO})I_p$	-0.0106	0.0451	-0.23	0.816
$\ln(C_s^{HMO})$	0.0231	0.0143	1.61	0.113
$\ln(C_s^{SrHMO})$	-0.0230	0.0348	-0.66	0.513
Dependent variable = θ_{pst}^{HMO} (only HMO referrals)				
$\ln(C_s^{HMO})A_tI_p$	-0.0515	0.0425	-1.21	0.231
$\ln(C_s^{SrHMO})A_tI_p$	-0.0032	0.1067	-0.03	0.976
A_tI_p	0.3054	0.5164	0.59	0.557
$\ln(C_s^{HMO})A_t$	0.0186	0.0228	0.82	0.417
$\ln(C_s^{SrHMO})A_t$	-0.0013	0.0782	-0.02	0.987
$\ln(C_s^{HMO})I_p$	-0.0130	0.0207	-0.63	0.531
$\ln(C_s^{SrHMO})I_p$	-0.0282	0.0641	-0.44	0.662
$\ln(C_s^{HMO})$	0.0282	0.0147	1.92	0.060
$\ln(C_s^{SrHMO})$	-0.0351	0.0509	-0.69	0.493
Dependent variable = θ_{pst}^{SrHMO} (only SrHMO referrals)				
$\ln(C_s^{HMO})A_tI_p$	0.0257	0.0209	1.23	0.223
$\ln(C_s^{SrHMO})A_tI_p$	-0.0418	0.0591	-0.71	0.483
A_tI_p	0.1182	0.2952	0.40	0.691
$\ln(C_s^{HMO})A_t$	-0.0171	0.0130	-1.32	0.193
$\ln(C_s^{SrHMO})A_t$	0.0422	0.0335	1.26	0.213
$\ln(C_s^{HMO})I_p$	-0.0182	0.0210	-0.87	0.389
$\ln(C_s^{SrHMO})I_p$	-0.0090	0.0478	-0.19	0.851
$\ln(C_s^{HMO})$	0.0225	0.0164	1.37	0.175
$\ln(C_s^{SrHMO})$	-0.0217	0.0329	-0.66	0.513

All regressions had 2,310 observations (one observation for each combination of month, PCP practice, and Ophthalmology practice). Standard errors are clustered on PCP practice, and p-values are for two-sided tests. Dummies for each PCP practice and month are included but not reported. The dependent variables were calculated using all IPA referrals data for the six-month period from November 2013 through April 2014, plus the month from May 15th to June 15th, 2014. Specialist average costs were calculated using IPA claims data for new patient referrals to Ophthalmology during the period July 2012 to June 2013.

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