Nurse Practitioner Independence, Health Care Utilization, and Health Outcomes*

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May 4th, 2014

Abstract

Many states allow nurse practitioners (NPs) to practice and prescribe drugs without physician oversight, increasing the number of autonomous primary care providers. We estimate the causal impact of NP independence on population health care utilization rates and health outcomes, exploiting variation in the timing of state law passage. We find that more independent primary care providers increases the frequency of routine checkups, improves care quality, and decreases emergency room use by patients with ambulatory care sensitive conditions. These effects come from decreases in administrative costs for physicians and NPs and patients' indirect costs of accessing medical care.

*We thank Karl Scholz, Chris Taber, Tom DeLeire, David Molitor, Kurt Lavetti, and seminar participants at the Midwest Health Economics Conference, APPAM Fall Research Conference, and Congressional Budget Office for useful comments and suggestions. Victoria Lee provided excellent research assistance. The research in this paper was conducted at the CFACT Data Center, and the support of AHRQ is acknowledged. The results and conclusions in this paper are those of the authors and do not indicate concurrence by AHRQ or the Department of Health and Human Services. Funding for this project was provided by the Robert Wood Johnson Foundation Health & Society Scholars program at the University of Wisconsin-Madison. All remaining errors are our own.

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1 Introduction

Implementation of the Affordable Care Act is expected to increase demand for medical services across the U.S., particularly for primary care services. Primary care providers offer regular preventive medical care, reducing the number of routine medical problems that become emergency room visits and keeping chronic conditions from worsening until a hospital stay is required. With 7 out of 10 deaths in the U.S. the result of chronic disease and nearly half of all adults diagnosed with a chronic illness, increasing utilization of preventive services may have significant long-term impacts on population health outcomes.[1] As primary care is generally more cost effective than acute care in an emergency room or hospital, diverting patients to preventive care may reduce medical care costs. Towards both these ends, the Affordable Care Act contains a number of provisions designed to increase U.S. consumption of primary care, including mandating coverage for preventive services, funding community disease prevention measures, and increasing the number of primary care providers.[2]

Petterson et al. (2012) estimate that the U.S. will have a shortage of just under 52,000 primary care physicians by 2025. Increasing the supply of primary care providers is a difficult task, and the number and variety of methods for doing so included in the Affordable Care Act reveal the importance of this goal and the lack of consensus on the best way to achieve it.[3] Traditionally, physicians have been the health care professionals responsible for providing primary care. But as new physicians increasingly choose to specialize, non-physicians provide a greater share of primary care. In 2010, nurse practitioners (NPs) were the largest group of non-physician primary care providers, with approximately 56,000 NPs in primary care representing 19% of all primary care providers (Schwartz et al., 2011; AHRQ, 2011). Since the mid-1990s, the number of NPs in primary care has grown at 9.4% per year while the number of physicians in primary care has grown at 1.1% per year, so NPs will likely be a larger share of primary care providers in the future (GAO, 2008). NPs are also slightly more

[1] For information on rates of various chronic diseases in the U.S., see the Center for Managing Chronic Disease (http://cmcd.sph.umich.edu/statistics.html) and the Center for Disease Control (http://www.cdc.gov/chronicdisease/).

[2] See http://www.healthreform.gov/newsroom/primarycareworkforce.html for more details on the provisions of the Prevention and Public Health Fund, established as part of the Affordable Care Act.

[3] Heisler (2013) lists 32 distinct sections of the Affordable Care Act designed to affect the supply of primary care physicians.

likely than physicians to practice in rural areas, helping alleviate distributional problems in primary care (AHRQ, 2012).

The extent to which NPs can fill the primary care role of physicians is partly governed by state scope-of-practice laws that regulate the types of medical services that NPs can provide and the necessary level of physician involvement in NP practice. Most notably, some states allow NPs to both practice and prescribe drugs without physician supervision, effectively enabling them to be independent providers of primary care services. Scope-of-practice laws are potentially important policy tools through which states can influence the total amount of preventive care provided, the provision of medical care in rural areas or to the underserved, and overall medical care costs.[4] In early 2012, more than 50 bills affecting scope-of-practice laws for NPs were under consideration in state legislatures across the U.S. (Cassidy, 2012). However, little is known about the effect of allowing NPs to be independent primary care providers on the utilization of primary care and population health outcomes.

The implications of NP independence for health care utilization are not immediately clear. NP independence may lead to greater health care utilization if independent NPs practice in areas with few doctors, thereby lowering the cost of accessing health care in these areas, or by removing administrative burdens of collaboration between NPs and doctors, thereby releasing time for patient care. Primary care utilization might not respond or decrease if fewer physicians provide primary care when NPs do not require supervision or if patients believe that unsupervised NPs provide a lower quality of care. Greater utilization of primary care may not improve societal welfare if the increase is concentrated among those already receiving primary care or if providers respond to increased competition by inducing more demand for services.

The impact of NP independence on health outcomes is also of significant debate and interest. The Institute of Medicine claims that "what nurse practitioners are able to do once they graduate varies widely for reasons that are related not to their ability, education or training, or safety concerns, but to the political decisions of the state in which they work" (Institute of Medicine, 2010, p. 5). A number of randomized trials conclude that patients using NPs for primary care have comparable health outcomes to patients who use

[4]See Lopatto (2012) for a popular press article advancing these arguments.

doctors.[5] NPs managing relatively simple ailments without the supervision of a doctor may also allow doctors to work on cases where advanced training has a larger marginal impact on patient health. On the other hand, physician groups argue that NPs working without physician supervision will lead to a decrease in the quality of primary care because NPs have less medical training than doctors (American Academy of Family Physicians, 2012). This relative deficiency in training may lead to misdiagnoses or patients receiving inadequate care for complex conditions. Randomized trials with few participating patients or short follow-up periods may not observe these effects or their results may not generalize to other settings, common weaknesses in many studies of NP effectiveness (Sox, 2000; Laurant et al., 2004).

We estimate the causal effect of NP independence on population utilization of care and health outcomes by exploiting plausibly exogenous variation in the timing of changes in state regulations governing the level of supervision that doctors must provide for NPs. We collect data on state scope-of-practice laws for NPs from 1970 to 2011. Identification comes from within state variation in regulations over time. We find that the timing of changes in NP regulations is not explained by state health care concerns, supporting the arguments of Safriet (2002), Institute of Medicine (2010), and Isaacs and Jellinek (2013).

We obtain individual level data on health care utilization and outcomes from the Medical Expenditure Panel Survey. As states have allowed NPs to practice or prescribe independently for different lengths of time, we use an event study approach to investigate the difference between short and long run effects of NP independence. These nationally representative data allow us to look for effects across a wider population and over a longer period than the randomized control trials on NP effectiveness. We also look for heterogeneous effects of NP independence in support of potential policy goals, such as whether female, rural, uninsured, or medically underserved populations have greater changes in utilization or health outcomes.

Several recent papers examine changes in health care utilization or health outcomes in response to changes in the supply of medical providers or occupational licensing. Cook et al. (2012) find that increasing minimum nurse staffing ratios does not improve patient

[5] See Naylor and Kurtzman (2010) for a summary of these studies. The two most prominent are the Burlington randomized control trial of Sackett et al. (1974) and Spitzer et al. (1974), which followed approximately 1600 families for 1 year, and a more recent analysis by Mundinger et al. (2000) and Letz et al. (2004) that followed approximately 1300 patients for 1 year and 400 patients for 2 years. Sox (2000) and Laurant et al. (2004) offer critical assessments of many of these studies.

outcomes, while Garthwaite (2012) finds that physicians spend less time with patients after public insurance expansions. Both papers show that increasing the number or availability of medical providers does not imply that utilization will rise or outcomes will improve, though neither focuses on NPs or scope-of-practice laws as we do.

Kleiner et al. (2012) and Stange (2014) are most similar in spirit to our paper. Kleiner et al. (2012) analyze responses in NP and physician wages and the prices of primary care services when NPs gain more independence between 2002 to 2007 using a difference-in-difference approach. They find that the wages of NPs and physicians both increase when NPs have more independence, while the price of well-child visits falls. Our paper has a longer time horizon and focuses on changes in utilization and health outcomes rather than labor market outcomes for primary care providers. Stange (2014) estimates the effect of county-level variation in the number of NPs and PAs on health care expenditures and preventive screenings, finding minimal impact.[6] Our paper has a different source of identification and estimates the direct impact of changes in regulations rather than head counts of suppliers, as regulations are more easily changed by policymakers. Kalist and Spurr (2004) suggest that regulation impacts long-term provider supply, motivating our use of time-varying treatment effects to test the extent to which changes in regulations or NP supply drive our results.[7]

We find that states that allow NPs to practice and prescribe without physician supervision see increases in health care utilization and care quality, with utilization increases primarily coming from adults while quality improvements impact both adults and children. We find that these increases have positive effects on self-reported health status. Patients with conditions that respond to primary care show reductions in emergency room use, suggesting both an improvement in health and cost savings in the provision of care. The response to NP independence shows little heterogeneity across the population, so the gains in health outcomes are widespread. States with a greater population share in primary care shortage areas have larger gains in utilization, indicating the effectiveness of changes in NP regulations as a policy to help underserved populations. Our evidence suggests that the primary mechanisms

[6]Stange (2014) also uses a difference-in-difference approach to show that preventive screening frequency does not change when NPs and PAs are allowed to prescribe controlled substances.

[7]Kalist and Spurr (2004) show that NP independence, under a somewhat different definition than ours, increases enrollments in NP training programs.

for these changes are the elimination of physician and NP time spent on supervision and decreases in indirect costs of receiving medical care such as better appointment availability and lower patient travel costs, rather than changes in provider supply or composition.

2 Background and Related Literature

Advanced practice nurses (APNs) are registered nurses who serve as health care providers in a broad range of settings. APNs include nurse practitioners, certified nurse midwives, certified registered nurse anesthetists, and clinical nurse specialists. In 2010, approximately two thirds of all APNs in the U.S. were NPs (Phillips, 2011). NPs most commonly specialize in primary care areas such as family practice, adult practice, women's health, pediatrics, and gerontology. NPs provide primary and preventive health care services, prescribe medications, diagnose and treat common minor illnesses and injuries, and counsel patients on adopting healthy lifestyles. NPs practice in community clinics, health centers, urgent care centers, hospitals, nurse practitioner practices/offices, retail-based clinics, and walk-in clinics.

Since the first NP program in Colorado in 1965, states set their own laws governing the scope of NP practice. These laws vary widely by state. States may require physicians to be physically on site to supervise an NP or require a written collaborative agreement between an NP and an offsite physician detailing referral policy and physician review of patient charts. States can mandate that physicians review or cosign all NP prescriptions, restrict prescriptions of certain medications, impose limits on private or Medicaid reimbursement and hospital privileges, and levy strict malpractice liability insurance requirements for NPs. For example, Texas requires that a collaborating physician spend at least one out of every ten days at the clinic where the NP practices, the NP's practice be located no more than 75 miles from the physician's office, and a randomly selected 10% of the NP's patient charts be reviewed each month (Pettypiece, 2013; Traweek and Goodman, 2011). Alabama similarly requires that a collaborating physician supervise at least 10% of the NP's working hours and review all adverse patient outcomes and 10% of all medical records (Alabama Board of Medical Examiners Administrative Code, 2012).

We focus on the two most significant areas of delivering primary care, practice authority (diagnosing and treating patients) and prescriptive authority (prescribing medications). While any barrier can reduce the willingness of an NP to have a fully independent practice, a requirement for physician involvement in NP practice or prescriptive authority legally constraints an NP's ability to setup primary care facilities in any chosen location and provide patients with medications. We consider NPs in states with laws requiring supervision or collaboration on any aspect of practice or prescriptive authority to be *dependent* and NPs in states without such laws to be *fully independent*.

Table 1 shows that after Montana's 1984 adoption of full NP independence, the number of states allowing NPs full independence has risen steadily. The chaos behind the passage of NP scope-of-practice laws is documented in several sources. According to Safriet (2002, p. 302-304), legislators are "bombarded by heavily-financed lobbying efforts emanating from state and national professional organizations, individual health care providers (who are also voters), and interested citizens" and scope-of-practice laws are influenced by "individual providers' professional groups, institutional providers' organizations, employers' associations, insurance and financing federations, specialized consumer advocacy groups, pharmaceutical and medical device manufacturers, and legislative and regulatory entities both state and federal." Isaacs and Jellinek (2013, p. 27) survey medical society executives and find that in a state with very restrictive scope-of-practice laws, an anonymous physician group leader credits "(a) having an effective lobbying team, (b) having representation on all the relevant boards, like the board of medical examiners, and (c) having friends on the key committees in the legislature. We invite them to our functions, we contribute to their campaigns, and we serve as a resource to them." Leaders in other states cite "pure political might" and "sheer political muscle" as main reasons for their states' current scope-of-practice laws (Isaacs and Jellinek, 2013, p. 25-26). The Federal Trade Commission indirectly pressured states to allow NPs more independence by holding hearings in 2002 on the level of competition between different types of providers of care.[8] These hearings indicate federal concern over state laws requiring physician supervision of NP practice and suggest that population health and welfare are not overriding factors for state legislatures and regulators when determining the

[8]See `http://www.ftc.gov/bc/healthcare/research/healthcarehearing.htm` for a summary of the hearings.

Table 1: NP Practice and Prescriptive Authority by State and Year

Independent practice authority	AK (1984), AR (1995), AZ (2000), CO (1980), CT (1989 - repealed in 1997 by attorney general decision), DC (1995), HI (1994), IA (1983), ID (2004), KY (2000), MD (2010), ME (1996), MI (1978), MT (1984), ND (1980), NH (1991), NJ (1996), NM (1993), OK (1972), OR (1987), RI (1975), TX (1989 - repealed in 2005), UT (1993), VT (2011), WA (1973), WV (1991), WY (1993)
Independent prescriptive authority	AK (1987), AZ (1984), CO (2010), DC (1995), HI (2011), IA (1995), ID (2004), MD (2010), ME (1996), MT (1984), ND (2011), NH (1991), NM (1993), OR (1979), UT (1998), VT (2011), WA (2001), WI (1995), WY (1993)
Independent practice and prescriptive authority	AK (1987), AZ (2000), CO (2010), DC (1995), HI (2011), IA (1995), ID (2004), MD (2010), ME (1996), MT (1984), ND (2011), NH (1991), NM (1993), OR (1987), UT (1998), VT (2011), WA (2001), WY (1993)

Data collected by authors from state statutes and Board of Nursing rules and regulations, cross-referenced with January issues of *The Nurse Practitioner* from 1995-2011. Year in parentheses is the year that state allows NPs the form of independence listed at left.

degree of NP independence. In our empirical work, we examine state-level determinants of the decision to allow NPs full independence.

Independence for NPs is also a potential source of cost savings in medical care. Medicare reimburses independent NPs at 85% of the rate of physicians for services performed, while Medicaid and private insurance reimbursement rates for NPs vary. Cost savings may also come from lower salaries and less training time for NPs relative to physicians. In a recent analysis of the Massachusetts health care reform, Eibner et al. (2009) estimate that medical costs would decrease between 0.6 and 1.3% over 10 years if Massachusetts allowed NPs full independence.[9] Given the similarities between Massachusetts' health care law and the provisions of the Affordable Care Act, this estimate of cost savings may be applicable for other states considering allowing NPs full independence.

Supplier-induced demand may complicate analysis of health care utilization when NPs are fully independent. Many early papers, following the work of Fuchs (1978), find that suppliers of medical services are able to increase demand beyond the optimal amount for patients to consume because providers and patients have asymmetric information about patients' medical conditions.[10] NP full independence may increase competition among physicians and NPs for patients, leading to an increase in primary care services consumed that does not increase welfare as NPs and physicians seek to boost practice revenues.

We do not believe that supplier-induced demand is a concern in this case for several

[9] The estimates here also include cost savings from relaxing scope-of-practice laws for physician assistants. However, NPs outnumbered physician assistants 4 to 1 in Massachusetts in 2007 when the estimates were created and the proposed policy changes for physician assistants did not include some freedoms given to NPs. Thus, the vast majority of this cost savings comes through NP independence.

[10] See Dranove (1988) for a summary of this literature.

reasons. The evidence in favor of supplier-induced demand is strongest for medical providers ordering additional tests when they own the equipment used for the tests (Hillman et al., 1990; Mitchell and Scott, 1992) or in procedures ordered by specialists with less opportunity for repeat business from the same patient than primary care providers have (Gruber and Owings, 1996). Hooker and McCaig (2001) show that there is little difference in the number of diagnostic tests ordered by NPs and physicians. Studies that have focused on primary care providers, such as Grytten et al. (1995), Van De Voorde et al. (2001), Cockx and Brasseur (2003) and Madden et al. (2005), do not find evidence of supplier-induced demand in the number of visits to primary care providers. The available empirical evidence suggests that there is at most a minimal effect of supplier-induced demand in primary care services.

3 Data

Our main analysis uses data on health care utilization and health outcomes from the Medical Expenditure Panel Survey (MEPS) Full Year Consolidated Data Files over the period 1996-2011. We treat this as a repeated cross-section of information about individuals in different states over time. The data contains information on checkups, emergency room visits, and total visits to a variety of medical providers, as well as self-reported health status and measures of patient ease of access and perceived quality of care. The data also contains a rich set of individual characteristics, including race, gender, education, marital status, income, employment status, and type of health insurance. The confidential version of the MEPS data that we use reveals each individual's state of residence and more detailed information about medical conditions and procedures than is available in the public use version.

Our primary data sources for laws governing NP practice are state statutes and regulations from state Boards of Nursing from 1970 to present. We cross-reference these data with annual surveys published in the 1995-2012 January issues of *The Nurse Practitioner* for the overlapping years. The survey data in *The Nurse Practitioner* are obtained from representatives of state nursing organizations or from a member of the state Board of Nursing and are based on the interpretation of the state statutes or regulations of the survey responder.

We define independent practice authority for NPs as the absence of statutory or regulatory requirements for physician collaboration or supervision and independent prescriptive authority for NPs as the right to prescribe medications (including controlled substances, if allowed) without physician collaboration or supervision. We consider a state to allow NPs full independence if the state offers NPs both independent practice authority and prescriptive authority. In these states, NPs may establish a practice without any physician involvement. 17 states and the District of Columbia allowed NPs full independence in 2011. Table 1 lists the years in which states changed their laws to allow NPs independent practice authority, independent prescriptive authority, and full independence from physician supervision.

In order to interpret the parameter estimates of our event study causally, the timing of states' decisions to allow NPs full independence must be exogenous to the outcomes of interest. As discussed above, state laws on NP practice are often the result of state board regulatory decisions made by political appointees, attorney general opinions, or other factors related to political bargaining rather than health concerns. We present evidence of the disorganization and unpredictability in law passage by looking for characteristics of states correlated with NPs gaining full independence. We choose variables that reflect components of population health as well as proxies for the political power of physician groups motivated by the political discussions of Safriet (2002), Institute of Medicine (2010), and Isaacs and Jellinek (2013). For each state and year, we include the physician to population ratio in the state, the amount of public money spent on medical benefits, the percentage change in public money spent on medical benefits, and the number of inpatient days and outpatient visits in the state per 100,000 residents. Each of these variables captures part of the state's current equilibrium in health care markets. The physician to population ratio may also reflect the relative political clout of physicians. We include the number of medical schools in a state as well as a dummy for whether the governor is a Democrat to measure the influence of political factors, as doctors may have greater political influence in states with more medical schools and the governor appoints regulators to state Boards of Nursing that can determine NP scope of practice.[11] We also include the share of the population over 65, under 20, living in an urban area, and real personal income to proxy for demand for medical services.[12]

[11] See American Medical Association (2009) for a list of appointment procedures to state Boards of Nursing.

[12] Data on number of physicians, inpatient and outpatient visits, and medical schools from the Area Resource File of the U.S. Department of Health and Human Services. Data on inpatient and outpatient

We compare the 1970 characteristics of states that ever allow full NP independence to states that never do so to see if there are pre-treatment systematic differences in the observables of the two types of states. We use 1970 as the start point for decisions over NP independence as it predates Montana's decision to become the first state to allow full NP independence in 1984, comes after the founding of the first NP training program in 1965, and is the earliest data point consistently available for many of the covariates we wish to compare. Table 2 presents our results. Though our test for differences among states has large standard errors because we have only 51 observations in 1970, we find some statistically significant differences between states that ever permit full NP independence and those that do not. We find that states that allow NPs full independence tend to have lower medical expenditures per person, fewer medical schools, and a younger population.[13] This contradicts the notion that high spending, older states permit NP full independence to cut primary care costs or to increase the availability of medical care for an elderly population. We separately examine children's health care in our empirical work below.

One interpretation of the fact that states that ever allow NPs full independence have many fewer medical schools is that the number of medical schools proxies for the future supply of medical service providers. However, this does not appear to be true. Only 38.6% of doctors practice in the state where they went to medical school (American Association of Medical Colleges, 2011) and the physician to population ratio is almost identical in states that ever allow full NP independence and those that never do. We also find no evidence that this difference in the number of medical schools in 1970 leads to later differences in physician to population ratios: by 2007, there were 261.41 physicians per 100,000 population in states that ever allow NPs full independence and 264.03 in states that never allow NPs full independence. Instead, we interpret this finding as evidence that the number of medical schools is proxying for the political power of physicians in the state. The observed differences across states and their persistence over time motivate the inclusion of state fixed effects in

visits is linearly interpolated between 1970 and 1975 and between 1975 and 1980. Data on public medical benefits and real personal income from the BEA Regional Economic Information System. Public medical benefits are defined by the BEA as payments made directly or through intermediaries to vendors for care provided to individuals for medical purposes, consisting of payments from Medicare, Medicaid, and State Children's Health Insurance Programs (SCHIP). We adjust personal income data to constant 2010 dollars using the CPI-U from the BLS. Share of population over 65, under 20, and urban from Census Bureau.

[13] These same differences exist in state data over 1970-1980, so they are not an artifact of the small sample.

Table 2: 1970 Characteristics of States by NP Independence

State Characteristics	Ever have full NP indep. Mean	Never have full NP indep. Mean	Difference (Ever-Never)
Physicians per 100,000	137.54	137.11	0.43
Medical Benefits per 100,000	$5,128.30	$6,660.61	-$1532.31*
Change in Medical Benefits per 100,000	10.67%	15.07%	-4.40%
Inpatient Days per 100,000	123,751.10	129,941.60	-6,190.50
Outpatient Visits per 100,000	96,718.63	83,999.98	12,718.65
# of Medical Schools	1.18	4.91	-3.73***
Democrat Governor	0.36	0.27	0.09
Share over 65	0.09	0.10	-0.01
Share under 20	0.39	0.37	0.02**
Share urban	0.69	0.74	-0.05
Real Personal Income	$4,027.38	$4,091.57	-$64.19
Observations	18	33	

*, **, *** denote statistical significance at the 10%, 5%, and 1% levels respectively. Variables as defined in text. All means calculated using state level population weighted data from 1970 only, except for "Change in Medical Benefits per 100,000," which is calculated using data from 1970 and 1971.

our empirical work, so both observed and unobserved differences across states accounted for by these fixed effects do not affect the internal validity of our estimates. We address the external validity of our estimates in our results section below.

We present empirical evidence that health care utilization and health outcomes do not predict the timing of law passage in states that adopt full NP independence in the Appendix. The key implication is that many groups have competing interests in scope-of-practice legislation, and the outcome of a legislative session is a function of political connections and fundraising efforts rather than data on public health or state budgetary issues. Our findings are consistent with Safriet (2002), Institute of Medicine (2010), and Isaacs and Jellinek (2013). We thus treat the timing of state law changes as exogenous to health care utilization and health outcomes and offer additional tests of the validity of this assumption below.

4 Empirical Approach and Results

We use an event study to examine how NP full independence affects health care utilization and health outcomes. We allow the effects of NP independence to differ across years and include pre-law passage estimates as a falsification test. Our baseline statistical model is

$$y_{ist} = \beta_0 + \sum_{k=-5}^{-1} \tau_k \cdot 1(t - T_s = k) + \sum_{k=1}^{11} \eta_k \cdot 1(t - T_s = k) + \gamma \cdot X_{it} + \alpha_t + \alpha_s + \epsilon_{ist} \qquad (1)$$

where y_{ist} is our outcome of interest for individual i in state s in year t, T_s is the year in which state s first allows NPs full independence, X_{it} are individual level control variables, and α_t and α_s are year and state fixed effects. We control for age, race, health insurance status, ethnicity, gender, living in an urban area, employment status, marital status, education level, and income.[14] All results use the MEPS sampling weights. The coefficients τ_k and η_k estimate the evolution of the effects of NP independence over time relative to the year of law passage. We include observations from states that never allow full NP independence in the control group. We group the event study dummies into two year intervals to improve the precision of our estimates. To estimate long run effects, all available data 11 years or more after the law change are considered part of the "year 11" treatment. Similarly, all data 5 years or more before the law change are part of the "year -5" treatment. When our dependent variable is binary, our regression is a logit model and reported coefficients are average marginal effects.[15] We cluster standard errors at the state level.

4.1 Preventive Care Utilization and Care Quality

We first estimate the effect of NP independence on the probability that an individual has had a routine checkup in the last 12 months. Increased utilization of yearly checkups is an important mechanism for NP independence to affect health outcomes. We split the sample into adults and children based on a cutoff at age 18, and Table 3 reports average marginal

[14] Hoynes and Schanzenbach (2009), among others, also include linear time trends based on historical state characteristics. In the Appendix, we show that our results are qualitatively robust to the inclusion of state-specific trends.

[15] As a robustness check, we also estimate these specifications using a linear probability model. All results are both qualitatively and quantitatively similar under this alternative functional form assumption.

Table 3: NP Independence Effect on Probability of Routine Checkup in Last 12 Months

	All states		Restricted: +/- 4 years		Law changing states only	
Years relative to	(1)	(2)	(3)	(4)	(5)	(6)
full independence	Adults	Children	Adults	Children	Adults	Children
5+ years before	-0.000946	0.0192			0.0180	0.0752
	(0.0306)	(0.0627)			(0.0345)	(0.0644)
3-4 years before	0.0000638	-0.0160	0.0182	-0.0147	0.0269*	0.0343
	(0.0124)	(0.0442)	(0.0154)	(0.0316)	(0.0153)	(0.0451)
1-2 years before	0.00905	-0.0277	0.0429	0.0255	0.0204	0.00258
	(0.0218)	(0.0438)	(0.0371)	(0.0819)	(0.0234)	(0.0390)
1-2 years after	0.0312*	-0.0262	0.0466***	-0.0327**	0.0281	-0.0133
	(0.0161)	(0.0166)	(0.00952)	(0.0157)	(0.0202)	(0.0177)
3-4 years after	0.0502***	-0.0182	0.0577***	-0.0396***	0.0493**	0.0123
	(0.0152)	(0.0181)	(0.0112)	(0.0142)	(0.0194)	(0.0170)
5-6 years after	0.0370**	-0.0500			0.0414*	-0.0193
	(0.0160)	(0.0392)			(0.0215)	(0.0391)
7-8 years after	0.0539**	-0.00771			0.0491*	0.00280
	(0.0212)	(0.0426)			(0.0246)	(0.0429)
9-10 years after	0.0613***	-0.0336			0.0415	-0.0442
	(0.0203)	(0.0276)			(0.0256)	(0.0286)
11+ years after	0.0742***	0.0162			0.0534	-0.00973
	(0.0192)	(0.0380)			(0.0307)	(0.0481)
Ever-Passed	-0.0107	-0.00584				
	(0.0209)	(0.0312)				
Observations	240,887	111,348	212,994	98,858	34,113	15,449

Regressions are weighted estimates of equation (1) using sampling weights provided in MEPS data. All columns are logit regressions and reported coefficients are average marginal effects. Dependent variable is listed at top and described in text. *, **, *** denote statistical significance at the 10%, 5%, and 1% levels respectively. Standard errors in parentheses are Huber-White robust estimates clustered at the state level. Ever-Passed reports the coefficient on a dummy variable for whether a state ever passed NP independence in a specification of equation (1) but without state fixed effects.

effects. For both adults and children, our pre-treatment dummies have small and statistically insignificant effects as desired. We find that the probability that an adult has had a checkup in the last year increases by 3.1 percentage points in the two years immediately after NP full independence. This effect rises to 7.4 percentage points after year 11, and the difference between the initial effect and the long run effect is statistically significant at the 1% level. We do not find evidence of an increase in the likelihood of a yearly checkup for children.

We present two specification checks of our main findings. Columns (3) and (4) use a balanced panel of states, where treated states are included in the sample only if there are at least 4 years of data available before and after the law change. None of the coefficients in columns (3) and (4) are statistically distinct from those in columns (1) and (2), indicating that limiting our sample to only states that appear in each period of the event study does not change our results. Columns (5) and (6) use a sample restricted to only states that

Table 4: Short and Long Run Treatment Effects

Dependent Variable	Baseline	LR Treatment	LR Treatment/Baseline	SR Treatment	SR Treatment/LR Treatment
Adult Checkup	0.66	0.0742	11.24%	0.0312	42.05%
Adult Appt. When Wanted	0.49	0.0747	15.24%	0.0465	62.25%
Adult Care When Sick	0.58	0.0933	16.09%	0.0576	61.74%
Adult Easy to Get to Provider	0.68	0.135	19.85%	0.0583	43.19%
Adult Health Care Rating	0.49	0.0546	11.14%	0.0515	94.32%
Adult Enough Time	0.49	0.0774	15.80%	0.0416	53.75%
Adult Listen Carefully	0.59	0.0698	11.83%	0.0609	87.25%
Adult Explained Clearly	0.60	0.0625	10.42%	0.0439	70.24%
Child Checkup	0.68	0.0162	2.38%	-0.0262	-161.73%
Child Appt. When Wanted	0.71	0.187	26.34%	0.0669	35.78%
Child Care When Sick	0.78	0.114	14.62%	0.126	110.53%
Child Easy to Get to Provider	0.70	0.159	22.71%	0.117	73.58%
Child Enough Time	0.70	0.160	22.86%	0.0773	48.31%
Child Listen Carefully	0.75	0.134	17.87%	0.0641	47.84%
Child Explained Clearly	0.77	0.115	14.94%	0.0695	60.43%

Dependent variables as defined in text. Baseline is the weighted sample mean of each dependent variable for pooled MEPS data from 1996-2011. LR Treatment is the coefficient on the dummy for 11+ years since the law change. SR Treatment is the coefficient on the dummy for 1-2 years since the law change.

ever allow NP independence, removing states that never change their laws from the control group. This eliminates any possible differential evolution of checkup probabilities in states that never change their laws. The results are very similar to those of columns (1) and (2), though with larger standard errors. Both of these checks demonstrate that our estimates vary little across specifications, so we present results using all available observations.

Table 4 shows the magnitude of the estimated 7.4 percentage point long run effect relative to the baseline rate of adults getting yearly checkups. Over 1996-2011, 66% of adults report having a checkup in the last year. The long run treatment effect therefore represents a 11.2% increase in the number of adults receiving yearly checkups. As noted above, states requiring NPs to be supervised by physicians often require 10% or more of the physician's and NP's time to be spent on direct supervision, consultations, or reviewing charts. Removing these requirements frees up more time for both physicians and NPs to see additional patients, so we believe that our estimated 11.2% increase in adults receiving yearly checkups is reasonable.

Table 3 presents treatment-on-the-treated estimates of the impact of NP independence on primary care utilization. We assume that states that allow NP full independence would have otherwise had no differential change in checkup frequency relative to states that do not. The lack of pre-NP independence differences between states that change laws and states that do not is suggestive evidence in favor of this parallel trends assumption. We are also interested in the expected effects of NP independence in a state that does not yet have NP independence, requiring an estimate of the average treatment effect of the law. If checkup frequency is uncorrelated with both the observables and unobservables of states that allow NPs full independence, this suggests that treatment status is random conditional on our covariates. To test this, we replace the state fixed effects in equation (1) with a dummy variable for whether a state ever allows NP full independence and report the coefficient at the bottom of columns (1) and (2) of Table 3. The statistical insignificance and small magnitude of this coefficient shows that there are no level differences in checkup frequency between states that ever allow NP independence and states that never do. We find no evidence that observable or unobservable differences between states that allow NP full independence and states that do not are correlated with health care utilization. This supports the assumption that the effect of NP independence in states that pass the law is comparable to the expected effect of NP independence in all states, so our treatment-on-the-treated estimates can be interpreted as average treatment effects. New states allowing NP full independence are likely to see increases in primary care usage similar to our estimates in Table 3.

NP independence may increase checkup frequency by reducing indirect costs to the patient through lower travel times or more convenient appointment scheduling. Lower travel costs are consistent with geographic spread of medical providers in response to NP independence, while better availability of appointments is consistent with less time spent on supervision and consultations between physicians and NPs. We estimate these effects using patient responses to questions about the availability of an appointment when one is wanted, the availability of appointment when the patient is sick, and whether it is difficult to travel to a provider. We code these responses as 1 if the patient reports "always" being able to get an appointment when wanted or when sick and 0 otherwise, and 1 if travel to a patient's usual source of care is "not at all difficult" and 0 otherwise.[16] Table 5 presents results for both adults and children. We find that all of these measures increase when NPs gain full independence, with more individuals reporting lower indirect costs. Table 4 shows that the long run effect magnitudes are economically significant, with 15-20% increases in these measures for adults and 15-26% increases for children relative to their baseline values.

[16] These responses correspond to the most favorable category available for each question.

Table 5: NP Independence Effect on Appointment Availability and Travel Costs

	Adults			Children		
Years relative to full independence	(1) Appt. when wanted	(2) Care when sick	(3) Travel	(4) Appt. when wanted	(5) Care when sick	(6) Travel
5+ years before	-0.0235	0.00735	0.0629	0.0448	0.0837	0.0668
	(0.0245)	(0.0174)	(0.0413)	(0.0979)	(0.117)	(0.0791)
3-4 years before	-0.00297	0.00900	0.0307	0.0157	0.0995	0.0193
	(0.0275)	(0.0346)	(0.0519)	(0.128)	(0.116)	(0.0689)
1-2 years before	-0.0374***	0.0361	0.00597	0.00717	0.0566	0.0612*
	(0.0108)	(0.0222)	(0.0186)	(0.0836)	(0.0790)	(0.0339)
1-2 years after	0.0465	0.0576**	0.0583***	0.0669***	0.126**	0.117***
	(0.0316)	(0.0226)	(0.0199)	(0.0128)	(0.0512)	(0.0448)
3-4 years after	0.0413	0.0607**	0.0979***	0.117***	0.0761	0.0925
	(0.0325)	(0.0299)	(0.0252)	(0.0237)	(0.0625)	(0.0600)
5-6 years after	0.0719**	0.0871***	0.0435**	0.112***	0.111	0.0793
	(0.0319)	(0.0316)	(0.0185)	(0.0403)	(0.0685)	(0.0513)
7-8 years after	0.0542	0.0947***	0.0354	0.0970***	0.0451	0.0527
	(0.0431)	(0.0233)	(0.0229)	(0.0204)	(0.0305)	(0.0452)
9-10 years after	0.0928*	0.0790**	0.111**	0.133***	0.0932*	0.139**
	(0.0496)	(0.0390)	(0.0502)	(0.0297)	(0.0482)	(0.0596)
11+ years after	0.0747	0.0933*	0.135***	0.187***	0.114**	0.159***
	(0.0498)	(0.0478)	(0.0488)	(0.0306)	(0.0491)	(0.0505)
Observations	146,933	69,776	225,757	69,762	23,678	94,755

Regressions are weighted estimates of equation (1) using sampling weights provided in MEPS data. All columns are logit regressions and reported coefficients are average marginal effects. Dependent variable is listed at top and described in text. *, **, *** denote statistical significance at the 10%, 5%, and 1% levels respectively. Standard errors in parentheses are Huber-White robust estimates clustered at the state level.

NP independence can lead to changes in patient health outcomes through channels other than checkup frequency. If checkups increase in quality, then even individuals who do not receive more frequent primary care after NP independence may have better health. We first use a survey question that asks adults to rate their health care quality over the past year on a ten point scale. We code responses as 1 if an adult rates health care quality as a 9 or greater and 0 otherwise.[17] Column (1) of Table 6 shows that adults report a higher level of overall health care quality after NP independence, and Table 4 indicates that the long run effect represents a 11% increase in the number of adults rating their health care as excellent.

To determine what drives this perceived increase in health care quality, we look for changes in visit quality using questions on whether the provider spent enough time with the patient, whether the patient felt that the provider listened to their concerns, and whether the provider explained things in a way that the patient understood. We code responses as 1 if the patient answers “always” and 0 otherwise and present results in columns (2)-(7) of Table

[17]As the other questions about visit quality are rated on a scale of 1-4, we code reports of 9 or 10 on a scale of 0 to 10 as 1 for consistency with the other measures.

Table 6: NP Independence Effect on Patient-Reported Visit Quality

	Adults				Children		
Years relative to full independence	(1) Health Care Rating	(2) Enough Time	(3) Listen Carefully	(4) Explained Clearly	(5) Enough Time	(6) Listen Carefully	(7) Explained Clearly
5+ years before	0.0212	0.0266	0.0204	0.0318**	0.0804	0.0906*	0.0823
	(0.0203)	(0.0239)	(0.0146)	(0.0148)	(0.0609)	(0.0548)	(0.0675)
3-4 years before	0.0168	0.0273	0.0244	0.0214	-0.0124	0.0220	0.0114
	(0.0125)	(0.0294)	(0.0238)	(0.0208)	(0.0688)	(0.0735)	(0.0812)
1-2 years before	0.00940	0.0130	0.00699	-0.00737	0.0481	0.0509	0.0260
	(0.0161)	(0.0228)	(0.0148)	(0.0176)	(0.0510)	(0.0738)	(0.0726)
1-2 years after	0.0515***	0.0416	0.0609***	0.0439	0.0773***	0.0641***	0.0695**
	(0.0187)	(0.0280)	(0.0157)	(0.0303)	(0.0193)	(0.0104)	(0.0338)
3-4 years after	0.0440**	0.0712***	0.0805***	0.0486	0.110***	0.0923***	0.0846***
	(0.0184)	(0.0180)	(0.0152)	(0.0312)	(0.0272)	(0.0233)	(0.0287)
5-6 years after	0.0717**	0.0822***	0.0650***	0.0558*	0.0585*	0.0650**	0.0510*
	(0.0300)	(0.0270)	(0.0177)	(0.0336)	(0.0298)	(0.0289)	(0.0285)
7-8 years after	0.0589**	0.0499	0.0463**	0.0579	0.0311	0.0341*	0.0267
	(0.0261)	(0.0358)	(0.0225)	(0.0434)	(0.0225)	(0.0193)	(0.0350)
9-10 years after	0.0721***	0.0768*	0.0660**	0.0640	0.120***	0.0949***	0.107***
	(0.0260)	(0.0460)	(0.0317)	(0.0464)	(0.0259)	(0.0169)	(0.0306)
11+ years after	0.0546*	0.0774	0.0698***	0.0625	0.160***	0.134***	0.115***
	(0.0322)	(0.0498)	(0.0255)	(0.0423)	(0.0328)	(0.0242)	(0.0435)
Observations	165,762	165,998	165,598	166,191	80,904	80,830	80,903

Regressions are weighted estimates of equation (1) using sampling weights provided in MEPS data. All columns are logit regressions and reported coefficients are average marginal effects. Dependent variable is listed at top and described in text. *, **, *** denote statistical significance at the 10%, 5%, and 1% levels respectively. Standard errors in parentheses are Huber-White robust estimates clustered at the state level.

6 with magnitudes of long run effects in Table 4. We find that patients consistently report visits to be of higher quality in the years following NP full independence, with increases ranging from 10-16% for adults to 15-23% for children relative to their baseline values.[18]

We summarize our results in Figures 1 and 2. These graphs report the coefficients of the treatment dummies in the logit regressions of Tables 5 and 6, so effects are measured in percentage point increases in the given dependent variable. The patterns of coefficients support our research design, as pre-NP independence dummy variables are generally statistically insignificant and have smaller point estimates than post-NP independence dummy variables.[19] Most measures show a sharp increase in the years immediately following NP independence. The patterns also highlight the strength of the event study design, as the pre-treatment years serve as placebo tests of the effect of NP independence and offer evidence that the timing of NP independence is exogenous to health care concerns.

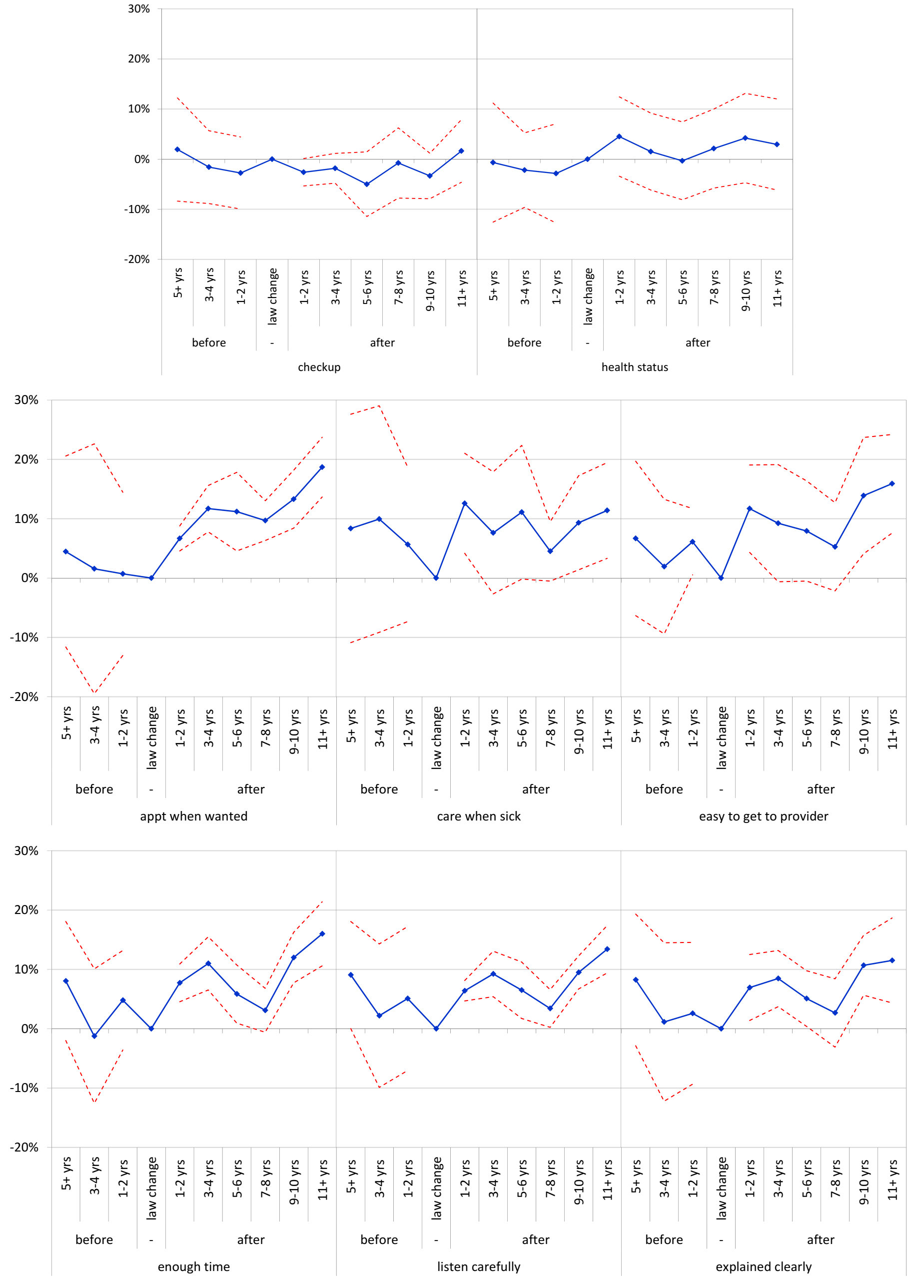
[18]Parents answer questions about the quality of health care received by their children.

[19]Note that while a few of our pre-treatment dummies are statistically significant, the number of significant point estimates is consistent with expected rates of Type I error.

Figure 1: Event Study Results: Adults

Graphs plot average marginal effects from event study logit regressions with dependent variable listed at bottom of graph. First panel depicts results from column (1) of Table 3, column (1) of Table 7, and column (1) of Table 6; second panel depicts results from columns (1)-(3) of Table5; third panel depicts results from columns (2)-(4) of Table 6. Blue lines indicate point estimates of coefficients, and red dashed lines indicate 90% confidence intervals of estimates.

Figure 2: Event Study Results: Children



Graphs plot average marginal effects from event study logit regressions with dependent variable listed at bottom of graph. First panel depicts results from column (2) of Table 3 and column (2) of Table 7; second panel depicts results from columns (4)-(6) of Table5; third panel depicts results from columns (5)-(7) of Table 6. Blue lines indicate point estimates of coefficients, and red dashed lines indicate 90% confidence intervals of estimates.

4.2 Changes in Health Outcomes

As NP full independence leads to increases in health care utilization and patient-reported care quality, we now look for resulting improvements in population health. We first examine patient self-reported health status on a 1-5 scale, with 5 representing "excellent" health.[20] We estimate the probability that an individual reports being in excellent health and present results in columns (1) and (2) of Table 7. Our results show that adults have higher self-reported health status after NP independence. While our point estimates for children are also generally positive, they are too imprecise to conclude that children's health status improves.

We also test whether the additional preventive care consumed after NP independence leads to better management of chronic conditions. The MEPS Medical Conditions data files report the number of emergency room visits related to each medical condition an individual has. For many conditions, we would not expect preventive care to affect condition severity. We focus on ambulatory care sensitive (ACS) conditions, identified by Billings et al. (1993, p. 163) as "diagnoses for which timely and effective outpatient care can help . . . by either preventing the onset of an illness or condition, controlling an acute episodic illness or condition, or managing a chronic disease or condition."[21] Because the distribution of emergency room visits for each condition is skewed towards 0, we estimate extensive and intensive margin effects in separate regressions using a dummy variable for any emergency room visit and the natural log of the number of visits as dependent variables. Columns (3) and (4) of Table 7 report results. We find consistently negative but imprecisely estimated effects of NP independence on the likelihood of any emergency room visit for ACS conditions, with small and insignificant effects on the likelihood of an emergency room visit for non-ACS conditions. On the intensive margin, we find a 21.7% reduction in the number of emergency room visits for ACS conditions in the long run after NP independence with no corresponding effect on non-ACS conditions. Point estimates for non-ACS conditions are much smaller than those for the ACS conditions, indicating that NP independence has a greater effect on the severity of ACS conditions than non-ACS conditions.[22] The mean number of emergency

[20] MEPS contains several self-reported health status variables. We use the report in the second and fourth wave of each panel to avoid the same individual giving two reports in a calendar year.

[21] We report the full list of ACS conditions in Table A.1 in the Appendix.

[22] Individuals may go to the emergency room less when NPs have independence because they have ACS

Table 7: NP Independence Effect on Health Outcomes

Years relative to full independence	Health Status (1) Adults	Health Status (2) Children	Medical Conditions (3) Any ER Visit	Medical Conditions (4) ln(ER visits)
5+ years before	0.00357	-0.00685		
	(0.0205)	(0.0722)		
3-4 years before	0.0463***	-0.0218		
	(0.0158)	(0.0452)		
1-2 years before	0.0232	-0.0285		
	(0.0201)	(0.0602)		
1-2 years after	0.0469**	0.0451		
	(0.0204)	(0.0482)		
3-4 years after	0.0466***	0.0150		
	(0.0156)	(0.0467)		
5-6 years after	0.0428**	-0.00348		
	(0.0179)	(0.0472)		
7-8 years after	0.0678***	0.0211		
	(0.0187)	(0.0480)		
9-10 years after	0.0309*	0.0420		
	(0.0182)	(0.0542)		
11+ years after	0.0555***	0.0292		
	(0.0181)	(0.0552)		
5+ years before x ACS condition			-0.00431	-0.0485
			(0.00875)	(0.0617)
3-4 years before x ACS condition			-0.00203	-0.107
			(0.0100)	(0.0744)
1-2 years before x ACS condition			0.00660	-0.0889
			(0.00815)	(0.0646)
1-2 years after x ACS condition			0.00231	-0.175**
			(0.00634)	(0.0759)
3-4 years after x ACS condition			-0.00611	-0.132*
			(0.00887)	(0.0661)
5-6 years after x ACS condition			-0.00801	-0.157**
			(0.0102)	(0.0758)
7-8 years after x ACS condition			-0.00844	-0.167*
			(0.00840)	(0.0849)
9-10 years after x ACS condition			-0.0148*	-0.146*
			(0.00869)	(0.0741)
11+ years after x ACS condition			-0.0100	-0.217**
			(0.0133)	(0.0985)
5+ years before x non-ACS condition			-0.00756	-0.0800***
			(0.00538)	(0.0256)
3-4 years before x non-ACS condition			-0.00859	-0.00216
			(0.00622)	(0.0277)
1-2 years before x non-ACS condition			-0.00524	-0.0234
			(0.00588)	(0.0272)
1-2 years after x non-ACS condition			-0.00803	-0.0427**
			(0.00849)	(0.0193)
3-4 years after x non-ACS condition			0.00607	0.0122
			(0.00655)	(0.0234)
5-6 years after x non-ACS condition			0.00320	0.0133
			(0.00655)	(0.0393)
7-8 years after x non-ACS condition			0.00220	0.0145
			(0.00924)	(0.0256)
9-10 years after x non-ACS condition			0.00316	0.00311
			(0.00761)	(0.0274)
11+ years after x non-ACS condition			0.000243	0.0437
			(0.0100)	(0.0338)
Observations	352,224	123,481	1,257,000	62,717

Regressions are weighted estimates of equation (1) using sampling weights provided in MEPS data. Columns (1)-(3) are logit regressions and reported coefficients are average marginal effects. Column (4) is an OLS regression. Dependent variable is listed at top and described in text. *, **, *** denote statistical significance at the 10%, 5%, and 1% levels respectively. Standard errors in parentheses are Huber-White robust estimates clustered at the state level.

room visits in a year for conditions with non-zero visits is 1.17 in our sample, so a 21.7% reduction represents 0.25 fewer emergency room visits per ACS condition per year.

There are nearly 27 million emergency room ACS condition-visits in the MEPS data over the period 1996-2011. The median total cost of an emergency room visit for which only ACS conditions are listed as causes is $339 per condition. Assuming that 0.25 visits per ACS condition can be eliminated through NP independence yields an estimated cost savings of approximately $142 million per year. This is an upper bound on cost savings because we do not consider expenditures on additional primary care. The median total cost for a office-based visit is $284 per ACS condition, so assuming that one primary care visit eliminates one emergency room visit still yields cost savings of over $23 million per year. This lower bound estimate of savings arises simply from changes in the site of care in response to NP full independence. Overall, our results show improvements in health outcomes and reductions in the costs of providing care when NPs have full independence.

4.3 Heterogeneous Effects of NP Independence

The propensity to consume primary care when more becomes available may vary based on individual characteristics such as age, gender, location, or insurance status. We therefore investigate potential heterogeneity in the response to NP full independence of the probability that an adult has had a checkup in the previous 12 months. We modify equation (1) to include interactions between the treatment effects and indicators for the individual characteristics of interest, and we test for heterogeneity of response across the two groups with F-tests of the difference in estimated coefficients.

Overall, we find little evidence of heterogeneity in the effect of NP independence. Figure 3 shows that responses to NP independence are similar among adults above and below 65 years of age, men and women, and those with and without a bachelor's degree. Figure 4 divides the sample geographically, with urban/rural results based on an individual's county

conditions that become less severe or because the greater availability of primary care leads patients to substitute office-based care for emergency room care. If individuals are substituting, we would expect to find a reduction in emergency room visits regardless of condition. Our results for non-ACS conditions indicate that the degree of substitution of primary care visits for emergency room visits across all medical conditions is likely small, though we cannot rule out the possibility that individuals substitute only for ACS conditions.

classification in the 1993 USDA Economic Research Service Rural-Urban Continuum Code and MSA/non-MSA results based on whether MEPS reports that an individual lives in a Metropolitan Statistical Area.[23] Despite large standard errors for the rural estimates, point estimates are similar for the two groups in both regressions. Rural areas do not consume disproportionately more primary care after NP independence, suggesting that providers do not move in significant numbers from urban to rural counties or from MSAs to non-MSAs when NPs have full independence. This is consistent with our results on travel costs decreasing after NP independence if providers relocate within rural and urban areas.

Figure 5 shows that NP independence has comparable effects on checkup frequency for those with and without insurance and those with private insurance compared to those on Medicaid. We find similar results when comparing the privately insured against those with any form of public insurance.[24] Overall, the uniformity of the effect of NP independence across groups is striking and suggestive of a broad mechanism driving these treatment effects, such as a reduction in the administrative burden of supervision. We explore potential mechanisms further below.

5 Mechanisms, Policy Effects, and Sensitivity Analysis

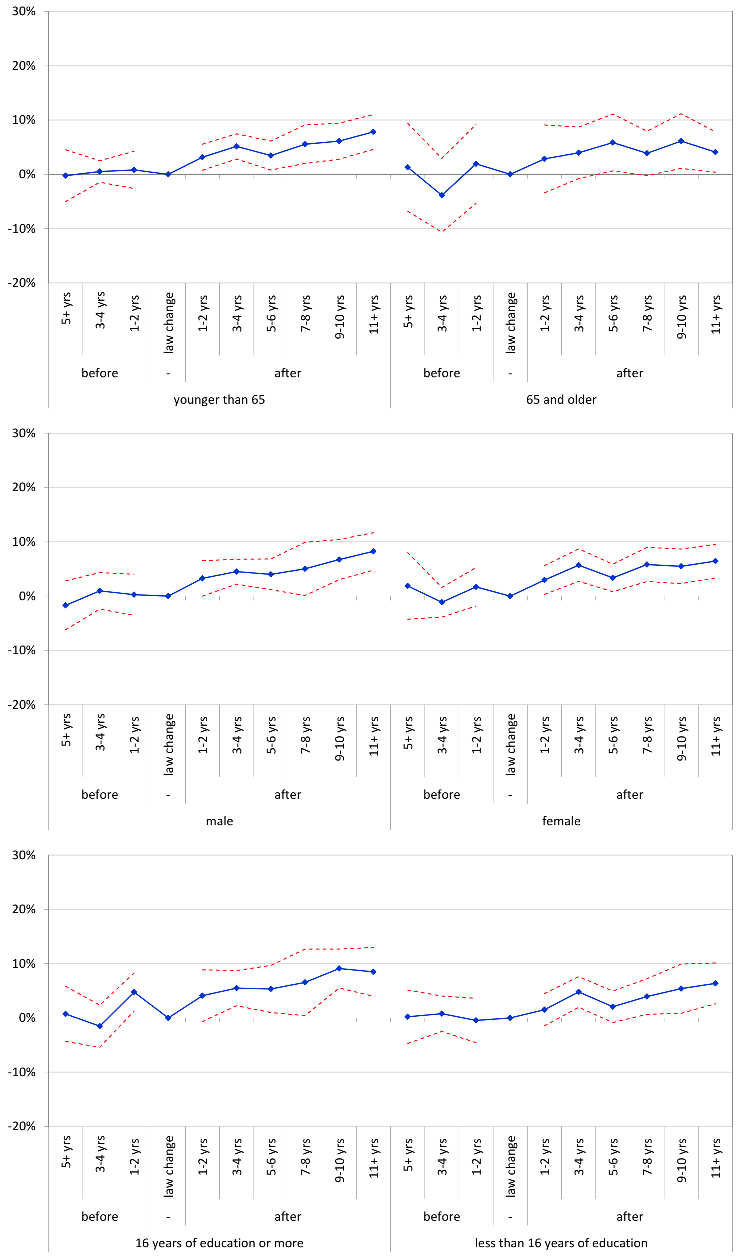
5.1 Changes in NP Supply

Kalist and Spurr (2004) suggest that the supply of NPs rises when NPs face fewer regulations. Since becoming an NP requires several years even for current registered nurses, our event study suggests a natural test of the relative importance of increases in NP supply and the removal of supervision requirements on health care utilization and health outcomes: we compare the post-NP independence treatment effect of the first period (1-2 years after law passage) to the last (11+ years after law passage). We assume that the required training time

[23] We consider a county rural if it is non-metropolitan and not adjacent to a metropolitan area. This corresponds to codes 5, 7, and 9 in the 1993 USDA ERS Continuum.

[24] Across all specifications, only three estimated effects of NP independence are statistically significantly different across groups. Most notably, the effect 11+ years after law passage is different for adults under or over 65 years of age and those with private insurance instead of public health insurance. In both cases, those who are not Medicare eligible show a slightly larger increase in utilization of regular checkups, though all other treatment effects are indistinguishable. The lack of significant differences in our other insurance regressions indicate this is likely a function of a high baseline rate of yearly checkups for the elderly.

Figure 3: Heterogeneous Effects of NP Independence on Checkup Frequency: Demographics

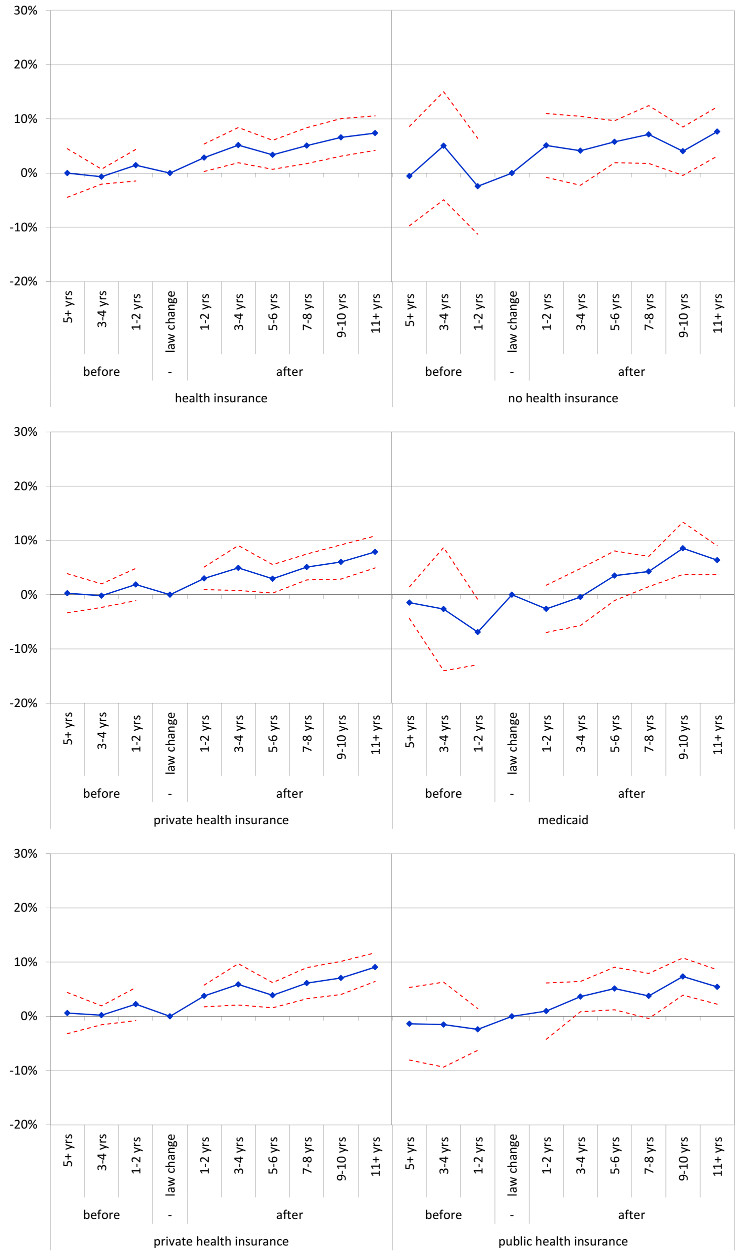


Graphs plot average marginal effects from event study logit regressions with dependent variable indicating if individual has had a routine checkup in past 12 months. Regression shown in first panel includes interactions of treatment effect dummies with indicators for whether individual is under/over age of 65; second panel, indicators for male/female; third panel, indicators for more/less than 16 years of education. Blue lines indicate point estimates of coefficients, and red dashed lines indicate 90% confidence intervals of estimates.

Figure 4: Heterogeneous Effects of NP Independence on Checkup Frequency: Geography

Graphs plot average marginal effects from event study logit regressions with dependent variable indicating if individual has had a routine checkup in past 12 months. Regression shown in first panel includes interactions of treatment effect dummies with indicators for whether individual lives in an urban/rural county according to USDA Economic Research Service 1993 Rural-Urban Continuum Code; second panel, indicators for whether individual lives in a metropolitan statistical area or not. Blue lines indicate point estimates of coefficients, and red dashed lines indicate 90% confidence intervals of estimates.

Figure 5: Heterogeneous Effects of NP Independence on Checkup Frequency: Insurance Status



Graphs plot average marginal effects from event study logit regressions with dependent variable indicating if individual has had a routine checkup in past 12 months. Regression shown in first panel includes interactions of treatment effect dummies with indicators for whether individual has any form of health insurance/no health insurance; second panel, indicators for private health insurance/Medicaid; third panel, indicators for private health insurance/Medicaid or Medicare. Blue lines indicate point estimates of coefficients, and red dashed lines indicate 90% confidence intervals of estimates.

is long enough that any increased supply effects are negligible within 1-2 years of NPs gaining independence, while estimates from 11+ years after NP independence allow sufficient time for any transitional effects in the supply of NPs to diminish.[25] Table 4 reports the percentage of the estimated long run effect represented by the estimated short run effect for each of our utilization, access, and quality measures. We find that short run effects account for 42-94% of long run effects for adults and 36-111% for children, with long run increases in NP supply having a relatively larger effect on checkup frequency than access or quality measures. We interpret this as evidence that increasing NP supply is not the primary channel through which NP independence affects health care, since immediate effects of the law such as the removal of administrative burdens explain a large and economically significant proportion of the observed effects. This is consistent with the finding of Stange (2014) that differences in NP supply have small effects on health care markets.

5.2 Changes in Primary Care Physician Labor Supply

One potential consequence of NP full independence is a change in the labor supply of physicians. If patient care hours worked by primary care physicians do not change after NP full independence, then observed increases in health care utilization and outcomes must come from greater use of NP services. If physician patient care hours increase, then the effects could arise from more appointments with NPs and physicians, as less time spent on supervision frees more time for both NPs and physicians to use on patient care.[26]

We first check if NP independence changes the ratio of primary care physicians per person. We construct the number of physician general practitioners in primary care per 10,000 total population for every available state and year from 1995-2008 using the Area Resource File of the Department of Health and Human Services. We enter this ratio as the dependent variable in an event study specification similar to equation (1) but include only event study dummies and state and year fixed effects. Column (1) of Table 8 shows that this ratio does

[25]As we estimate the direct impact of changes in NP regulations, we account for any increases in utilization or health outcomes caused by additional NP supply if the supply increase is caused by changes in NP regulations, so this possibility does not affect the internal validity of our estimates.

[26]As above, our estimation strategy accounts for these changes if they occur because of changes in NP regulations, and so they do not affect the internal validity of our estimates.

Table 8: Mechanisms, Policy Effects, and Sensitivity Analysis

	Physician Responses			HPSA Status		Prescriptive Authority	Placebo Tests			
Years relative to full independence	(1) GP/Pop Ratio	(2) ln(Care Hours)	(3) ln(Med. Hours)	(4) Checkup last year	(5) Checkup last year	(6) Checkup last year	(7) Dental checkup last year	(8) ln(Dental visits)	(9) Any Opto. visit	(10) ln(Opto. visits)
5+ years before	0.628	0.0248	-0.0415	0.00740	0.00984	-0.00185	-0.0295	0.00450	0.0105	-0.0800
	(0.868)	(0.0397)	(0.0468)	(0.0553)	(0.0545)	(0.0312)	(0.0201)	(0.0288)	(0.0214)	(0.0571)
3-4 years before	1.447	0.0126	-0.0492	-0.0275	-0.0250	-0.00139	-0.0170	0.00613	-0.00749	-0.0512
	(1.068)	(0.0409)	(0.0472)	(0.0186)	(0.0166)	(0.0127)	(0.0209)	(0.0503)	(0.0129)	(0.0391)
1-2 years before	0.215	0.0464	-0.0369	-0.0256	-0.0225	0.00813	0.0354**	0.0464*	0.00990	-0.0695*
	(0.517)	(0.0367)	(0.0469)	(0.0378)	(0.0358)	(0.0223)	(0.0167)	(0.0244)	(0.0137)	(0.0406)
1-2 years after	0.184	0.0459	-0.0282	-0.00460	0.000933	0.0429***	-0.0154	0.00936	0.00424	0.00952
	(0.416)	(0.0427)	(0.0476)	(0.0349)	(0.0303)	(0.00612)	(0.0164)	(0.0536)	(0.0164)	(0.0465)
3-4 years after	-0.0296	0.0647**	0.00130	0.0588*	0.0659**	0.0768***	0.0175	0.00314	0.0180	-0.0632
	(0.640)	(0.0303)	(0.0397)	(0.0338)	(0.0300)	(0.00641)	(0.0158)	(0.0373)	(0.0167)	(0.0426)
5-6 years after	0.136	0.0294	-0.0382	-0.0252	-0.0271	0.0314***	0.0183	0.0533	0.00915	-0.0328
	(0.963)	(0.0329)	(0.0455)	(0.0415)	(0.0389)	(0.0112)	(0.0256)	(0.0360)	(0.0182)	(0.0442)
7-8 years after	-0.199	0.0864***	0.00509	-0.0688**	-0.0752***	0.0432**	0.0259	0.0517	0.00644	0.0319
	(1.201)	(0.0312)	(0.0457)	(0.0346)	(0.0288)	(0.0168)	(0.0168)	(0.0398)	(0.0185)	(0.0515)
9-10 years after	0.423	0.119*	0.0645	-0.0314	-0.0379	0.0499***	0.0225	0.0185	0.0181	0.0257
	(1.430)	(0.0691)	(0.0575)	(0.0367)	(0.0324)	(0.00766)	(0.0280)	(0.0389)	(0.0235)	(0.0409)
11+ years after	2.338	0.0802***	0.0259	0.00860	0.00279	0.0750***	0.0272	0.0369	0.0137	0.0125
	(1.444)	(0.0208)	(0.0297)	(0.0350)	(0.0313)	(0.00856)	(0.0315)	(0.0395)	(0.0219)	(0.0584)
5+ years before x HPSA ratio				-0.281	-0.415					
				(0.448)	(0.668)					
3-4 years before x HPSA ratio				0.214*	0.301*					
				(0.110)	(0.172)					
1-2 years before x HPSA ratio				0.302	0.423					
				(0.249)	(0.403)					
1-2 years after x HPSA ratio				0.276	0.365					
				(0.178)	(0.266)					
3-4 years after x HPSA ratio				-0.0753	-0.228					
				(0.172)	(0.268)					
5-6 years after x HPSA ratio				0.490*	0.821*					
				(0.269)	(0.431)					
7-8 years after x HPSA ratio				0.858***	1.489***					
				(0.206)	(0.269)					
9-10 years after x HPSA ratio				0.784***	1.378***					
				(0.206)	(0.313)					
11+ years after x HPSA ratio				0.667***	1.177***					
				(0.199)	(0.312)					
1-2 years after Rx authority						-0.0142				
						(0.0189)				
3-4 years after Rx authority						-0.0407***				
						(0.0148)				
5-6 years after Rx authority						0.0150				
						(0.0193)				
7-8 years after Rx authority						0.0232				
						(0.0227)				
9-10 years after Rx authority						0.0231				
						(0.0164)				
11+ years after Rx authority						0.00989				
						(0.0170)				
Observations	712	35,681	35,695	230,650	230,650	240,887	307,541	182,034	352,664	19,246
HPSA Ratio				Population	Underserved					
Data Source	ARF	CTS-HTS	CTS-HTS	MEPS	MEPS	MEPS	MEPS	MEPS	MEPS	MEPS

Regressions are estimates of equation (1) using data source listed at bottom. All regressions are weighted using provided sample weights in data. Columns (1), (2), (3), (8), and (10) are OLS regressions. Columns (4), (5), (6), (7), and (9) are logit regressions and reported coefficients are average marginal effects. Dependent variable is listed at top and described in text. *, **, *** denote statistical significance at the 10%, 5%, and 1% levels respectively. Standard errors in parentheses are Huber-White robust estimates clustered at the state level. "GP/Pop Ratio" is the number of general practice physicians in primary care divided by the state population in 10,000s.

not change significantly after NP full independence. The absence of a pre-law passage trend is consistent with our Appendix results, as a low supply of primary care physicians does not precede the law change. Thus, NP independence does not crowd out primary care physicians.

To determine if physicians increase their labor supply after NP independence, we use data on the average amount of time per week physicians spend providing direct patient care and on all medically related activities from the 5 available years of the Community Tracking Study and the Health Tracking Physician Survey over 1996-2008. We again use the regression framework of equation (1) and include controls for physician specialty and practice type, gender, age and age squared, board certification status, location in a MSA, and international medical school graduate status in addition to state and year fixed effects.[27] The results in column (2) of Table 8 show that in the long run, physicians increase patient care time by approximately 8% when NPs have full independence. This effect size is similar to the 10% statutory requirement for time spent supervising NPs. Importantly, the results in column (3) show no increase in hours spent on all medical activities. F-tests of the long run effects and the entire response path of patient care hours and all medical activity hours reject equality at the 1% level. Together, these results suggest that physicians substitute additional patient care hours for time spent supervising.

Unfortunately, we lack data to examine directly if NP labor supply changes after full independence. Nonetheless, an 8% increase in physician patient care hours is appropriate given our finding that adult checkups increase 11% in the long run, suggesting that both physicians and NPs see additional patients after NP independence. These results are consistent with removal of administrative costs of oversight as an important mechanism behind our main effects.

5.3 Primary Care Health Professional Shortage Areas

Whether NP independence benefits medically underserved populations is an important policy question. Health Professional Shortage Areas (HPSAs) are geographic areas with an insuf-

[27] We restrict the data to physicians who report working with other providers of any type, as there are not enough physicians who do not work with any other providers in the data to make estimates using this potential control group sufficiently precise. Results are qualitatively similar but more imprecise when we further restrict the sample to primary care physicians working with other providers.

ficient number of health care providers according to the Department of Health and Human Services. A primary care HPSA has a population to primary care physician ratio greater than 2000 to 1.[28] Low provider supply may lead to poor access to primary care services, so we expect that allowing NPs full independence may have a larger effect on utilization of health services when the supply of primary care physicians is lower.

We use two statewide measures of primary care physician shortage, the ratio of all people in primary care HPSAs to the state population and the ratio of the underserved population in primary care HPSAs to the state population.[29] As our MEPS data begins in 1996, we compute these ratios using 1995 data and exclude states that allow NP full independence before 1995 so that NP independence does not affect our measures.

We modify equation (1) by including interaction terms between the treatment effect and the 1995 HPSA ratio and present results in columns (4) and (5) of Table 8. We find that the increase in yearly checkups is larger in states with a higher population share underserved using both measures of the primary care physician supply shortage. Increased utilization of health care in response to NP independence is thus larger when there is plausibly more unmet demand for primary care services, indicating that our main results are driven by features of the primary care market. This result also suggests that NP full independence is an effective policy for alleviating primary care provider shortages.

5.4 Prescriptive Authority

We identify the effects of NP full independence based on the year in which a state permits it. However, Table 1 shows that in many states, the first year of NP full independence is the same year that NPs gain prescriptive authority. To determine whether prescriptive authority or full independence drives our results, we include treatment effects for years since NPs gained prescriptive authority in equation (1) and present results in column (6) of Table 8. The effect of prescriptive authority alone is identified by laws in Arizona and Wisconsin,

[28]Subpopulations within a geographic area can be individually designated as underserved. See http://www.hrsa.gov/shortage/ for more information.

[29]The underserved population is calculated by the Department of Health and Human Services by multiplying the number of primary care physicians in HPSAs by the target ratio (2000/1) and subtracting this number from the population in HPSAs.

and the timing of law passage allows us to estimate treatment effects only after prescriptive authority is granted. We find that prescriptive authority alone has no effect on the probability of an adult receiving a checkup, while estimates of the effect of full independence remain positive and statistically significant. This is consistent with removal of administrative costs as the main mechanism behind our observed main effect, as retaining physician oversight on NP practice does not lead to increased utilization. These results show that we identify the effect of full independence rather than just prescriptive authority and that both independent practice and prescriptive authority for NPs are necessary to increase health care utilization.

5.5 Placebo Tests: Utilization of Other Medical Services

To show that the estimated effects of NP independence are not biased by coincident increases in demand for all medical services, we use equation (1) to estimate the effect of NP independence on dental checkups and optometrist visits for adults. Results in columns (7)-(10) of Table 8 show no significant impact of NP independence on utilization of these services on either the intensive or extensive margin. This placebo test result also shows that NP independence is not coincident with state Medicaid expansions, as many SCHIP and Medicaid programs cover dental visits for both children and adults to varying degrees.[30]

6 Conclusion

Health care costs and the supply of primary care providers are critical issues of current and future health policy in the U.S. We show that population health care utilization and health outcomes increase and the indirect costs of primary care fall when the supply of providers rises, exploiting variation in scope-of-practice legislation for nurse practitioners. Our results indicate that allowing NPs to practice and prescribe drugs without physician oversight increases medical care for underserved populations and reduces emergency room use for conditions responsive to primary care. Previous estimates do not consider this source of cost savings, suggesting that NP full independence may reduce costs by more than 0.6

[30]See Shulman et al. (2004) and McGinn-Shapiro (2008) for information on Medicaid dental programs for children and adults.

to 1.3%. We show that these effects arise from removing the administrative burden of oversight and supervision from NPs and physicians and that removing this burden has a larger impact than long-term effects of the law change on primary care provider supply. Changing NP scope-of-practice laws increases the labor supply of the vast majority of primary care providers, making NP independence a powerful policy tool.

Our findings have implications for several health policy debates. The cost-effectiveness of acute medical care is low relative to primary care, and our analysis indicates that patients substitute towards primary care when it is more easily available. The yearly cost savings from this diversion alone is more than half of the Affordable Care Act's one-time allotment for training primary care providers and could be used to supplement these efforts on an ongoing basis.[31] We find that utilization and outcome gains from NP independence are widespread across the population, with larger gains in states with more medically underserved people. This suggests that NP independence accomplishes multiple policy goals, including short- and long-term health care improvements for rural or underserved populations.

Further research is needed to assess other supply side effects of changes in NP scope-of-practice laws, including changes in the quality of NPs caused by higher ability workers choosing to become NPs when NPs have greater freedoms. Increases in NP ability may allay physician group concerns about allowing NPs a wider scope-of-practice and increase the overall productivity of the health care sector, generating further cost savings. Kleiner et al. (2012) show that NPs and physicians are both complements and substitutes in the production of medical services, implying that changes in NP quality may impact the outcomes of services provided by physicians. Additional analysis of outcomes for individuals within primary care HPSAs, particularly HPSAs with differences in the degree of underprovision across populations, may provide more detailed evidence of heterogeneity of effects of increased primary care access. Finally, scope-of-practice laws govern many medical professions besides NPs. Isaacs and Jellinek (2013) list 9 other types of medical providers ranging from audiologists to psychologists and note that over 350 scope-of-practice bills affecting these groups were enacted in 2011 and 2012 alone. This paper is a first step in assessing this

[31] In 2010, the Affordable Care Act spent $250 million on training for primary care providers, mostly for NPs and physicians. See `http://www.ahrq.gov/research/findings/factsheets/primary/pcworkforce/pcworkforce.pdf` for more details.

approach for changing provider supply that may ultimately reshape our health care system.

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Appendix

Timing Regressions

We offer further evidence of the unpredictability of the timing of the adoption of NP full independence in the 18 states that ever allow it, supplementing our finding in many of our regressions of no difference in the pre-treatment years between states that allow NP full independence and states that do not. We use the state level covariates described in Section 3 to predict the timing of law passage. We first use the approach of Hoynes and Schanzenbach (2009) and Bailey (2012), selecting covariates in one pre-treatment year to predict the length of time between that year and the date of law passage. However, our state level data set has only 18 observations, which is too few to use all our covariates in a single meaningful regression. We therefore estimate separate regressions using data from 1970 only where each covariate is used as the sole explanatory variable and present results in column (1) of Table A.2.

Of these regressions, only medical benefits per 100,000 people has a statistically significant coefficient of 0.00266, suggesting that states with higher medical benefits take longer to allow NPs to practice independently. This contradicts the idea that states implement NP full independence in response to rising costs. Our small number of observations, however, leaves large standard errors for many of our regressions. We therefore also report R^2 values for each regression to determine which variable explains the largest amount of the existing variation across states in the timing of law passage. We find that medical benefits per 100,000 people has by far the most explanatory power of our covariates, with differing levels of medical benefits spending in 1970 accounting for 20.2% of the variation in the timing of when states allow NPs full independence. The predictive power of medical benefit spending does not appear to come from its correlation with utilization of either inpatient or outpatient services nor from its correlation with demographic characteristics or income, as the 1970 values of all of these variables explain less than 0.5% of variation in timing. The second most powerful predictor is whether a state has a Democratic governor in 1970, which explains 3.3% of variation in timing. We therefore do not find evidence that health care utilization or demand for health care, as measured by demographics and income, explain the timing of a state's decision to allow NP independence.

As an additional test of the power of these covariates to predict the timing of NP independence, we use state panel data over 1970-2008 and regress the covariates described above on the number of years remaining until the state allows full NP independence. We also estimate results on a shortened panel of 1980-2008 to insure that results are not greatly affected by having a long early period before any state allows full NP independence. While this approach allows states with later law passage dates to have greater weight in the data, this exercise maximizes the available information in our sample.

We present results of these panel regressions in columns (2)-(5) of Table A.2, with an additional regression in column (6) that limits the panel to the 20 years prior to law passage in each state to address the above sample concern. Given the correlations between many of our health-related explanatory variables, it is perhaps not surprising that the only variable consistently individually correlated with the number of years until NPs gain full independence is the party of the governor. We find that states with Democratic governors tend to wait more years before allowing NPs full independence.

Since individual statistical significance is likely affected by multicollinearity, we again look at R^2 as a measure of the overall power of all the variables in the regression when assessing the explanatory power of these covariates. The R^2 values of these regressions, ranging from 0.36 to 0.54, indicate that these variables capturing important features of the health care markets and political conditions in states jointly explain only a moderate amount of the variance in the timing of passage of NP independence laws. However, since the number of years remaining until law passage is a simple linear trend within each state, these R^2 values are likely inflated by time trends in the explanatory variables. A regression of years until law passage on only year fixed effects shows an R^2 of 0.436 in the panel using data from 1970 and 0.250 using data from 1980.[32] This result is consistent with the presence of common unobserved trends, as we would expect more inflation of the regression R^2 in the longer panel. This suggests that our covariates add minimal explanatory power beyond simple flexible time controls, as the R^2 in columns (2) and (4) only increases by 0.103 and

[32]Using a linear trend as the only regressor yields $R^2 = 0.416$ over 1970-2008 and $R^2 = 0.240$ over 1980-2008.

0.162, respectively, with the inclusion of the additional covariates. This result holds when restricting the sample to the 20 years before law passage in column (6), as adding all the covariates increases the regression R^2 by only 0.071. Overall, these regressions indicate that a high proportion of the variance in the timing of the passage of NP full independence laws is unexplained by variables capturing state health care utilization and state demand for health care. We treat this as additional evidence that the timing of state law changes is exogenous to health care utilization and health outcomes.

State Trends

A weakness of MEPS data for our analysis is that the lack of data before 1996 leaves few years before the policy change to identify preexisting state-specific trends in most states. As pointed out by Wolfers (2006), the inclusion of state-specific trends can therefore lead to upward or downward bias in our treatment effect estimates depending on the nature of the dynamic response of the outcome of interest to the policy change, and so we omit them in equation (1). To check the robustness of our results to the inclusion of state-specific trends, we use data from the Center for Disease Control's Behavioral Risk Factor Surveillance System (BRFSS) over 1988-2011. While BRFSS data do not contain information on children or offer the same level of detail on utilization or outcomes as MEPS data, the extra years of data allow for more precise estimates of underlying trends. Columns (1) and (2) of Table A.3 show the effect of including state-specific trends on our MEPS estimates of the probability of an adult receiving a routine checkup. While the estimated increase in probability is similar in the first 2 years after NP independence, the estimates with state trends are very close to zero in later years. This indicates that the estimated preexisting trend in checkup frequency in states that allow NP independence is upward, a counterintuitive result if concerns over faltering health care utilization play a role in states allowing NPs full independence. Columns (3) and (4) show that when using the longer pre-trend in the BRFSS data to reduce bias, the estimated effect of NP independence is smaller but the pattern is consistent with our original estimates, even when extending the dependent variable to include checkups over the past 2 years. The estimates in column (1) lie within the 95% confidence interval of our estimates in column (3) with the exception of the effect at 1-2 years after law passage, indicating we cannot rule out our original estimates after adding state-specific linear trends. Our results are therefore qualitatively robust to the inclusion of state trends, though the direct inclusion of state trends in the MEPS analysis would induce downward bias due to poor estimates of trends in utilization prior to NP independence.

Appendix Table A.1: ICD-9 Codes for Ambulatory Care Sensitive Conditions

ACS Condition and ICD-9-CM Code(s)	Comments
Congenital syphilis [090]	Secondary diagnosis for newborns only
Immunization-related and preventable conditions [033, 037, 045, 320.0, 390, 391]	Hemophilus meningitis [320.0] age 1-5 only
Grand mal status and other epileptic convulsions [345]	
Convulsions "A" [780.3]	Age 0-5
Convulsions "B" [780.3]	Age >5
Severe ENT infections [382, 462, 463, 465, 472.1]	Exclude otitis media cases [382] with myringotomy with insertion of tube [20.01]
Pulmonary tuberculosis [011]	
Other tuberculosis [012-018]	
Chronic obstructive pulmonary disease [491, 492, 494, 496, 466.0]	Acute bronchitis [466.0] only with secondary diagnosis of 491, 492, 494, 496
Bacterial pneumonia [481, 482.2, 482.3, 482.9, 483, 485, 486]	Exclude case with secondary diagnosis of sickle cell [282.6] and patients < 2 months
Asthma [493]	
Congestive heart failure [428, 402.01, 402.11, 402.91, 518.4]	Exclude cases with the following surgical procedures: 36.01, 36.02, 36.05, 36.1, 37.5, or 37.7
Hypertension [401.0, 401.9, 402.00, 402.10, 402.90]	Exclude cases with the following procedures: 36.01, 36.02, 36.05, 36.1, 37.5, or 37.7
Angina [411.1, 411.8, 413]	Exclude cases with a surgical procedure [01-86.99]
Cellulitis [681, 682, 683, 686]	Exclude cases with a surgical procedure [01-86.99], except incision of skin and subcutaneous tissue [86.0] where it is the only listed surgical procedure
Diabetes "A" [250.1, 250.2, 250.3]	
Diabetes "B" [250.8, 250.9]	
Diabetes "C" [250.0]	
Hypoglycemia [251.2]	
Gastroenteritis [558.9]	
Kidney/urinary infection [590, 599.0, 599.9]	
Dehydration - volume depletion [276.5]	
Iron deficiency anemia [280.1, 280.8, 280.9]	Age 0-5
Nutritional deficiencies [260, 261, 262, 268.0, 268.1]	
Failure to thrive [783.4]	Age < 1 only
Pelvic inflammatory disease [614]	Exclude cases with a surgical procedure of hysterectomy [68.3-68.8]
Dental Conditions [521, 522, 523, 525, 528]	
Cancer of the cervix [180]	

List of conditions and notes from Billings et al. (1993) and Wisconsin Department of Health Services (2011). We omit “skin grafts with cellulitis” as an ambulatory care sensitive condition because no ICD-9 code is provided to identify it. MEPS does not indicate whether a diagnosis is primary or secondary, so we search all listed diagnoses to determine if a condition is ambulatory care sensitive.

Appendix Table A.2: Determinants of Timing of NP Full Independence

	(1)	(2)	(3)	(4)	(5)	(6)
Physicians per 100,000	0.0229	-0.00748	-0.0122	-0.0110	0.00172	0.0141
	(0.0361)	(0.0434)	(0.0458)	(0.0345)	(0.0379)	(0.0176)
	[0.027]					
Medical Benefits per 100,000	0.00266*	-0.00003	-0.0000516	-0.0000151	-0.0000969	-0.0000695**
	(0.00128)	(0.0000506)	(0.0000732)	(0.0000469)	(0.0000619)	(0.0000268)
	[0.202]					
Change in Medical Benefits per 100,000	24.25	-3.928	-13.17**	4.834	-1.490	5.956
	(30.69)	(4.978)	(5.499)	(6.666)	(5.247)	(4.314)
	[0.032]					
Inpatient Days per 100,000	-0.0000161	0.0000356	0.00000834	0.0000169	0.0000104	-0.0000221
	(0.0000409)	(0.0000543)	(0.0000766)	(0.0000518)	(0.0000665)	(0.0000284)
	[0.004]					
Outpatient Visits per 100,000	0.00000485	-0.0000132	-0.000009.18	-0.0000136	-0.00000796	0.0000150
	(0.0000363)	(0.0000231)	(0.0000262)	(0.000018)	(0.0000224)	(0.0000146)
	[0.001]					
# of Medical Schools	1.481	3.903	3.642	2.501	2.688	1.533
	(2.535)	(2.348)	(2.333)	(2.241)	(2.372)	(1.216)
	[0.019]					
Democrat Governor	3.260	4.172*	4.779**	4.503**	5.418**	3.331**
	(4.612)	(2.236)	(2.169)	(2.058)	(2.050)	(1.322)
	[0.033]					
Share over 65	8.582	-65.83	-134.7	-106.1	-75.05	123.6
	(108.2)	(130.1)	(152.9)	(108.6)	(149.2)	(75.43)
	[0.001]					
Share under 20	-8.479	34.18	-99.67	-80.58	-82.61	77.84
	(64.79)	(72.54)	(86.05)	(67.99)	(79.26)	(49.98)
	[0.001]					
Share urban	1.466	-8.314	-7.578	-6.045	-8.504	-6.316
	(14.67)	(13.39)	(12.06)	(9.173)	(8.857)	(5.187)
	[0.001]					
Real Personal Income	0.000112	-0.00136	-0.00329	-0.00268	-0.00284	0.00140
	(0.00406)	(0.00240)	(0.00286)	(0.00230)	(0.00307)	(0.00122)
	[0.000]					
Constant		19.93	94.62	65.76	67.79	-33.76
		(43.69)	(55.00)	(40.93)	(52.12)	(30.41)
Observations	18	443	443	285	285	300
R^2		0.436	0.539	0.362	0.412	0.335
R^2 with only Year FE			0.436		0.250	0.264
Start Year	1970 only	1970	1970	1980	1980	1970
Year FE	No	No	Yes	No	Yes	No

Dependent variable is the number of years until the state allows NP full independence in all regressions. Observations are state-years in 1970 only in column (1), state-years from 1970-2008 in columns (2) and (3) and 1980-2008 in columns (4) and (5), and all state-years after 1970 and no more than 20 years prior to law passage in column (6). *, **, *** denote statistical significance at the 10%, 5%, and 1% levels respectively. Standard errors are Huber-White robust estimates clustered at the state level. Each cell in column (1) represents a separate regression of the listed variable in 1970 on the number of years until law passage. Changes in Medical Benefits regression in column (1) uses data from 1971 due to data availability. Number in brackets in column (1) is the R^2 value of the above regression.

Appendix Table A.3: State Trends

Years relative to full independence	(1) Checkup last year	(2) Checkup last year	(3) Checkup last year	(4) Checkup last 2 years
5+ years before	-0.000946	0.0240	-0.00852	-0.00262
	(0.0306)	(0.0269)	(0.0217)	(0.0186)
3-4 years before	0.0000638	0.0169	-0.0109	0.00120
	(0.0124)	(0.0243)	(0.0173)	(0.0160)
1-2 years before	0.00905	0.0158	-0.0232	-0.00151
	(0.0218)	(0.0223)	(0.0154)	(0.00939)
1-2 years after	0.0312*	0.0184	-0.0100	0.000889
	(0.0161)	(0.0194)	(0.0141)	(0.00844)
3-4 years after	0.0502***	0.0174	0.0198	0.0206
	(0.0152)	(0.0254)	(0.0212)	(0.0139)
5-6 years after	0.0370**	-0.0116	0.0119	0.0186
	(0.0160)	(0.0325)	(0.0193)	(0.0133)
7-8 years after	0.0539**	-0.0119	0.0196	0.0196
	(0.0212)	(0.0412)	(0.0229)	(0.0174)
9-10 years after	0.0613***	-0.0141	0.0325	0.0313
	(0.0203)	(0.0456)	(0.0335)	(0.0238)
11+ years after	0.0742***	-0.000931	0.0420	0.0416
	(0.0192)	(0.0539)	(0.0372)	(0.0260)
Observations	240,887	240,887	3,346,088	3,346,088
State Trends	No	Yes	Yes	Yes
Data Source	MEPS	MEPS	BRFSS	BRFSS

Regressions are estimates of equation (1) using data source listed at bottom. All regressions are weighted using provided sample weights. All columns are logit regressions and reported coefficients are average marginal effects. Dependent variable is listed at top and described in text. *, **, *** denote statistical significance at the 10%, 5%, and 1% levels respectively. Standard errors in parentheses are Huber-White robust estimates clustered at the state level.