The Impact of the Political Response to the Managed Care Backlash on Health Care Spending: Evidence from State Regulations of Managed Care*

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Abstract

During the late 1990s, there was a substantial cultural, media and legal backlash against the cost-containment practices of managed care health insurance organizations (particularly, HMOs). Most states passed a variety of laws in this period that restricted the cost-cutting measures that managed care firms could use. I use variation in the passage of these regulations as a proxy for the intensity of the political response to the managed care backlash across states and over time to investigate the effects of this political backlash on health care spending increases. I find that the political response to the managed care backlash increased the U.S. health care spending share of GDP by 2 percentage points, which is slightly more than its entire increase during the backlash period, relative to a counterfactual with no such political backlash. I provide evidence suggesting that alternative, purely economic factors for the increase in health care spending during the managed care backlash, do not drive my results. I also show that the political response to the backlash increased medical provider salaries and utilization, and that it decreased HMO penetration. Additionally, I find that the political backlash had small or negative effects on average health, but may have improved health for vulnerable subpopulations.

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1 Introduction

The growth of health care spending as a share of GDP has been one of the defining features of the U.S. health care sector. Personal health care spending has nearly doubled as a fraction of GDP in thirty years, rising from 8% of GDP in 1980 to 14.8% of GDP in 2009, and often has grown at a linear (and hence, unsustainable) rate for decades at a time. Health care expenditures form a significant fraction of U.S. government spending, which will increase markedly with the passage of the Affordable Care Act (ACA) as it will lead to substantial government subsidies to individuals to purchase private insurance. It is therefore important to understand the extent to which government policy – including state policies as well as those of the federal government – may influence the share of GDP spent on health care.

The overall trend of rising health care expenditures in the U.S. saw a temporary break during the 1990s, when personal health care spending as a share of GDP remained nearly constant (actually, declined slightly) from 12.1% in 1993 to 11.94% in 2000. This stabilization of health care expenditures coincided with the peak of the so-called managed care revolution, which saw the replacement of conventional insurers (who reimbursed hospitals and physicians for services provided without regulating utilization) by health insurance organizations that managed the medical care of their enrollees. The organizational innovation of managed care firms was to integrate physicians and insurers partially or completely to align their incentives and discourage physicians from inducing demand for medical care. The most well-known type of managed care organization, the HMO, restricted its patients to see a strictly delimited network of providers, who sometimes were its employees. While the growth of health insurance premiums slowed significantly, patients and physicians chafed under managed care controls. At the end of the 1990s, there arose a widespread backlash against managed care cost containment practices, with increasingly negative media coverage of managed care. Ultimately, state governments passed "patients' bills of rights" that limited the ability of managed care firms to restrict care and shape the incentives of medical practitioners. Health care spending resumed rising as a share of GDP in 2001, at the height of the managed care backlash. It remains an open question whether managed care succeeded in stabilizing U.S. health care spending or whether the slowdown in U.S. health care spending growth in the 1990s was a product of other factors (Glied 2003).

This paper finds that the political response to the managed care backlash, as proxied by the amount of legislation passed to restrict managed care cost containment practices (hereafter, backlash regulations), in fact had a causal effect on increases in health care expenditures. My identifying assumption is that the political managed care backlash increased health care expenditures only to the extent that managed care was already containing costs in the given state, while the timing of backlash regulations is exogenous with respect to all other variables whose effect on changes in health care costs is a function of managed care intensity. This assumption is weaker than the standard difference-in-difference assumption that the timing of the backlash regulations is uncorrelated with shocks to health care spending. My assumption is plausible because backlash regulations are politically determined variables, which are likely to arise from distinct data generating processes than are outcomes in health care markets. While it could fail in various ways – for instance, if regulations are passed in response to severe cost containment, which also decreases health care share, or if regulations are correlated with other trending variables in the health care market – many of these alternative hypotheses may be addressed through robustness checks.

To obtain my findings, I use panel variation in the passage of backlash regulations, which were passed in different years and in different numbers in different states. I allow the health spending share of gross state product to depend on both the number of backlash regulations as well as, crucially, on its interaction with managed care intensity. I proxy managed care intensity by HMO penetration in each state in 1995. HMO penetration is a natural proxy for managed care intensity both, directly, because HMOs are the most restrictive form of managed care, and, indirectly, because looser managed care organizations in the same state had to cut costs more substantially to compete with the HMOs.[1] Furthermore, I explicitly model the substantial persistence in the health care share by estimating models with

[1] The performance and prevalence of HMOs also could have provided demonstrations to less managed health care plans that tightly managed policies are marketable, encouraging these plans to adopt them. I provide evidence that HMO penetration is correlated with tight management of care (the degree of restriction on patients seeing providers) in Section 3.

the lagged health share as a regressor. An econometric difficulty in estimating such models is their mechanical failure of strict exogeneity and the poor performance of instrumental variables estimators when the persistence of the dependent variable is high (as documented by Hahn, Hausman and Kuersteiner 2007). Therefore, I use a novel approach pioneered by Hausman and Pinkovskiy (2013) that avoids the bias of instrumental variables by estimating a transformed version of the lagged dependent variable model with fixed effects via nonlinear least squares and an incidental parameters correction.

My results indicate that because of the political response to the managed care backlash, health care spending in a state with average HMO penetration in 1995 grew by 0.16 percentage points more per year than it would have otherwise, which is larger than the average change in the health care share across states in 2005. To assess the magnitude of my result, I use my regression to make a dynamic counterfactual forecast of the evolution of each state's health care share under the assumption that the number of backlash regulations was equal to zero in every state and year, and aggregate the forecasts to predict the counterfactual for the U.S. health care share for each specification I run. I find that under the counterfactual of no political response to the managed care backlash, the U.S. health care share in 2005 would have been 11.52%, nearly two percentage points of GDP lower than the actually observed level, and somewhat below the 2000 level of 11.94%. I provide a variety of robustness checks for my identifying assumption by including state trends and covariates, accounting for the timing of the passage of the regulations, varying the geographic unit of analysis, accounting for other health insurance regulations being passed at the time, accounting for trends in the hospital industry and instrumenting the passage of the backlash regulations by changes in the political power of parties and interest groups that contributed to their passage.

I intepret my estimate as the general equilibrium effect of the political response to the managed care backlash, broadly defined, on the health care and insurance market, rather than as a direct effect of the backlash regulations upon the population of HMO enrollees. The number of backlash regulations passed in a state may proxy for other dimensions of the policy response to the managed care revolution, such as the severity with which they would be enforced or the willingness of the state government to pass more binding regulations in the future. Moreover, the political response to the backlash, insofar as it reduced the range of cost containment practices that the most restrictive insurers could pursue, should have changed behavior for insurers who were not engaged in these practices but had to compete with insurers who did. I do, however, interpret my estimate as the result of a political backlash against managed care rather than as a consequence of purely economic shifts in the health care and insurance market, such as a change in tastes in favor of less restrictive care by consumers or a change in the insurer-provider competition dynamic, which I attempt to rule out through my robustness checks and the instrumental variables approach.

My auxiliary findings present further effects of the political backlash on the health care and insurance market in general equilibrium. I show that the political backlash raised the salaries of medical providers (consistent with Cutler, McClellan and Newhouse [2000]), but did not significantly increase medical employment, except in hospitals. Moreover, I find that utilization (measured by lengths of stay and inpatient days per capita) rose moderately with the passage of the backlash regulations. Finally, I find that HMO penetration declined with the intensity of the political backlash, which, in particular, can explain the full decline of HMO penetration from its peak in the late 1990s. The effects on HMO penetration are consistent with a hypothesis that the political backlash discouraged less-intensive insurers from forming HMOs, or decreased the ability of HMOs to compete with less restrictive insurers on price, thus pushing the whole market towards less restrictive cost containment practices and a higher health care share.

To my knowledge, this is the first paper that attempts to assess the impact of the entire complex of political responses to the managed care backlash on health care spending in the U.S. as a share of the economy in the late 1990s and early 2000s. Vita (1999) considers the effects of a particular type of backlash regulations (any-willing-provider laws) and finds that they increased health spending levels, but does not investigate other types of backlash regulations. Glied (2003) considers the reasons for the resumption of health care spending growth in the early 2000s, but does not give a quantitative estimate for the possible effect of the backlash. A large literature in health policy and law (Peterson 1999) has studied the managed care backlash qualitatively, discussing the reaction of the public, the legislation passed, and the weakening of managed care cost containment practices, but has not calculated the impact on health care spending.

In this paper, I remain largely agnostic about the welfare effects of the political managed care backlash. It is obvious that lower health care spending does not imply that consumers were able to obtain the same quality of health care at a lower price, and since health care spending has been found to improve health cost-effectively on average (Cutler 2004) the backlash may have improved health and welfare while increasing spending. However, a growing literature on the impact of managed care on health that uses experimental methods typically finds that managed care cost containment practices do not have substantial adverse health effects on average, although there may be some adverse effects for disadvantaged and chronically ill populations (Miller and Luft 1997, Cutler, McClellan and Newhouse 2000, Glied 2000). Moreover, it may be the case that the political managed care backlash improved welfare through other channels than improving health; for example, people may have incurred disutilities from not being able to choose their own doctor regardless of the impacts of that decision on their health. While I cannot explore the hedonic channels through which the managed care backlash may have affected welfare, I can consider whether the managed care backlash is associated with health improvements. Consistent with the literature, I find that the political response to the managed care backlash does not seem to have decreased mortality in the under-65 population (in fact, I barely fail to reject the null hypothesis that the political backlash had no effect in favor of the hypothesis that it *increased* mortality in this age group). Aside from mortality, I also look at self-reported health in the Current Population Survey. I find that the fraction of people saying that they quit their jobs because of health conditions decreased with the passage of backlash regulations, which is consistent with the hypothesis that managed care may have hurt a fraction of people with conditions that had to be managed chronically and who perform physically intensive jobs.

The rest of the paper is organized as follows. Section 2 presents a brief history of managed care in the United States and describes managed care cost containment practices as well as the laws regulating them. Section 3 describes the data. Section 4 explains the empirical specification. Section 5 presents the baseline results for health spending growth, as well as the associated robustness checks. Section 6 presents results for health resources utilization and health outcomes. Section 7 discusses political determinants of backlash regulations and presents an instrumental variables analysis. Section 8 concludes.

2 Institutional Background and History

Since patients and doctors have substantial flexibility in choosing the intensity of treatment, the health insurance market suffers from moral hazard (Arrow 1963) unless insurers monitor treatment choices or use financial incentives for insurees to economize on care. Most U.S. health insurance before the 1980s (and all of Medicare and Medicaid) was conventional: insurers reimbursed physicians and hospitals for each procedure performed, using deductibles and copayments to provide incentives against unlimited utilization, but they did not intervene in physician treatment choices. An alternative arrangement, referred to as managed care, involves insurers directly contracting with or even employing physicians and regulating their choice of care either through more sophisticated financial incentives or through the threat of termination (or "deselection" from the contract network) if the insurer deems that the physician utilizes health resources beyond what is clinically necessary. The most restrictive variety of managed care, the health maintenance organization (HMO) either hires the physicians whose care it reimburses, or forms exclusive contracts with a panel of physicians, forbidding its patients to see other physicians in most circumstances. A less restrictive (and currently most widespread) version of managed care is the preferred provider organization (PPO), which contracts with a network of physicians to receive discounts on their fees in return for the PPO giving a discount to its patients to see the physicians in the network. HMOs depart from fee-for-service reimbursement by paying physicians salaries, bonuses for low utilization, or capitated reimbursement for each patient regardless of the care provided by the physician. Additionally, managed care firms restrict patient choices through gatekeeping (the requirement to see specialists only after a referral by a primary physician) and utilization review (submission of proposed procedures to the insurer, and potential refusal to cover expensive or experimental treatments).

For most of the postwar period, managed care remained a small fraction of the U.S. health insurance market, but between the late 1980s and early 1990s it became the dominant form of health insurance in a phenomenon known as the "managed care revolution," as more and more employers and individuals saw relatively less expensive managed care as preferable to conventional fee-for-service insurance.[2] To the extent that they lowered the level and growth rate of medical spending and insurance premiums, the cost containment practices of managed care benefited healthy patients, employers and the federal and state governments. However, they hurt physicians, who now had to compete for membership in the networks of managed care organizations and incorporate financial considerations into their practice style, as well as less healthy consumers, who now obtained much lower quality insurance. The employment-based system of health insurance served to increase the salience of discontents with managed care and decrease the salience of their advantages because the wage increases resulting from cheaper health insurance were not explicitly tied to the change in health insurance arrangements in the minds of workers (Blendon et al. 1999).[3] Instead, workers suffered the disruption of switching not only to a new insurance regime but also to a new provider network without attributing any resulting wage increases to the switch to managed care.

As a consequence of these discontents, in the late 1990s a powerful cultural, media and legal backlash took place against managed care in general and HMOs in particular. HMOs were depicted in special reports in major newspapers and in popular films such as *As Good as it Gets* as impersonal, greedy bureaucracies that denied life-saving care to critically ill people in order to enhance their profits. Brodie et al. (1998) document that the tone of media coverage of managed care, especially in the most visible news sources such as television and newspaper special reports, grew to be increasingly critical, and gave increasing weight to anecdotes of managed care patients being denied essential care. Partially in response to this backlash, states passed "patients' bills of rights" that limited the cost-control practices allowed to managed care organizations. There were four types of backlash regulations as

[2] Managed care was integrated into Medicare through the voluntary program Medicare part C, which allowed patients to opt out of traditional Medicare in favor of a managed care plan. Medicaid employed managed care by shifting patients to it by fiat at the state (or sub-state) level.

[3] A robust finding in the health care labor literature is that increases in health insurance premiums are shifted almost completely to worker wages. See e.g. Gruber (1994, 1997).

shown in Table I : regulations to provide access to physicians and treatments, regulations to facilitate appeals of managed care decisions, regulations of the insurer-provider relationship and regulations to mandate particular procedures. Access regulations permitted patients to continue to see doctors outside a managed care firm's network for long-term illnesses (continuity of care) and to have direct access to specialists without having to first go through a gatekeeper. Appeals regulations provided internal or external procedures for appealing managed care decisions, in some states including holding insurers liable for medically adverse events resulting from denial of coverage. Provider regulations limited how managed care firms could reimburse physicians in their networks or in their employment and limited managed care's control over the composition of their network (Any Willing Provider or Freedom of Choice laws). Mandated benefits mostly consisted of maternity stays, reconstructive surgery after cancer, and diabetes supplies. While no federal legislation was ultimately passed, nearly all states enacted various legislation of their own, at different times and of differing severity.

3 Data

I obtain data on HMO penetration, backlash and other health insurance market regulations, state-level mortality and self-reported health data, state-level health care expenditures and hospital-level expenditure, payroll, employment, and utilization variables for all 50 U.S. states and the universe of U.S. hospitals over the time period 1994-2005. I compute all my regressions over the period 1995-2005, using the observation in 1994 to construct lagged dependent variables.

HMO Penetration

I obtain data on HMO penetration indirectly from the survey firm Interstudy. I obtain state-level data for the percentage of the total population (including Medicare and Medicaid recipients) enrolled in HMOs for 1980, 1985, 1990 and 1995-2007 from the Statistical Abstract of the United States. I use HMO enrollment, rather than total enrollment in managed care, to measure managed care intensity because by the beginning of my sample period (1995-2005), most U.S. private health insurance was some form of managed care, with HMOs being the most restrictive, while the share of conventional insurance was low and falling, and thus, unlikely to be very informative. I also obtain data on total population HMO penetration at the county level for the years 1990-2003 from Laurence Baker, who constructed these measures using unit records from Interstudy, which are not available for the public. The exact method of construction of the county-level data is described in Baker and Phibbs (2002) and involves extrapolation on the basis of county population and the regional enrollment of HMOs serving each county in question as reported to Interstudy. The Statistical Abstract and Laurence Baker use somewhat different definitions of HMOs to construct their HMO penetration rates, but my results are robust to using either measure in the state-level analysis. For regressions at the state level I use the Statistical Abstract series, and for substate-level regressions, I use the Baker series.

Throughout this paper, I use HMO penetration as a proxy for the intensity of managed care activity in a given region (state, MSA, county). It is intuitive that HMO penetration should be a good proxy for the overall level of managed care activity because HMOs were the most restrictive form of managed care. Since HMOs and less restrictive forms of health insurance operate in the same product and factor markets, high HMO penetration should incentivize other insurers to adopt restrictive practices to lower costs so that they could better compete with HMOs. The presence of HMOs should spread restrictive cost containment practices through the "demonstration effect" of showing that the health insurance market will bear such practices (e.g. that large numbers of people will purchase plans that do not cover all local providers). As discussed by Bloch and Studdert (2004), physicians and hospitals would be likely to use the same practice style for all their privately insured patients, whether those belonging to HMOs or not, which would lead to spillovers. A large literature in managed care documents that premium growth rates within and outside HMOs track each other very closely (Ginsburg and Pickreign (1996, 1997) use KPMG data to show that HMO premium growth was at least 75% of conventional premium growth over the period 1992-1996), and a series of papers show that increases in HMO penetration in a region decrease the health spending growth rate of conventional insurers in the same region (Baker 1997, Chernew et al. 2008). HMO penetration also correlates very well with evidence of restrictive cost containment practices. The MEPS-IC, which is a nationally representative survey of health insurance plans, asks about the extent to which a plan contracts selectively, and about the extent to which care is managed in the plan, with answers to these questions being independent of whether a plan is formally an HMO (so a conventional plan without selective contracting but with some utilization review would answer "yes" to the question of whether there is any managed care in the plan). Part 1 of Figure 1, shows the correlation between HMO penetration in a state and the state-level estimates of the number of firms that offer plans with any managed care from the 1996 MEPS-IC (correlations between HMO penetration and the extent of exclusivity of providers are even stronger).[4] We see that the correlation is tight, which reinforces our confidence in HMO penetration as a proxy for the intensity of managed care cost containment practices.[5]

Part 2 of Figure 1 shows a time plot of the Statistical Abstract HMO penetration measure for the United States as a whole. We see the steady rise of managed care during the 1980s and the 1990s, followed by a partial but precipitous decline during the backlash period. In Section 6 of the paper I will argue that much of this decline can be explained by the political response to the managed care backlash. Part 4 of Figure 1 shows a scatterplot of backlash regulations passed by 2005 against (Statistical Abstract) HMO penetration by state in 1995. We see that states with high HMO penetration were on West Coast, in the Northeast (especially Massachusetts) and in the Midwest (Minnesota).

Backlash Regulations

The key independent variable in my analysis is state regulation of managed care cost containment practices, which I refer to as *backlash regulations* throughout the paper for brevity. I obtain data on the passage of various managed care regulations during the backlash from the National Council of State Legislatures, which maintains databases of state laws on

[4] The 1996 MEPS-IC was not large enough to support state-level estimates for 10 of the smallest states; hence, this correlation is on the basis of the 40 largest states only.

[5] In my main analysis, I prefer the HMO penetration measure to the MEPS-IC measures because the MEPS-IC statistics are liable to have measurement error. MEPS-IC publishes statistics only on the fraction of firms offering plans with various levels of intensity of managed care, rather than on the number of people enrolled in any such plans. Since large firms tend to have different health insurance purchasing behavior than do smaller firms, I do not expect the two measures to be the same. Moreover, since health care costs depend on the number of patients involved rather than on the number of firms involved, I prefer the population-based HMO penetration measure to the firm-based MEPS-IC measures.

various topics for research purposes freely available to the public. Each type of regulation is listed separately for each state, even if multiple regulations were passed together in a single bill, and multiple regulations on a single topic (e.g. banning financial incentives for physicians) are listed separately. Altogether, there are about 750 backlash regulations. Table I shows the different types of regulations, both in a fine (27 groups) and in a coarse (4 groups) categorization, as well as how many regulations of each type were passed.[6] Part 3 of Figure 1 shows a time series of the adoption of new backlash regulations. We see that most such regulations were passed in the 1996-2001 period, although a few were passed before and after this period. No new backlash regulations were passed after 2005. In my analysis, I will use the raw total of backlash regulations as a measure of regulation intensity in most specifications, although I will check for robustness to alternative parametrizations of the regulations.

Throughout the paper, I do not assert that the effect I find is the causal effect of the regulations themselves. The heterogeneous nature of the regulations and of their enforcement precludes such a causal attribution. Moreover, the passage of regulations may have signaled to managed care organizations that more binding legislation may be passed if they do not change their practices. Instead, I interpret the estimated effect as the effect of the broader *political response* to the managed care backlash, which may include the direct effects of the backlash regulations, their effect from signaling, as well as other indications that state governments may have given the insurance industry as to the strength of their potential response to the popular discontent with managed care. In Section 5, I will argue that the effect that I am measuring does not come from changes in consumer preferences or other purely economic trends in the medical care and insurance market, or from state policies in the health care sector that were unrelated to the managed care backlash. [7] In

[6] These groupings have been constructed by the National Council of State Legislatures. My only alteration to the coarse grouping structure has been to reassign mandated maternity benefits to the "Mandated Benefits" rather than to the "Provider" grouping, since it is patently a mandated benefit rather than a regulation influencing providers. Keeping the grouping structure exactly as it has been set by NCSL would further strengthen the result in Table VIII that provider regulations were the key components of the backlash regulation package.

[7] The incidence of backlash regulations is not straightforward because public insurance (Medicare and Medicaid) tended to be regulated separately from private insurance, and because self-insured firms were exempt from state regulation through ERISA. As I have discussed in Section 1 and earlier in Section 3, there is good reason to believe that backlash regulations had substantial spillovers to insurers who were not regulated by them directly because of the extent of spillovers between HMO and non-HMO insurance.

this paper, I will use the phrases *political backlash*, and *political managed care backlash* to refer to the actions (overt or hidden) taken by state governments to address the popular discontent manifested in the managed care backlash, and which I measure by the passage of the backlash regulations.

We see that backlash regulations are not associated with pre-period state HMO penetration. Part 4 of Figure 1 presents a scatterplot of the number of backlash regulations passed by 2005 against HMO penetration in 1995. The relation is positive, but weak and insignificant.[8] We see that some states with low HMO penetration (like Wyoming and Mississippi) also had few regulations. However, some states with low to moderate HMO penetration (Texas, South Dakota, Virginia, Kentucky, Tennessee) were leaders in backlash regulations, while the managed care leaders (California, Oregon, Massachusetts) had lower levels of backlash regulations. I explore controlling for other potential time-varying correlates of backlash regulations in Section 5.

Other Regulations

I obtain data on other health insurance regulations from the Blue Cross Blue Shield publication "State Legislative Health Care and Insurance Issues." From this data, I extract the series of state mandated benefits, the series of state small-group insurance reforms, and the series of state individual insurance reforms. Since mandated benefits are qualitatively similar (although involving mandates of different expense), I use the raw total number of mandated benefits in each state-year as an independent variable. However, since different small-group and individual insurance reforms regulate different aspects of the insurer-insuree relationship, I follow Simon (2005) and code whether each state has a "full reform" or does not have a "full reform." I define a full reform by the presence of a guaranteed issue law, a guaranteed renewal law, and rating reform. I supplement these regulations data with data from Avraham (2010) on state tort reforms, which limited physicians' vulnerability to malpractice suits. The tort reforms in the Avraham (2010) database are caps on noneconomic damages, caps on punitive damages, caps on total damages, split recovery reform, collateral

[8] A 10 percentage point increase in 1995 HMO penetration is associated with an additional 0.6 regulations, with a t-statistic of 0.6.

source reform, punitive evidence reform, periodic payments reform, contingency fee reform, joint and several liability reform and patient compensation fund reform; see Avraham, Dafny and Schanzenbach (2009) for a detailed description of each measure. Avraham, Dafny and Schanzenbach (2009) find that a wide variety of tort reform regulations decrease PPO insurance premiums during the period 1998-2006 (they have no effect on HMO premiums because, the authors conjecture, HMO utilization control limits "defensive medicine" within HMOs). Following my parametrization of the backlash regulations variable, I use the total number of types of tort reform regulations passed as an independent variable. This is a variant of a specification used by Avraham, Dafny and Schanzenbach (2009); results using indicators for different tort reform regulations are virtually identical to just using the sum of the tort reform regulations. Additionally, I obtain data on simulated Medicaid eligibility (eligibility in a demographically constant population) from Gruber and Simon (2008).

Dependent Variables

I obtain state-level data on economic activity (gross state product) and data on total (public and private) personal health expenditures as well as separate data on personal health expenditures in Medicare and Medicaid from the Center for Medicare and Medicaid Services (CMS). I also obtain county-level data on economic activity (personal income) from the Bureau of Economic Analysis (BEA), which I use to normalize my health spending variable when I run regressions at sub-state levels. Additionally, I obtain state-level BEA data on employment and salaries in the hospital sector and separately in the ambulatory health sector (which comprises of physician offices, outpatient centers and home health care). I use the American Hospital Association Annual Survey for data on admissions, inpatient days and beds, as well as for disaggregated data on hospital expenditures. I also obtain data on under-65 mortality rates by state and year from the Center for Disease Control, as well as on self-reported health status and an indicator for whether someone has quit their job for health reasons for the under-65 population from the Current Population Survey.

Table II presents summary statistics for state-level data in 2005, including personal health expenditures, regulations, and HMO penetration. We see the sample mean of backlash regulations in the entire dataset was about 15, and the sample mean of 1995 HMO penetration is 14.5%. The mean annual change in the health care share of GSP in a typical state was about 0.1 percentage points.

4 Empirical Strategy

It is intuitive that health spending is very persistent. The set of sick and healthy people, their medical needs, and the practice styles and technology used to treat them tend to be the same over short periods of time, because of the relatively unchanging landscape of human illness and because rapid change in the medical system would be unsettling to patients. The persistence of health spending is found to be important in papers in which it is modeled, such as Cutler and Sheiner (1998). Furthermore, many papers find that institutional changes in health care markets have effects not only on the level, but on the trend of health care spending or of utilization patterns in the health care sector (Finkelstein 2007; Acemoglu and Finkelstein 2008). I therefore estimate a flexible dynamic panel specification that allows the lagged value of health care costs to affect the current value of health care costs, as well as contains state and year fixed effects.

$$P_{s,t} = \alpha_s + \lambda_t + \delta P_{s,t-1} + \beta R_{s,t-1} + \gamma R_{s,t-1} \times HMO_s^{1995} + X'_{s,t}\eta + \varepsilon_{s,t} \quad (1)$$

where $P_{s,t}$ is the total health spending share of gross state product in state s and year t (in some regressions, the dependent variable will be different), α_s and λ_t are state and year fixed effects respectively, $R_{s,t-1}$ is the number of regulations in force in state s in year $t-1$, HMO_s^{1995} is HMO penetration in state s in 1995, and $X_{s,t}$ is a vector of controls (absent in the baseline specification). The coefficients of interest are γ, the interaction effect of regulations on health spending as a share of GSP as a function of HMO penetration, β, the level effect of regulations as a function of HMO penetration, and the persistence parameter δ.

My identification assumption is that states with different pre-period HMO penetration have differential trends in *changes* in health care costs as a share of output in the period

1995-2005 only because of the political response to the managed care backlash, as proxied by the passage of backlash regulation, taking into account the natural persistence of the health care spending share of GSP. In particular, because I use panel data with fixed effects, I avoid the potential danger that states with different amounts of regulation also differ in other static characteristics that influence health care spending growth as a share of GSP uniformly over time. A threat to my identification strategy is the potential for the political response to the backlash regulations to be correlated with purely economic trends in the health care and insurance market that lead to rising health care spending. In Section 5, I present numerous robustness checks that control extensively for such a possibility, and in Section 7 I present an instrumental variables analysis that finds my baseline effects using variation in the passage of backlash regulations that is solely attributable to plausibly exogenous changes in political power of various groups. In this instrumental variables analysis, I will not be able to reject the hypothesis that the passage of the backlash regulations, and therefore, the political response to the managed care backlash, is exogenous to the innovations in state health care spending shares, which implies that estimating equation (1) is more efficient than using instrumental variables.

It is well known (Anderson and Hsiao 1982; Arellano and Bond 1991; Blundell and Bond 1995) that estimation of equation (1) by ordinary least squares yields biased and inconsistent estimates of the coefficients δ, β and γ. The standard technique for dynamic panel estimation is the approach of Arellano and Bond (1991) of differencing equation (1) and using lagged dependent and independent variables as instruments for the lagged difference via GMM. However, this approach exhibits substantial bias in the case when δ is close to unity because the correlation between the instruments and the endogenous variables is close to zero (Blundell and Bond 1995, Hahn, Hausman and Kuersteiner 2007). In particular, the coefficient δ tends to be biased downward, suggesting less persistence in the dependent variable than is actually present. Therefore, in this paper, I follow Hausman and Pinkovskiy (2013) and use the Dynamic Panel Nonlinear Least Squares Estimator (DPNLS). Specifically, I back-substitute for $P_{s,t-1}$ in equation (1) to express $P_{s,t}$ in terms of $P_{s,0}$ and lags and levels of the independent variables; and estimate the resulting equation by nonlinear least squares augmented by a correction for the fact that the number of regressors (the fixed effects) goes to infinity as the sample size goes to infinity. This procedure assumes that there is no serial correlation in the error terms; this assumption can be relaxed, but at the cost of reduced efficiency when it actually holds in the data. For most of my analysis the estimates using either assumption are very similar, so for my baseline results and all the robustness checks except the ones involving sub-state level regressions (for which the no serial correlation assumption appears to be violated) I maintain the no serial correlation assumption. [9] I compute standard errors by running 100 boostrap iterations of this procedure, drawing 50 states with replacement for each bootstrap iteration.

5 Results: Spending Growth

To assess the magnitudes of my estimates, in all my tables, I present forecast values of the total health spending share of U.S. GDP (or the Medicare, Medicaid or private share in some specifications) under the assumption that no backlash regulations had been passed. I forecast by bootstrapping the coefficients on the terms in the model that depend on backlash regulations and computing the increase in the dependent variable coming from backlash regulations for each state and year (using the point estimate of the lagged dependent variable coefficient to compute the dynamic contributions of regulations in a given year upon health care shares in future years). I then subtract the bootstrap estimates from the true values of the dependent variable (in levels) for each state and year, and aggregate the state-level forecasts (with suitable weights) to obtain a national forecast. I present a point forecast based on the estimated values of the coefficients, and I repeat this procedure 500 times, each time drawing a different set of the backlash regulation coefficients from the estimated distribution, to obtain a forecast distribution. I report the upper and lower 90% confidence bounds of the forecast distribution below the point forecast. Since the hypothesis of interest will typically be that the difference between the observed and the counterfactual values of

[9] I provide a complete description of the procedure I use in Hausman and Pinkovskiy (2013) currently available as a mimeo from my website, http://www.newyorkfed.org/research/economists/pinkovskiy/index.html. I provide a simulation exercise that shows that for plausible parameter values, DPNLS performs significantly better in terms of mean-squared error than do the Arellano-Bond and Blundell-Bond estimators. Table IV shows that the differences between Arellano-Bond, Blundell-Bond and DPNLS estimates are consistent with the large value of δ creating bias in GMM estimation.

a certain series (e.g. the U.S. health care spending share) is *greater* than zero, rather than that it is *different* from zero, looking at 90% confidence intervals is an easy way to perform such a one-sided test at the 5% significance level.[10]

Baseline Results

Table III presents estimates of equation (1) when the dependent variable is the total personal health spending share of GSP, the private share, the Medicare share and the Medicaid share. We see that the coefficient of interest – the coefficient on the interaction between backlash regulations and pre-period HMO penetration – is significantly different from zero with more than 99% confidence for both the total share and the private share. The magnitude of the interaction coefficients when the dependent variable is the total health share is 0.119 percentage points, and is very similar for the private share.[11] The (insignificant) main effect of regulations is (-0.007) for the total share, and similarly for the private share. Since the average number of regulations in 2005 is 15.22, and the average 1995 HMO penetration is 0.145, for a typical state, the managed care backlash is associated with an extra 0.16 percentage point increase in the personal health share of GSP every year. Given that the mean increase in the personal health share of GSP across all states in 2005 was 0.1 percentage points, we see that the estimated effect of the political backlash is substantial. The counterfactual predictions of the model for what would have happened without the political managed care backlash are striking. The total health share of U.S. GDP was 13.48% in 2005, but with backlash regulations set to zero, it would have been 11.52%, about 2 percentage points of GDP lower, which, given that U.S. GDP in 2005 was about 12 trillion dollars, amounts to 235 billion dollars lower. This is equal to 77% of Medicare spending in 2005 (which was 2.6% of GDP) and is 17% of the counterfactual health care share in 2005. The confidence interval of this forecast, however, is large, and permits us to rule out the

[10] In this procedure, I fix the value of the lagged dependent variable coefficient to its point estimate. Unfortunately, allowing for error in the lagged dependent variable coefficient causes the forecast errors to explode, and the forecast distribution to no longer be Gaussian (as it becomes a sum of products of correlated Gaussian random variables). The forecast distribution variances I obtain are similar to those I get if I impose the lagged dependent variable coefficient to equal unity.

[11] An elementary robustness check is to verify that my estimates are not sensitive to excluding individual states or groups of states from my sample. I therefore re-estimate equation (1) 50 times, dropping a different state each time, and look at the highest and lowest values attained by the interaction coefficient. I also repeat this exercise again 8 times, each time dropping a different region of the U.S. (New England, Mid-Atlantic, Southeast, Great Lakes, Plains, Southwest, Rocky Mountains, Pacific). The lower bound on the interaction coefficient is 0.09, and the upper bound is 0.13.

observed 2005 level only with 90% confidence. Part 5 of Figure 1 plots the observed path of the total share of GDP and its counterfactual under the assumption of no backlash regulations; we see that without the political managed care backlash, the model suggests that the total health care share of GDP would have tended to be somewhat below 12%, its long-run level during the 1990s. [12] A similarly low forecast, this time statistically different at 5% from the observed 2005 level, can be observed for the private share.[13] The point estimate of the lagged dependent variable coefficient is equal to 1.005, almost exactly unity, and is statistically significant at 1%, showing the importance of controlling for dynamic effects in the analysis.[14]

I interpret this result as the measurement of the effect of the broader political backlash against managed care cost containment practices on general equilibrium outcomes in the health care market as a whole, rather than as direct effect of the backlash regulations on changes in spending by people enrolled in HMOs. First, the backlash regulations are used only as a proxy for a broader (and likely, partially unobservable) political backlash against managed care cost containment practices. It is likely that states that passed more backlash regulations also enforced them more rigorously and had greater political will to pass potentially more stringent regulations unless managed care organizations changed their practices. Second, it is clear that much of the effect of the political backlash was a spillover effect to non-HMO insurance (conventional and looser managed care arrangements) rather than a direct effect on HMOs (since HMOs enrolled only 30% of the insured). As discussed in Section 3, such spillovers are both theoretically expected and empirically documented in the

[12] In Part 5 of Figure 1, the counterfactual path of the health care share without backlash regulations first falls, then rises slightly during the recession of 2001, and then falls to its 1999 level by 2005. The fact that the difference between the counterfactual path and the observed path is slightly increasing over time is because backlash regulations affect the change of the health share of GSP, and therefore have a trend effect on the level of the health share of GSP. My estimates suggest that absent the backlash regulations, the health share of GDP would have been on a slight negative trend, and with the backlash regulations it was on a positive trend instead. Negative health share trends actually did take place in states most affected by the managed care revolution, such as California.

[13] In results not reported, I estimate equation 1 with log health share, log health expenditures per capita and log total health expenditures as dependent variables. The results are qualitatively similar, although the effects when the dependent variable is log expenditure or log expenditure per capita are of lower magnitude.

[14] Taken literally, this estimate suggests that state health care shares follow random walk (actually, mildly explosive) processes; however, the confidence interval on this estimate allows coefficients as low as 0.91, which would imply mean reversion in health care shares. Hence, I cannot reach conclusions about whether health care shares appear to be headed to a long-run level or whether they may rise indefinitely, and therefore view my estimates as a local approximation to the behavior of health care shares in the 1990s and 2000s. I view a random walk approximation as plausible given that the U.S. health care share has been rising for decades. This finding also suggests that Arellano-Bond estimation, which instruments lagged differences of the dependent variable with lagged levels, would encounter substantial weak instrument problems.

managed care literature. Some channels for this spillover will be shown in Table XI, where we will see that backlash regulations are associated with increases in hospital salaries and utilization, which should have impacted hospital spending beyond that on HMO patients. The backlash regulations are also associated with declines in HMO penetration, which would be consistent with spillovers to non-HMO insurers in general equilibrium through lower pressure to control spending from HMOs.

It is important to relate the measured effect of the political managed care backlash to the behavior of the health care share of the U.S. in the past 30 years. First, the implication of the counterfactual is not that the U.S. health care share would have declined by 2 percentage points in the absence of the regulations, but that its (observed and non-counterfactual) rise by 1.5 percentage points during the managed care backlash would have been avoided completely. In the counterfactual, the U.S. health care share would have largely remained at its 1993-2000 level, declining slightly by 2005. Since the U.S. health care share had been stable for the previous seven years, it is not implausible that the stabilization could have continued; what is radical is the shock to the health share in the early 2000s and not the prediction of the dynamic panel data model that I use. The magnitude of the additional predicted decline in the U.S. health care share (by an additional 0.4 percentage points) is consistent with data on actual declines in health care shares during the managed care revolution of the 1990s. For the U.S. as a whole, the health care share decreased by 0.16 percentage points (from 12.1% of GDP to 11.94% of GDP) and for states with high HMO penetration, health care share declines were even more striking. For example, California experienced a 1.56 percentage point decline in its health care share between 1993 and 2000 (from 11.13 to 9.57% of GSP) and Massachusetts had a 1.37 percentage point decline in the same time period (from 13.57 to 12.2% of GSP). Hence, the counterfactual prediction of the further decline of the U.S. health care share is not out of line with observed experience during the managed care revolution.

Part 6 of Figure 1 provides some of the intuition behind the association between backlash regulations and health spending by plotting average health share changes for states with HMO penetration above the lower quartile (blue line) and below the lower quartile (red line) of the HMO penetration distribution around the year in which a state passed most of its outstanding backlash regulations. We see that the low-HMO states experienced high health care share growth before and after passing backlash regulations. On the other hand, the high-HMO states consistently experienced low health care share growth before passing backlash regulations, but within two years of passing the regulations, their health care shares started growing at the same rate as those of the low-HMO states. The backlash could thus be seen as a partial reversal of the managed care revolution that took place in the early to mid-1990s.

Finally, I present associations between backlash regulations and Medicare and Medicaid shares of GSP. As described above, both Medicare and Medicaid contain elements of privately provided managed care (Medicare Part C allows seniors to trade conventional Medicare for a private plan, and Medicaid is provided by managed care organizations in multiple states). The interaction coefficient when Medicare share is the dependent variable is much smaller than the magnitude of the Medicare program would suggest (it is one-fifth of the U.S. health share, but the interaction coefficient is less than one-tenth of the baseline interaction coefficient), though it is positive and statistically significant. The interaction coefficient when Medicaid share is the dependent variable is negative and statistically insignificant. Finally, the counterfactual forecasts had the managed care backlash not taken place are very close to the observed Medicare and Medicaid shares in 2005. One rationalization of these results is that Medicare is a federal program with a federal-level reimbursement schedule that creates high-powered incentives (Clemens and Gottlieb 2012) and should therefore not have been directly affected by backlash regulations. Medicaid, though regulated by the states, has its own regulations for managed care as well as its own reimbursement practices that change the cost-cutting incentives of Medicaid managed care. The small spending increases that are observed probably come from spillovers from private insurance. The finding that the total health care share rose because of the managed care backlash is mostly driven by the behavior of the private health share.

Alternative Specifications

For most of the estimates of the persistence parameter δ that I obtain in Table III, δ is extremely close to unity. Moreover, for some of these estimates, I cannot reject the null hypothesis that δ is equal to unity, and for the baseline specification the upper bound of the confidence interval for δ is unity. If δ is taken to be unity, equation (1) implies an equation of the form

$$\Delta P_{s,t} = \alpha_s + \lambda_t + \beta R_{s,t-1} + \gamma R_{s,t-1} \times HMO_s^{1995} + X'_{s,t}\eta + \varepsilon_{s,t} \tag{2}$$

Unlike equation (1), equation (2) is readily estimable by OLS and is more efficient when δ is actually equal to unity. It has an intuitive interpretation: it is just the regression of the change in the dependent variable on state characteristics, national trends, and the independent variables of interest. Since this specification is less general than specification (1), and since the autoregressive coefficient δ does not equal unity for some specifications in my analysis (in particular in Table XI for dependent variables other than the health spending share), I continue using specification (1) for my baseline analysis. Results computed with the difference specification equation (2) are available on request.

Table IV presents several versions of the baseline specification equation (1). Column 1 omits the lagged dependent variable altogether and estimates a standard fixed-effects model. We see that failing to include a lagged dependent variable results in noisy estimates that suggest that backlash regulations lowered health care costs. However, including the lagged dependent variable, even in the presence of state fixed effects, is appropriate because it is always significant at very high levels whenever it is included, and it is correlated with backlash regulations and their interaction with HMO penetration.[15] Column 2 includes the lagged dependent variable and estimates equation (1) using OLS, while Columns 3 and 4 use Arellano-Bond and Blundell-Bond respectively. We see that for all three specifications, the interaction coefficient is smaller than for the baseline specification and is statistically insignificant, and that the coefficient on the lagged dependent variable appears underestimated

[15] In a regression of the lagged dependent variable on regulations and their interaction with HMO penetration as well as state and year fixed effects, the p-value of the F-test that both regulation variables are jointly zero is less than 0.01.

both by OLS and Arellano-Bond. Column 5 presents nonlinear least squares estimates of of equation (1) without an incidental parameters correction, and Column 6 presents DPNLS estimates (the baseline estimates of the paper). We see that DPNLS estimates the covariate coefficients to be statistically significant and much larger than do Arellano-Bond and Blundell-Bond, and it estimates a larger autoregressive coefficient than Arellano-Bond. We also see that failing to correct the incidental parameters problem (column 5) decreases the estimate of the autoregressive coefficient. Column 7 presents DPNLS estimates of equation (1) relaxing the assumption that errors within panel units are uncorrelated and using past values of the regressors as instruments to perform nonlinear GMM and augment the first order condition, as is described in Hausman and Pinkovskiy (2013). We see that the estimates are very close to the baseline (if anything, suggesting a greater impact of the political backlash) and we verify it formally with a Hausman test. Finally, Column 8 presents estimates of equation (2). The coefficient estimates are very similar to the baseline, as is the counterfactual forecast, which now is statistically different from the observed 2005 level at 5%.

5.1 Robustness Checks

Robustness to Trends and Panel Covariates

Table V reestimates equation (1) when additional trends or control variables are added to the regression. Column 1 reestimates the baseline. Column 2 adds trends for each of the 8 subregions of the U.S. (listed in footnote above) with little change to the results. Column 3 adds region-year fixed effects, also with few changes to results. Column 4 adds state-specific trends, a demanding robustness check (it effectively involves quadratic trends in the health share of GSP because of the persistence of the lagged dependent variable). The interaction coefficient remains very close in magnitude to the baseline (0.109) but loses significance, while the coefficient on the lagged dependent variable increases to 1.24. The counterfactual forecast is 11.23, slightly smaller than the baseline. Column 5 adds demographic covariates (log fractions of the population that are over 65, black, and female) to the baseline regression; the interaction coefficient shrinks slightly to 0.094 but remains significant at 1%. Column

6 tests robustness to accounting for cycles in economic activity. While a natural control variable would be log GSP, it is well known that log GSP may be endogenous to shocks affecting a locality's health spending (Acemoglu, Finkelstein and Notowidigdo 2012), and therefore including it may bias the estimates of the effects of backlash regulations. Therefore, I construct a plausibly exogenous proxy for GSP by using the method of Bartik (1991): the GSP that would have obtained in each state had each sector of the state economy grown at the national growth rate for that sector since 1990. This measure assumes that the national growth rates for major sectors of the economy are uncorrelated with state-specific shocks to the health care share, conditional on state and year fixed effects and the regulation variables.[16] The coefficient estimates are nearly unchanged from the baseline, although the counterfactual forecast is no longer statistically different at 90% from the observed 2005 U.S. health care share.[17]

Robustness to the Dynamic Structure of Regulations

An essential robustness check to ensure that my results are not being driven by mean reversion, or by various forms of reverse causation, is to include leads and lags of my right-hand-side variables into the regression. Glied (2003) presents several theories of the rise in health care costs in the late 1990s and early 2000s, all of which argue that the health care spending slowdown in the 1990s was a product of a coincidence of transient factors (a low point in the underwriting cycle and strategic behavior of managed care firms during the health insurance market's transition to managed care in order to gain market share) that dissipated as the processes generating them reverted to the mean. Including leads and lags (together with contemporaneous effects) of the regulation variables into my regression helps control for mean reversion, and allows me to test an implication of the hypothesis that regu-

[16] The sectors I use are: agriculture, mining, construction, transportation and utilities (bundled together), durable goods manufacturing, nondurable goods manufacturing, wholesale, retail, services, and government. It is clear that they are sufficiently aggregated that no state accounts for a dominant share of any given sector.

[17] If log GSP were used directly as a control, the interaction coefficient would have declined to 0.88 (statistically significant at 1%) and the counterfactual forecast would have risen to 12.6%. However, as mentioned, there are substantial endogeneity concerns with using log GSP as a control. If the Bartik proxy for log GSP is used to instrument log GSP, the results are virtually identical to using the proxy directly as a control, with the first stage regression being identified and suggesting there is a statistically significant elasticity of 0.85 between log GSP and the Bartik proxy, conditional on state and year fixed effects as well as the regulation variables. A Hausman test also shows that log GSP is endogenous if the Bartik proxy is a valid instrument (p-value less than 0.001). I provide instrumental variables evidence that the backlash regulations are exogenous with respect to health share shocks in Table XIII.

lations are causing health care spending increases. Moreover, including leads and lags allows me to control for endogenous timing of the backlash regulations. For instance, if backlash regulations were passed in states with abnormally low health care share increases (because of aggressive cost containment that generated discontent), but then health care shares resumed rising (because of mean reversion), there would be a spurious positive correlation between lagged backlash regulations and current health care shares, and a spurious negative correlation between future backlash regulations and current health care shares. If the political response to the managed care backlash is causing changes in the health care share of GDP, it must be the case that when leads and lags of the regulations are included, the leads of the regulations are not significant conditional on the lags, while the lags are significant conditional on the leads. Table VI presents the results for the dynamic panel specification (1). We see that the coefficients on the leads are two to six times lower than the coefficients on the lags (the largest lead coefficient is 0.032, and the smallest lag coefficient is 0.067). If only one lead, one lag and the contemporaneous effect are included, the interaction coefficient on the first lag is statistically significant. If two leads and lags with contemporaneous effects are included, each of the lag interaction coefficients is about 0.07, but neither is significant individually. Since multicollinearity becomes severe as leads and lags are added, I perform joint F-tests that all leads are zero and joint F-tests that all lags are zero. We see that the coefficients on leads are always jointly insignificant, while the coefficients on lags are jointly significant at 2% with only one lead and lag, and at 5% with two leads and lags. Therefore, we have some reassurance that it is the lags and not the leads that are driving my results.

Robustness to Other Health Insurance Regulations

A significant concern is that the political response to the managed care backlash in general, and backlash regulations in particular, proxy for other changes in the policy environment that cause the health spending share to rise. As discussed in Section 3, during the backlash period, other health insurance reforms that did not directly target HMO cost containment mechanisms – mandated benefits, small group and individual market insurance reforms, tort reforms and Medicaid expansion – were being passed. It would be troubling both for my identification strategy and for my use of backlash regulations as a proxy for the intensity of the political managed care backlash if controlling for these political changes in the health insurance environment significantly altered my baseline estimates, and it would be reassuring for my approach if accounting for other health insurance reforms did not appreciably change my results. Table VII attempts to address this concern by including these regulations in my baseline dynamic panel specification (1) alongside with the backlash regulations. Column 1 reproduces the baseline. Columns 2 through 6 add mandated benefits, small group reforms, individual market reforms, state tort reforms and simulated Medicaid eligibility (both as levels and in interaction with HMO penetration) to the baseline regression, one at a time, respectively. Finally, Column 7 contains all the additional health insurance controls simultaneously (coefficients not reported). We see that the interaction coefficient on backlash regulations remains significant and unchanged in magnitude from the baseline specification, while the coefficients on the other health insurance reforms are insignificantly different from zero (with the exception of the individual market reforms). Moreover, the counterfactual forecasts under the hypothesis that no backlash regulations were passed are similar to the baseline.

Robustness to Reparametrization

I construct my backlash regulations variable as the raw total of backlash regulations passed in a state by a given year, counting separately regulations on different topics in a single bill, and counting separately multiple regulations on a single topic passed in different bills. While this does not appear to be an unreasonable method of creating a proxy for backlash regulations (for instance, if each provision requires the same amount of legislative effort, and legislative effort to pass backlash regulations is a good measure of the intensity of the political response to the managed care backlash in the given state), it is somewhat ad hoc, especially since different regulations may have impacted state health care shares differently. Therefore, I consider robustness to alternative formulations of the regulations variable. Column 1 reproduces the baseline. Column 2 counts years since 1994 in which any regulations were passed. This is a good proxy for political backlash intensity if all provisions passed in the same year are passed as a single bill, and any bill requires the same legislative effort. Column 3 replaces the regulation count with a dummy variable for whether the year in question is after the year in which the given state passed its largest number of regulations. Column 4 counts the number of types of regulations (out of the 27 types in Table I) passed in the given state by the given year. Column 5 replaces the backlash regulations variable with 27 dummy variables and their interactions with HMO penetration (estimates not reported), each indicating whether a particular type of regulation has been passed. We see that for all the tables, the counterfactual forecasts are similar to (and occasionally lower than) the baseline forecast, and the interaction coefficients, where available in aggregate, are statistically significant at 1%. Finally, column 6 decomposes the raw count of regulations passed into individual counts for the 4 broad categories of regulations (access, appeals, mandates and provider regulations). This specification nests the baseline specification (which would obtain if the coefficients on all categories of regulations were the same), but allows different categories of regulations to affect the health care share differently. We see that the largest and statistically significant coefficients are on provider regulations, which suggests that regulations affecting the relationship between managed care and physicians (such as bans on financial incentives for physicians to treat less intensively, or any-willing-provider laws) were particularly important, followed by mandates for services that managed care especially tried to curtail (e.g. minimum maternity stays), while regulations expanding patients' access to physicians and procedures may have actually lowered the health care share. [18] The forecast is almost exactly equal to the baseline forecast.

5.1.1 Robustness to Alternative Hypotheses about Change in The U.S. Health Care Industry

Two alternative hypotheses that may also explain the increase in health care spending in the early 2000s may be hospital consolidation over the 1990s and technological change. The U.S. hospital industry underwent a process of consolidation that many have attributed

[18] However, all of these inferences should be interpreted with caution because the type of backlash regulations passed may be correlated with other aspects of the managed care backlash, such as adverse media coverage of managed care, which may have led it to curtail its cost containment practices.

I have also estimated separate specifications for each of the four categories of regulations. For all of them, the effects are similar in significance to the baseline. However, these specifications cannot be interpreted causally as the passage of some backlash regulations is proxying for the passage of the omitted backlash regulations.

to a search for bargaining power against managed care insurers. It could well have been the case that consolidation was greater in states with greater HMO penetration, and that it was this consolidation that resumed the increasing trend of the U.S. health care share. In this story, states that experienced substantial consolidation also passed backlash regulations, but the political backlash had no causal role. Another story could be that the managed care revolution was overwhelmed by technological innovation in health care. Technological change is considered to be the major driver of health care spending (Newhouse 1992, Clemens 2013), and if the managed care revolution was only able to constrain resource use for given treatment technologies but not the adoption of more sophisticated technologies, states with high HMO penetration could have experienced particularly abrupt rises in health care spending as new medical technologies were introduced (these states, such as California and Massachusetts, are also U.S. technology leaders). While these hypotheses would presumably be captured by the inclusion of state trends in Table V, I specifically include them in specifications that I present in Table IX. Column 1 reproduces the baseline specification. Column 2 adds each state's hospital Herfindahl-Hirschmann index (computed as the sum of squared hospital expenditure shares for each hospital in the state multiplied by 100; this index is 0 if there are infinitely many hospitals each with an infinitesimal share of total expenditures and is 100 if there is a single hospital in the state that accounts for all the expenditures) both in levels and interacted with HMO penetration in 1995. Column 3 considers the baseline specification augmented by a different measure of tightness in hospital markets: log beds per capita (and its interaction with HMO penetration), which could be viewed as a proxy for slack in hte hospital industry (and hence, insurer market power).[19] Column 4 augments the baseline specification by the fraction of medical patents originating from that state which are patents of medical and surgical devices. I use data on U.S. patents from the NBER Patent Database (Hall, Jaffe and Traijtenberg 2001) extended to 2006 by the NBER Patent Data Project (PDP 2006). This variable is justified and discussed in great detail by Clemens (2014), who argues that innovation to medical devices, unlike pharmaceutical innovation, originates from

[19] I obtain the same results for other possible measures of slack in the hospital market, such as log admissions per capita. However, given that beds (and the space they take up) represent capital investment, it is plausible that they are harder to vary in the short run than admissions.

practitioners rather than from research firms with nationwide markets. Therefore, medical device innovation is local in nature and responds to local shocks (while pharmaceutical innovation responds to the nationwide market and does not react to state variation in the strength of the political managed care backlash). Clemens (2013) shows that medical device innovation as a fraction of all patents rose markedly in states that were substantially affected by the introduction of Medicare as opposed to states that were least affected.

Looking at the results in Table IX, I do not see evidence that the association I find between backlash regulations and health care spending increases is masking either one of these hypotheses. The coefficient on the interaction of backlash regulations and HMO penetration in 1995 is very close to the baseline no matter what controls are added. The coefficients on the controls are consistent with the stories that hospital concentration increases the health care share, that capacity (log beds per capita) decreases the health care share (perhaps because hospitals can be "held up" by the insurers) and that technology actually decreases the health care share; however, it is difficult to interpret these coefficients causally as it is unclear whether they are identified.[20]

Robustness to Regional Disaggregation[21]

I further run my regressions using sub-state variation. While backlash regulations vary at the state by year level, I can use disaggregated data on HMO penetration (from Baker and Phibbs [2002]), health spending and economic outcomes to add rich locational controls. Since only hospital spending data is available at the sub-state level (from the AHA Annual Survey), I can only look at total hospital spending, rather than at total health care spending. Moreover, because gross product data is not calculated for most sub-state units (in particular, for counties), I use county personal income as a measure of economic activity,

[20] In principle, if the technology or the market concentration controls are exogenous, then all the coefficients in the regression are unidentified. However, it is very unlikely that omitted variables bias is affecting the coefficients on the backlash regulations, because these coefficients are the same as in the baseline specification. There are no reasons, though, to believe that omitted variables bias is not affecting the magnitude of the coefficients on the controls.

[21] For the sub-state level regressions, the assumption that errors are not serially correlated within panel unit no longer appears to be valid, because the estimates without this assumption are different from the estimates that rely on this assumption. If analysis is performed using the difference specification (2), the estimates resemble closely those from DPNLS that do not rely on the no serial correlation assumption. Moreover, the standard errors from the difference specification (2) estimates increase markedly when they are clustered by panel unit in the sub-state analysis, whereas they are unaffected by clustering for the state-level analysis. Hence, for the sub-state robustness checks, I use the version of DPNLS that does not make the no serial correlation assumption.

which is different from gross output. Table X presents results when the unit of analysis is states, urban and rural counties of states agglomerated together (which I call MSU's), MSA's (with rural counties of a state combined into a single unit) and counties. For each unit of analysis, I include specifications with and without state trends. We see that since personal income is smaller than gross output, the shares are larger: the observed share of total spending out of U.S. personal income is 16.2%, and the forecast share without regulations is 14.9%. The first column reproduces the equivalent of my baseline specification with the new variables: the dependent variable is the change in health spending as a share of state personal income. The magnitude of the interaction coefficient is similar to the baseline estimate in Table III, and the forecast share is practically and statistically significantly smaller than the observed share. Subsequent specifications use the change in hospital spending as a share of personal income as the dependent variable. Each specification at each unit of analysis includes unit fixed effects (thus, the MSA specification has MSA fixed effects), and since a lagged dependent variable is included, these fixed effects approximate linear unit trends, which is a very flexible way of controlling for many time-varying covariates (demographics, economic conditions) at a local level. The interaction coefficient is typically between 0.03 and 0.06, which is reasonable given that hospital spending is approximately a third of total personal care spending. The counterfactual forecasts are all substantially lower than the observed hospital health share in 2005. This finding does not change when state trends are added (although the regression coefficients may change magnitude and significance, they do so in a way that does not increase the counterfactual forecast).

6 Results: Utilization and Health

6.1 Effects on Salaries, Stays and HMO Penetration

It is interesting to examine what aspects of the health care production function did the political response to the managed care backlash affect to raise health care costs. I obtain state-level data on employment and salaries in the hospital and ambulatory (physician office, outpatient center and home health) treatment settings from the Bureau of Economic Analysis (BEA), and aggregate them to get data on employment and salaries in the health care sector as a whole. Health care utilization is difficult to measure in the private sector because of a lack of centralized, consistent panel data; however, data on lengths of stay is available for hospital care (one-third of all health care spending) through the AHA Annual Survey, which provides data on hospital aggregates. Table XI presents results of estimating the dynamic panel specification (1) for a variety of dependent variables measuring expenditures, salaries, employment, utilization and HMO penetration. In addition to all the previously included statistics, I also compute, for each dependent variable, the growth of that variable between 1995 and 2005 that I attribute to the political backlash. Column 1 replicates the baseline. Column 2 sets hospital expenditures as a share of gross state product as the dependent variable. We see that the interaction coefficient is about 0.044 (which is reasonable given the fact that hospital expenditure is only 4.5% of GSP) and significant at 5%. Hospital expenditures as a fraction of GSP rose by over 25% (relative to the counterfactual level) during the managed care backlash. It is interesting to attempt to understand how this rise in hospital expenditures as a share of GSP was allocated between employment (as a share of population) and salaries (as a share of GSP per capita) in the health care sector. Column 3 shows results for the association between backlash regulations and health sector employment as a share of population.[22] We see that the interaction coefficient is positive and signficant; however, the main effect on backlash regulations is negative and large in magnitude. Jointly, the counterfactual change in health care employment attributable to the political managed care backlash is small: health care employment rose by 0.15 percentage points or 4.34% during the backlash. Column 4 shows estimates for the association between backlash regulations and average health sector salaries as a fraction of GSP per capita.[23] We see that the interaction coefficient is significant at 5% while the main effect is tiny, and that the observed 2005 average health sector salary as a fraction of GSP per capita is 14.10% higher than the counterfactual salary, the difference being statistically significant at 1%. However, analyzing employment and salaries across the entire health care sector masks interesting

[22] Health care employment is obtained from the State and Local Personal Income Indicators of the Bureau of Economic Analysis, specifically line 1600 of Table SA25N. Other BEA variables are obtained similarly.

[23] Health care salary as a fraction of GSP per capita is measured in percentage points; hence, a value of 100 indicates that the health care salary in the given time period and location is equal to GSP per capita. It is not surprising to see health care salaries less than GSP per capita because disposable income typically is less than GSP per capita. The fact that in some health care settings (e.g. medical offices) health care salaries exceed GSP per capita testifies to the fact that health care professionals are relatively well paid.

and important variation. Columns 5 and 6 show the effects of the political managed care backlash on employment and salaries, respectively, in the hospital sector, and columns 7 and 8 show effects on employment and salaries in the ambulatory (physician office) sector. Hospital salaries mainly reflect the salaries of nurses; ambulatory salaries should be good proxies for physician incomes because they are the salaries of people employed by physicians, and hence, their salaries should rise with the marginal revenue product of physicians. We see that in both sectors, provider salaries increased with the political backlash, by 11.2% and 19.3%. Employment behaved more heterogenously: hospital employment increased by 9.8%, the difference between the counterfactual forecast and the observed value in 2005 being statistically different from zero at 10% significance (although the interaction coefficient is not significant), but amublatory employment *decreased* by 9.2%. It is not clear why hospital employment rose while ambulatory employment fell with the political backlash, and given the large standard errors on the estimates and the forecasts, it is difficult to distinguish this pattern from the noise in the data. Hence, there is suggestive evidence that much of the rise in medical expenditures as a share of income went into higher relative salaries for medical workers rather than into increasing the fraction of the population in the medical sector. This tentative finding is consistent with the estimates of Cutler, McCllelan and Newhouse (2000), which suggest that managed care reduced the salaries of hospital medical providers.

One of the tools employed by managed care organizations to restrain health care costs was to limit the length of stay of patients in hospitals, and to transfer as many procedures to outpatient settings so as not to incur the fixed costs of hospitalization. Columns 9 and 10 present the effects of the political response to the managed care backlash on the length of a hospital stay (inpatient days divided by admissions) and on the number of inpatient days per capita (which also captures reductions in admissions per capita). We see that for both measures, the coefficients on the backlash regulations are positive (and the interaction coefficient is significant at 10% for the inpatient days per capita measure), and the counterfactual forecasts suggest modest increases in length of stay and inpatient days per capita on the order of 6-8%. The forecast on the length of stay is significantly different from the observed average length of stay in 2005, while the forecast on the number of inpatient days per capita

The magnitude of this effect is plausible given findings that managed care reduced hospital lengths of stay by over 15% (Glied 2000).

A final outcome that I consider is HMO penetration itself. In the baseline specification, I use state HMO penetration in the pre-period (1995) in the interaction term because HMO penetration would develop endogenously with shocks to health care spending. However, changes in HMO penetration may have been one of the channels through which the political backlash could have increased costs; a lower HMO share could have weakened competition between insurers to limit spending, and could have allowed for the U.S. health care spending share to rise. In particular, changes in HMO penetration may capture changes in intensity of treatment that are not reflected in the AHA data, which just reports hospital inputs rather than treatment choices. Column 11 shows that the political backlash not only decreased HMO penetration but also explains the full decline in HMO penetration after it peaked at 30% in 1999. HMO penetration was 22.8% in the U.S. in 2005, but under the counterfactual of no political managed care backlash, it is predicted to have been 34.3% that year, more than 10 percentage points higher, the difference being statistically significant at 5%.[24] Hence, a possible mechanism for how the political managed care backlash may have affected health care expenditures could have been that it dissuaded health insurers from starting new HMOs or expanding old ones, which weakened competition in the health insurance market as a whole.

6.2 Effects on Health

Given the strong association between managed care regulation and health care spending growth, it is interesting to consider whether the backlash regulations had any effects on health. The literature on the impact of managed care on health outcomes and health care quality (summarized in Miller and Luft 1997 and Glied 2000) has not found substantial deteriorations or improvements in health arising from managed care. Theoretically, health could even improve with the introduction of managed care if some costly medical procedures were unnecessary or mildly harmful. However, chronically ill and disadvantaged subpopu-

[24] It is easy to see from Figure 1 that the counterfactual forecast is still below what HMO penetration would have been if its growing trend in the 1990s continued linearly in the 2000s.

lations may be hurt if they have to change providers frequently because of transitions to and between managed care organizations. Miller and Luft (2002) present a meta-analysis of the health literature on managed care, suggesting that managed care organizations might be more effectve than conventional insurers for treating acute appendicitis and cancer, but that they may be less effective in treating patients with chronic conditions. In this paper, I present evidence consistent with this hypothesis: average mortality rates for people under 65 (the population most affected by the managed care revolution), if anything, rise with the intensity of the political response to the managed care backlash, but there is evidence that the backlash may have helped vulnerable subpopulations.

It is not clear how to measure health improvements unambiguously. Mortality is obviously one measure of (the lack of) health; however, mortality in the under-65 population is (fortunately) low, making it an indicator that is unlikely to show large changes because of the managed care revolution or the backlash against it. It is not clear that measures of the frequency of particular diagnoses and procedures, such as incidence of particular diseases or hospital admissions or re-admissions, should monotonically change as the population gets healthier; for example, hospital re-admissions may increase because patients who would otherwise have died on the first admission are saved, which should count as improving health. The incidence of particular diagnoses and procedures may also depend on institutional variables, such as incentives arising from insurance, and therefore may not be directly comparable across changes in the health insurance market. Therefore, in addition to mortality, I look at indicators of self-reported health and health behavioral limitations (whether someone quit their job because of health issues), which I obtain from the Current Population Survey. Self-reported health is available for every year since 1996, which limits my sample, but the indicator for quitting one's job because of health is available for my full sample.[25]

Table XII presents the impacts of the political response to the managed care backlash

[25] Strictly speaking, changes in the fraction of people in poor health, or in the fraction of people who quit their jobs for health reasons may also reflect improvements in health care that save people's lives (but allow them to live in poor health or without being able to work). However, the fraction of people under 65 who die each year is very small compared even to the fraction who report themselves in poor health (usually it is a tenth of that fraction), so it is unlikely that the interpretation of our results for these variables is being driven by composition bias. In results not reported, I have re-estimated these specifications with the dependent variable being formed as (share in poor health) + (mortality rate as fraction of population), or (share quit job because of health) + (mortality rate as fraction of population) and obtained the same results.

on these health measures. Column 1 presents evidence for all-cause age-adjusted mortality for people under 65 (in counts per 100,000) using the dynamic panel specification (1). We see that the forecast mortality rate in 2005 if backlash regulations were absent is lower than the observed rate by about 5%, the one-sided difference barely failing to be significant at the 95%. The interaction coefficient is positive and significant at the 10% level, suggesting that the political backlash may have raised mortality in states with high HMO penetration relative to states with low HMO penetration.[26] Hence, the political managed care backlash, if anything, raised the mortality of the affected population on average. However, this effect was likely not distributed equally across the population. Columns 2 and 3 show effects of the political backlash on the percentage of people reporting themselves to be in excellent health (top health score) and poor health (bottom health score) in the CPS. We see that the results for people in excellent health (column 2) parallel the mortality results; the political backlash appears to decrease the fraction of people in excellent health, though the difference between the observed and counterfactual values for 2005 are not significant. However, Column 3 suggests that the political managed care backlash may have *decreased* the fraction of people in poor health by about 0.09 percentage points, or 3.7% of its counterfactual level. Moreover, Column 4 suggests that the backlash decreased the fraction of people quitting their jobs for health reasons by 8.2%, the difference barely failing to be statistically greater than zero at 5%.[27] Hence, it is plausible that the political backlash was actually beneficial for people in poor health or for people whose health problems were sufficiently chronic (or whose jobs were sufficiently strenuous) that they could have to quit their jobs because of health problems.

While these estimates appear to agree with the literature on the health effects of managed care, they cannot be interpreted as welfare assessments of the political response to the managed care backlash. The proxies used to measure health are very imperfect, and a

[26] Under the assumption of a 12 trillion U.S. GDP, a 260 million U.S. population of under-65 year olds and a value of a statistical life of $5 million (consistent with Cutler 2004), the upper confidence bound on the counterfactual mortality rate implies that the loss in VSL from counterfactual increased mortality is 4.9 billion dollars, which is much less than the 235 billion dollar counterfactual decrease in health expenditures. However, given that mortality reduction may be only one of the health benefits of the political managed care backlash, this calculation cannot be seen as an indication of the overall welfare effect of the political backlash. In fact, as we see from impacts on people in poor health, there were additional negative effects of the managed care backlash that are hard to monetize.

[27] Considering the effects of the backlash on the fraction of people who either quit their jobs because of health or died would avoid the concern that the backlash may have decreased this measure by raising mortality. Such a specification yields a similar percentage difference between the observed and counteractual levels of the quit rate plus mortality variable (8%).

large fraction of the utility gains or losses to consumers from the political backlash could have come from factors that did not directly affect their health, such as the ability to choose their own doctor, or not to have to haggle with an impersonal bureaucracy over which procedure they could get.[28] It is, however, unclear how such utility gains or losses can be measured given the paucity of data on the relevant variables and the substantial deviations of the market for health insurance from classical theory.

7 Political Determinants of Regulations and Instrumental Variables Estimation

Since I argue that it was specifically the political response to the managed care backlash, mediated through the passage of the backlash regulations, that increased health care expenditures, it is natural to ask whether political variables explain backlash regulations. Moreover, if it can be argued that these political variables could have impacted the health care market only through the passage of backlash regulations, it would be possible to test my identification strategy further by using the political variables as instruments. Obtaining valid instrumental variables estimates for the effect of the political managed care backlash on health care costs that matched with the OLS estimates presented in the baseline results would be reassuring confirmation of the validity of the central findings of my paper.

The political variables I will be using for most of my analysis will be numbers of years of Democratic control of the state governorships, upper houses of state legislatures, and lower houses of state legislatures since 1994 (since the first large wave of backlash regulations came in 1995). There is good reason on the basis of the health policy literature to believe that Democrats were more favorably disposed to backlash regulations than Republicans were. When the U.S. House of Representatives voted on the Bipartisan Consensus Managed Care Improvement Act (H.R. 2723) in 1999 (also known as the Norwood-Dingell Act), which would have imposed a federal version of the backlash regulations (including managed care liability for poor health outcomes resulting from denials of care), all but five

[28] For example, Miller and Luft (1997) report that consumer satisfaction with care tends to be lower in managed care plans than in conventional plans. While such an association is not necessarily causal, none of the health measures used so far would capture the disutility that consumers get from dissatisfaction with their health plan.

Democrats voted for passage, while nearly three-fourths of Republicans voted against passage (author's calculations from Poole and Rosenthal 2012). Brodie et al. (1998) and Gray et al. (2007) provide evidence that self-identified Democrats were more likely to support backlash regulations. However, there were exceptions: the Texas Health Care Liability Act, one of the most comprehensive pieces of backlash legislation, was passed in Texas in 1998 with the strong support of the Republican governor, George W. Bush. Since there are three parts of the state government whose control I can assign to a party, I create multiple variables for Democratic control of the various combinations of parts of the state government. Moreover, motivated by the example of Texas, I include interactions of the Democratic control variables with an indicator that the state in question is a Southern state, since the relative Democratic propensity to support backlash regulations there is very different than in the rest of the country. I describe in detail my procedure for parametrizing the Democratic control variables in Online Appendix I, and I provide tentative evidence that Democratic control outside the South increases the passage of backlash regulations, although the individual coefficients are imprecisely estimated.

An objection to this identification strategy could be that Democratic control of branches of the state government could affect health care costs through other legislation that affects the economy as a whole. Hence, I also investigate a more conservative instrumental variables strategy in which I use the interaction of the Democratic control variables described above with a time-constant measure of physician dominance of health interest groups (specifically, the fraction of health lobby registrations by primary care clinic organizations from Gray et al. 2007) as instrumental variables, and include the Democratic control main effects as exogenous regressors. From the discussion in Section 2, we see that physicians were vocal opponents of managed care cost containment practices, both because these practices interfered with the clinical practices that they were accustomed to and that were parts of their training, and because managed care adversely impacted medical provider salaries (Cutler, McClellan and Newhouse 2000, Section 6 this paper). Gray et al. (2007) finds that physician dominance in the early period of the backlash is correlated with the subsequent passage of backlash regulations in a cross section of states. Therefore, we should expect physician dominance of health interest groups to make it easier for state governments to pass backlash regulations, all else the same. The identification assumption becomes that the only way in which Democratic control of the state government could differentially affect the health care share as a function of pre-period physician dominance of health interest groups is through the passage of backlash regulations. Since it is very implausible that Democrats would have a propensity to pass legislation that does not affect the health care market directly in a way that varies with physician dominance of interest groups, we no longer have the concern that Democrats may have passed non-health-related legislation with indirect effects on the health care market.[29] Physician dominance could be endogenous to the health share, but using measures of physician dominance for the pre-backlash period and the early backlash period should ameliorate this problem.

Table XIII presents instrumental variables estimates of specification (1) based on the two instrumental variables strategies I propose. To estimate Table XIII, I use instrumented DPNLS by exploiting the exclusion restrictions implied by the excluded political instruments and the regressors assumed to be exogenous. Since there are many instruments in each regression, I simply note what groups of instruments are included, and present the first stages in Online Appendix I. Column 1 reproduces the baseline results. Column 2 instruments backlash regulations and backlash regulations interacted with 1995 HMO penetration using the Democratic control variables only, both as main effects and interacted with the South dummy. We see that the interaction coefficient drops and loses significance, though the counterfactual forecast is consistent with a large effect of the backlash regulations. Column 3 executes the more conservative identification strategy and instruments the two backlash regulation variables with Democratic control-physician dominance interactions only (with or without the South dummy). The main effects of the Democratic control variables are included as exogenous variables in the regression in order to isolate the variation coming from the interaction terms. We see that now the interaction coefficient is slightly larger than the baseline (0.129), statistically significant at 5%, and the counterfactual forecast is 10.67%,

[29] It still could be the case that physician groups differentially influenced Democrats' ability to pass other health insurance regulations that were not related to the backlash. However, we have seen in Table VII that of the major health insurance regulations passed during the backlash period, only the backlash regulations appear to affect health care costs.

much lower than the baseline counterfactual forecast, though statistically insignificantly different from the observed 2005 U.S. health care share. Hence, it is likely that whatever failure of endogeneity we obtain when using Democratic controls as instruments biases our estimates downward, and we recover our initial estimates once we use the more conservative identification strategy.

To run specification tests, I note that for all sets of estimates, I cannot reject the null hypothesis that the autoregressive coefficient is equal to unity, and for most of the specifications it its estimated value is very close to unity. Hence, I estimate specification (1) by replacing the dependent variable with the difference in the health care share, dropping the lagged dependent variable, and estimating the resulting equation by fixed-effects OLS or 2SLS (which is a valid procedure if the autoregressive coefficient is equal to unity), essentially estimating equation (2) by 2SLS. Since this is a more efficient way to estimate specification (1) if the autoregressive coefficient is indeed equal to unity, the overidentification and Hausman tests will be more likely to reject their null hypotheses. Nevertheless, for both of my instrumental variables regressions, the overidentification test fails to reject the null hypothesis that my model is overidentified, and the Angrist-Pischke underidentification test shows that my excluded instruments are correlated with the instrumented regulation variables even in the presence of the exogenous controls and fixed effects. Finally, and the Hausman test for the endogeneity of the regulation variables fails to reject the null hypothesis that they are both exogenous. Hence, including instrumental variables merely increases the variance of my estimates (in fact, the estimates in Column 3 of Table XIII have substantially higher variance than do the baseline estimates in Column 1), and is inefficient, which justifies ex post my use of specification (1) as the baseline specification in this paper.

Therefore, under either of my identification assumptions, it is clear that endogeneity in the backlash regulations is not likely to affect my finding that the political response to the managed care backlash increased health care spending, and in particular, it is clear that that the backlash regulations are most likely exogenous with respect to shocks to the health care share of GSP, justifying my previous techniques. We also obtain a tentative story for an aspect of the political system's role in the passage of backlash regulations: the Democratic party, at least outside the South, was relatively more likely to pass such regulations than the Republican party was, and the presence of physician-dominated health interest groups increased this party differential in backlash regulation passage. [30]

8 Conclusion

This paper finds that the political response to the managed care backlash of the late 1990s, as measured by state regulation of managed care cost containment practices, has increased the U.S. health care spending share of GDP by nearly 2 percentage points, and accounts for much of the growth in the health care share of GDP since the health care expenditure growth stagnation of the 1990s. This result is robust to a variety of specification checks, which, in particular, rule out alternative explanations based on neglected geographic heterogeneity, mean reversion, confounding with other health insurance policies, and trends in health care market competition. There is suggestive evidence that the backlash operated by increasing provider salaries, preventing hospital stays from shrinking, and limiting further spread of the HMO business model. I further show that there were no statistically significant mortality improvements caused by the managed care backlash, although the chronically ill may have seen improved health from it. Finally, I present evidence that political variables can explain part of the variation in the backlash regulations, and exploit this observation to execute an instrumental variables strategy.

Given that the magnitude of the health expenditure increase that I attribute to the political managed care backlash is comparable to the sizes of the major U.S. public health insurance programs, it is worth studying the phenomenon of the political backlash in greater detail. While a great deal of qualitative research has been done on the backlash in the health policy literature, very little analysis has been done on the backlash in public economics. It is important to understand precisely what components of the political response to the managed care backlash (media or regulatory) had the largest effects on health care spending, and what channels did the backlash operate through to raise the U.S. health care

[30] Since I do not make any normative claims on whether passing the backlash regulations was welfare-improving or welfare-deteriorating, this finding does not suggest that either party's position was the one consistent with welfare maximization. While I find evidence that backlash regulations increased health care costs, I cannot quantify evidence on the benefits of backlash regulations for health and peace of mind of patients, and therefore, cannot judge whether the benefits exceeded the costs.

share. It is also important to understand better why some states experienced a much stronger level of backlash than did others. Additionally, my finding highlights the importance of studying health care spending dynamics in the private sector, especially given the Affordable Care Act's emphasis on using government subsidies to private insurance to achieve universal coverage.

Furthermore, my findings emphasize the importance of studying the virtues and defects of different managed care cost containment mechanisms. We cannot quantify the many inconveniences – reduced choice of treatment strategy, inability to see a doctor one has been accustomed to, unpredictability of utilization review committees – that managed care created for its patients, and therefore cannot trade them off against the declines in spending. Suggestive evidence from looking at the effects of different types of regulations hints that some of these hardships could have been regulated away without substantial tradeoffs, and that most of the decreases in spending from managed care occurred from its ability to influence physicians rather than patients. In light of the inclusion of managed care into the Affordable Care Act through ACOs, it is imperative to understand what particular aspects of the managed care program created value for its customers so that it could be possible to improve on the managed care model in the future.

Appendix I: More on Instrumental Variables – FOR ONLINE PUBLICATION

To parametrize the extent of Democratic control during the backlash period, I create 7 variables in total for the 7 combinations of Democratic control that can obtain in any given year.[31] Each variable is the number of years since 1994 that the state government experienced the particular configuration of Democratic control.The omitted variable is the number of years since 1994 that Democrats have controlled no part of the state government. Since the dependent variable is the total number of regulations outstanding in a given state by a given year, it makes sense to look at the cumulative number of years of Democratic control rather than at whether Democrats control the state government at the given point in time. The main motivation for such a parametrization is that if support for backlash regulations was partisan, then the Democratic control variables span the possible combinations of partisan control of the state government, and therefore, flexibly capture any influences of partisan control.

Column 1 of Table A1.1 shows the regression of backlash regulations on the 7 Democratic control variables. We see that while the coefficients of these variables have different signs, one year of Democratic control of any combination of the branches of a state government increases the number of backlash regulations.[32] However, none of the coefficients is significant, and the 7 Democratic coefficients are insignificant jointly. The explanation for this failure of statistical significance is that the relative support of the Democratic party for backlash regulations was not homogeneous across the United States. Motivated by the Texas example, in which a Republican governor supported backlash regulations, in column

[31] Hence, these variables are the numbers of years since 1994 that Democrats have controlled 1) the governorship, 2) the upper house, 3) the lower house, 4) both the upper house and the lower house, 5) both the governorship and the upper house, 6) both the governorship and the lower house, and 7) the governorship, the upper house and the lower house all together.

[32] To see this, consider a configuration of Democratic control, e.g. governor and upper house. An extra year of this configuration of Democratic control will have an impact on regulations equal to the coefficient for a Democratic governor, plus the coefficient for a Democratic upper house, plus the coefficient for the combination of a Democratic governor and a Democratic upper house. We see that this sum is greater than zero. A similar analysis can be done for all other configurations.

2, I present the regression of backlash regulations on the 7 Democratic control variables as main effects, and on 7 interactions between Democratic control variables and a dummy variable indicating that the state in question is a Southern state. The specification in column 2 explicitly allows for differences in relative Democratic support for backlash regulations between the South and the rest of the U.S. [33] We see that an additional year of Democratic control of any configuration of state government branches increases the number of backlash regulations outside the South (with the exception of just the control of the lower house), but not necessarily in the South. Most importantly, we see that the 14 Democratic controls with interactions for the South are jointly significant, and therefore, help explain the passage of backlash regulations.

Tables A1.2 and A1.3 provide some intuition concerning the relationship between backlash regulations, Democratic control, and pre-period physician dominance. Since in a specification with Democratic main effects, Democrat-South interactions, Democrat-physician dominance interactions, and Democrat-physician dominance-South interactions, there are 28 different coefficients, I present these coefficients in columns 3 and 4 of Table A1.1 but do not discuss them. Instead, Table A1.2 provides the p-values that all regressors are zero, and the p-values that all regressors with physician dominance interactions are zero for four specifications that explain backlash regulations with the variables discussed. We see that controlling for differences in Democratic relative support for backlash regulations between the South and the rest of the U.S. is crucial for joint significance of all regressors. We also see that the Democrat-physician dominance interactions (with and without South dummy interactions) are significant even when Democrat main effects are included in the regression. In fact, these interactions are significant at 10% even when South dummy interactions are not included (they are significant at 1% when they are included). Therefore, the political variables I have identified have explanatory power for backlash regulations, and specifically, there appear to be statistically significant differential effects on relative Democratic propen-

[33] There are two reasons why the relationship between Democratic control of the state government and the passage of backlash regulations could have been different in the South as compared to the rest of the United States. First, the Democratic and Republican parties were much more similar in the South than they were nationally in the 1990s – many Southern Republicans had earlier been Democrats, and many Southern Democrats were maintaining their party affiliation by force of habit rather than because of substantial agreement with the nationwide Democratic party. Second, the 1990s saw a transition from virtually solid Democratic state government in the South to a substantial presence of Republicans, which created further policy convergence because of political competition.

sity to pass backlash regulations as a function of pre-period physician dominance, so there is variation to exploit for my second, more conservative identification strategy. Unfortunately, Table A1.2 does not provide good information for the direction of the effects: on whether Democrats are more inclined to support backlash regulations relative to Republicans, and on how pre-period physician dominance affects this relative support. Therefore, Table A1.3 presents the coefficients for regressions when each Democratic control indicator is analyzed separately. Each regression has four variables: the Democratic control in question, the Democratic control interacted with the South dummy, the Democratic control interacted with pre-period physician dominance, and the triple interaction of all three variables. No variable in any regression is statistically significant, so this exercise should be interpreted as, at most, illustrative. We see that in all the regressions, the Democratic main effect is positive, suggesting Democrats pass more backlash regulations outside the South than Republicans do, as expected. The Democrat-South interaction is negative in all but one of the specifications, suggesting this effect is decreased or reversed in the South, also as expected. The Democrat-physician dominance interaction is positive in all but one specification, suggesting that physician dominance of health interest groups increased the relative Democratic propensity to pass backlash regulations outside the South. This is expected, because it is likely that the efforts of physician groups and of Democrats to pass backlash regulations were supermodular (since physician groups could mobilize grassroots support for the regulations, while Democrats could vote the regulations into law). Hence, physician interest groups were more capable of getting backlash regulations passed when Democrats were in office than when Republicans were. Finally, the triple interaction coefficient is sometimes positive and sometimes negative. However, there is no reason to expect this coefficient to be of a particular sign; in the South, physician interest groups may have been especially helpful in increasing the differential Democratic propensity to pass backlash regulations because this differential propensity was low to begin with, or they may have had less of a differential effect on Democratic passage of regulations because both parties were sufficiently similar to begin with. Hence, we have evidence that there exists experimental variation in backlash regulations that I can exploit for an instrumental variables strategy, and we have suggestive evidence for a story that backlash regulations were passed more frequently by Democrats than by Republicans, with this differential increased in the presence of physician-dominated health interest groups.

Table A1.1: Determinants of Regulations				
Dep. Var. is # Backlash Regulations				
	(1)	(2)	(3)	(4)
Governor	.442	.301	.607+	.441
	(.336)	(.310)	(.360)	(.400)
Upper Hse	.331	.170	1.068	-.075
	(.381)	(.297)	(1.241)	(.552)
Lower Hse	.456	-.061	3.307+	-3.483*
	(.588)	(.367)	(1.694)	(1.542)
State. Leg.	-.499	.314	-3.985	3.917*
	(.736)	(.485)	(2.489)	(1.696)
Ctrl. All	-.139	-.963	2.077	-6.243**
	(1.029)	(.779)	(3.157)	(1.997)
Gov.+ UH	-.444	-.254	-1.498	-.201
	(.608)	(.481)	(1.377)	(.605)
Gov.+ LH	.148	.915	-1.626	5.928**
	(.863)	(.636)	(2.496)	(1.815)
Governor X South		.721		-1.039*
		(.480)		(.508)
Upper Hse X South		1.428		21.965**
		(1.720)		(3.751)
Lower Hse X South		3.317**		-41.584*
		(1.088)		(19.545)
State. Leg. X South		-5.203*		20.185
		(2.334)		(22.784)
Ctrl. All X South		7.381+		-25.088
		(4.178)		(25.929)
Gov.+ UH X South		-1.239		-19.244**
		(2.071)		(3.032)
Gov.+ LH X South		-6.801*		43.992+
		(2.755)		(22.645)
Governor X Phys. Dom.			-.096	-.107
			(.108)	(.116)
Upper Hse X Phys. Dom.			-.266	.093
			(.392)	(.188)
Lower Hse X Phys. Dom.			-1.489+	1.654*
			(.873)	(.705)
State. Leg. X Phys. Dom.			1.706	-1.704*
			(1.089)	(.760)
Ctrl. All X Phys. Dom.			-.649	2.910**
			(1.654)	(1.110)
Gov.+ UH X Phys. Dom.			.465	.100
			(.424)	(.285)
Gov.+ LH X Phys. Dom.			.584	-2.876**
			(1.499)	(1.058)
Governor X Phys. Dom. X South				.781**
				(.192)
Upper Hse X Phys. Dom. X South				-16.252**
				(2.828)
Lower Hse X Phys. Dom. X South				60.523*
				(25.610)
State. Leg. X Phys. Dom. X South				-45.920
				(27.957)
Ctrl. All X Phys. Dom. X South				47.355
				(28.893)
Gov.+ UH X Phys. Dom. X South				14.861**
				(2.612)
Gov.+ LH X Phys. Dom. X South				-60.689*
				(26.488)
Number of Obs.	550	550	550	550
Number of Clusters	50	50	50	50
R^2	.87	.89	.88	.91
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes

(A1.1)

Table A1.2

Determinants of Regulations: Full Specifications				
Dep. Var. is # Backlash Regulations				
	(1)	(2)	(3)	(4)
Dem. Ctrls.	Yes	Yes	Yes	Yes
Dem. Ctrls. X South	No	Yes	No	Yes
Dem. Ctrls. X Phys. Dom.	No	No	Yes	Yes
Dem. Ctrls. X Phys. Dom. X South	No	No	No	Yes
Number of Obs.	550	550	550	550
Number of Clusters	50	50	50	50
R^2	.87	.89	.88	.91
P-value All Regressors are Zero	.41	.00	.15	.00
P-value Phys. Dom. Intracts. are Zero			.07	.00
StateFE	Yes	Yes	Yes	Yes
YearFE	Yes	Yes	Yes	Yes

(A1.2)

Standard errors clustered by state in parentheses. Data on state regulation of managed care obtained from the NCSL. The regulations variable is the sum of all regulations in force in the given state and year. Data on the percentage of state population enrolled in HMOs in 1995 is obtained from the Statistical Abstract. Data on health expenditures and GSP from CMS. Data on Democratic control obtained from the Statistical Abstract of the United States. Data on physician dominance of health interest groups (fraction of health lobby organizations by primary care clinic organizations obtained as personal communication from Virginia Gray.

Table A1.3

Determinants of Regulations: Demonstration *Dep. Var. is # Backlash Regulations*							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dem. Ctrl. Type	Dem. Gov.	Dem. U. Hse	Dem. L. Hse	Dem. Gov. + UH	Dem. Gov. + LH	Dem. State. Leg.	Dem. Cntrl All
Dem. Cntrl.	.263	.064	.202	.027	.325	.063	.092
	(.236)	(.219)	(.246)	(.233)	(.397)	(.239)	(.373)
Phys Dom. X Dem. Cntrl.	-.026	.024	.005	.068	.042	.050	.082
	(.055)	(.071)	(.101)	(.076)	(.183)	(.092)	(.154)
Dem. Cntrl. X South	-.226	-.221	.027	-.798	-1.074	-.252	-1.053
	(.680)	(.416)	(.493)	(.858)	(.918)	(.491)	(.995)
Phys Dom. X Dem. Cntrl. X South	.177	.075	-.070	.266	.423	-.040	.428
	(.181)	(.188)	(.479)	(.238)	(.400)	(.469)	(.396)
Number of Obs.	550	550	550	550	550	550	550
Number of Clusters	50	50	50	50	50	50	50
R^2	.86	.86	.86	.86	.86	.86	.86
StateFE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
YearFE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

(A1.3)

Standard errors clustered by state in parentheses. Data on state regulation of managed care obtained from the NCSL. The regulations variable is the sum of all regulations in force in the given state and year. Data on the percentage of state population enrolled in HMOs in 1995 is obtained from the Statistical Abstract. Data on health expenditures and GSP from CMS. Data on Democratic control obtained from the Statistical Abstract of the United States. Data on physician dominance of health interest groups (fraction of health lobby organizations by primary care clinic organizations obtained as personal communication from Virginia Gray.

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9 Tables

Table I

Backlash Regulation Type (Fine Grouping)	Coarse Grouping	Number of Regulations of given Type
Comp. Consumer Rights	Access	68
Continuity of Care	Access	40
Direct Access, OB/GYN	Access	48
Direct Access, other	Access	21
Emergency Care Coverage	Access	39
Emergency Room	Access	3
Emergency Prudent Lay Person	Access	23
Ombudsman	Access	21
Specialist as PCP	Access	10
Standing Ref. To Specialist	Access	28
Insurer Liability	Appeals	14
Independent External Review of Denials	Appeals	58
Liability, Financial: Enrollee	Appeals	16
Liability: Provider Contracts	Appeals	26
Point of Service	Appeals	21
Diabetes Supplies	Mandates	54
Hospital Stay after Childbirth	Mandates	42
Inpatient Care after Mastectomy	Mandates	22
Post-Mastectomy Breast Reconstruction	Mandates	10
Off-label Prescription Drug Use	Mandates	18
Any Willing Provider	Provider	16
Ban All Products Clauses	Provider	6
Ban on Financial Incentives	Provider	38
Ban on Gag Clauses	Provider	57
Freedom of Choice	Provider	9
Medical Director Requirements	Provider	26
Report Cards	Provider	27

(I)

Table II

Summary Statistics, State-Level Data

VARIABLES	(1) N	(2) Mean	(3) SD	(4) Min	(5) Max
Personal Health Share of GSP, %	50	14.28	2.408	9.462	20.59
Change, Personal Health Share of GSP, %	50	0.104	0.357	-1.259	0.847
Backlash Regulations (T-1)	50	15.06	5.339	2	33
HMO Penetration in 1995, %	50	14.54	10.17	0	40.00
Backlash Regulations (T-1) X HMO Penetration (1995)	50	2.271	1.752	0	7.200
Log Gross State Product	50	11.90	1.046	10.03	14.34
Backlash Regulations (T-1), Access	50	8.220	3.466	0	20
Backlash Regulations (T-1), Appeals	50	1.120	1.003	0	4
Backlash Regulations (T-1), Mandates	50	2.800	1.485	0	7
Backlash Regulations (T-1), Provider	50	2.920	1.469	1	9
Mandated Benefits, Count (T-1)	50	15.24	5.446	4	27
Small Group Full Reform (T-1)	50	0.720	0.454	0	1
Indiv. Mrkt. Full Reform (T-1)	50	0.180	0.388	0	1
State Tort Reform, Count (T-1)	50	5	1.990	1	9
Simulated Medicaid Eligibility (T-1)	50	0.426	0.0982	0.279	0.712
Hospital HHI, Expenditure-Based	50	0.0489	0.0477	0.00593	0.250
Log Hospital Beds per Capita	50	-1.107	0.271	-1.560	-0.463
Fraction Patents in Medical Devices	49	0.0390	0.0504	0	0.250
Hospital Expenditures, Share of GSP, %	50	4.942	1.125	3.200	8.550
Hospital Employment as Share of Population, %	50	1.739	0.334	1.087	2.807
Average Hospital Salary as Share of GSP per Capita, %	50	150.7	20.59	96.60	203.3
Ambulatory Employment as Share of Population, %	50	2.129	0.284	1.602	3.145
Ambulatory Average Salary as Share of GSP per Capita, %	50	120.9	19.96	75.41	165.5
Average Inpatient Days Per Admission (Length of Stay)	50	6.746	1.276	4.773	10.18
Average Inpatient Days Per Capita	50	0.843	0.220	0.498	1.439
HMO Penetration, %	50	17.59	10.94	0	49.10
All-Cause Under-65 Mortality Rate per 100,000	50	241.1	46.78	172.7	364.6
Fraction Under-65 in Excellent Health, %	50	36.82	4.101	26.97	45.52
Fraction Under-65 in Poor Health, %	50	2.489	1.099	0.916	5.731
Fraction Under-65 Quit or Retired from Job Because of Health, %	50	2.749	0.816	1.371	5.172

(II)

Data on state regulation of managed care obtained from the National Conference of State Legislatures. The regulations variable is the sum of all regulations in force in the given state and year. Data on the percentage of state population enrolled in HMOs in 1995 is obtained from the Statistical Abstract, originally from Interstudy. Data on health expenditures and GSP from CMS. Data on hospital expenditures, payrolls, employment, lengths of stay and facilities from the AHA Annual Survey. Data on ambulatory employment and salaries from the BEA. Data on other health insurance regulations from "State. Legislative Health Care and Insurance Issues" by BCBS, from Avraham (2010) and from Kosali Simon. Data on mortality from the CDC. Data on self-reported health status from CPS. Data on patents from the NBER Patent Database.

Table III

Baseline Estimates *Dynamic Panel Specification*				
	(1)	(2)	(3)	(4)
	Total Share	Private Share	Medicare Share	Medicaid Share
Lag. DV	1.005***	.991***	.950***	.924***
	(.049)	(.037)	(.049)	(.068)
Regs (T-1)	-.007	-.009	-.001	.003
	(.007)	(.006)	(.001)	(.003)
Regs (T-1) X HMO (1995)	.119***	.110***	.011**	-.011
	(.033)	(.021)	(.005)	(.008)
Observed level in U.S. (2005)	13.48	8.59	2.59	2.29
Forecast w/o Regulations	11.52*	7.16**	2.53	2.22
90% CI Upper Bound	13.25	8.34	2.80	2.60
90% CI Lower Bound	9.83	6.05	2.27	1.80
Number of Obs.	550	550	550	550
Number of Clusters	50	50	50	50
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes

(III)

Each column presents results from estimating equation (1) with suitable covariates. Bootstrapped standard errors clustered by state in parentheses. Data on state regulation of managed care obtained from the NCSL. The regulations variable is the sum of all regulations in force in the given state and year. Data on the percentage of state population enrolled in HMOs in 1995 is obtained from the Statistical Abstract, originally from Interstudy. Data on health expenditures and GSP from CMS. Private health expenditures are defined as the difference between total expenditures and Medicare and Medicaid expenditures.To compute the 2005 forecast, I draw 500 independent observations from the distribution of the coefficient vector and dynamically simulate the counterfactuals of all regulation variables being set to zero.

Table IV

Specification Analysis

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dep. Var.	Total Share	Total Share	Total Share	Total Share	Total Share	Total Share	Total Share	Diff Total Share
Lag. DV		.799***	.858***	1.030***	.929***	1.005***	1.032***	
		(.032)	(.034)	(.017)	(.040)	(.049)	(.065)	
Regs (T-1)	.002	-.006	-.000	-.002	-.007	-.007	-.007	-.008
	(.026)	(.007)	(.009)	(.004)	(.007)	(.007)	(.007)	(.006)
Regs (T-1) X HMO (1995)	-.196*	.050	.048	.013	.093***	.119***	.128***	.112***
	(.101)	(.031)	(.037)	(.018)	(.030)	(.033)	(.036)	(.023)
Method	OLS	OLS	AB	BB	NLS	DPNLS	DPNLS, GMM	OLS
No. Observations	550	550	550	550	550	550	550	550
No. Clusters	50	50	50	50	50	50	50	50
R^2	.94							.38
P-value of Hausman Test against Col. 4							.99	
Observed 2005 Dep. Var. in U.S.	13.48	13.48	13.48	13.48	13.48	13.48	13.48	13.48
Forecast 2005 Dep. Var. in U.S. if no Regulations	14.27	13.28	12.83	13.53	12.48	11.52*	11.06*	11.92***
90% CI Upper Bound	14.46	14.11	14.17	14.63	13.74	13.25	13.17	12.88
90% CI Lower Bound	14.08	12.50	11.55	12.49	11.30	9.83	9.01	10.95
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

(IV)

Bootstrapped or asymptotic standard errors clustered by state in parentheses. Data sources as in Table III. See text for description of the specifications.

Table V

	(1)	(2)	(3)	(4)	(5)	(6)
Robustness Checks *Dynamic Panel Specification* *Dep. Var. is Total Health Share*						
Spec.		Region Trends	Region-Year FE	State Trends	Demo Graph.	GDP (Bartik)
Lag. DV	1.005***	1.035***	.967***	1.237***	1.009***	1.008***
	(.049)	(.038)	(.053)	(.004)	(.037)	(.060)
Regs (T-1)	-.007	-.009	-.009	-.013	-.005	-.008
	(.007)	(.006)	(.006)	(.015)	(.006)	(.008)
Regs (T-1) X HMO (1995)	.119***	.132***	.107***	.109	.094***	.117***
	(.033)	(.028)	(.034)	(.067)	(.025)	(.027)
Observed level in U.S. (2005)	13.48	13.48	13.48	13.48	13.48	13.48
Forecast w/o Regulations	11.52*	11.19**	12.15	11.23	11.85**	11.60
90% CI Upper Bound	13.25	12.88	13.71	14.78	13.14	17.31
90% CI Lower Bound	9.83	9.61	10.65	7.34	10.63	7.11
Number of Obs.	550	550	550	550	550	550
Number of Clusters	50	50	50	50	50	50
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes

(V)

Each column presents results from estimating equation (1) with suitable covariates. Bootstrapped standard errors clustered by state in parentheses. Regulations, costs and GSP data as in Table III. Data on industrial composition of GSP to construct Bartik proxy from BEA. Column 3 includes demographic controls for (log) proportion of the population over 65 (in Medicare), proportion black and female, proportion black and male, proportion white and male, and proportion white and female. Column 6 includes a Bartik-style proxy for log GSP per capita as a control. To compute the 2005 forecast, I draw 500 independent observations from the distribution of the coefficient vector and dynamically simulate the counterfactuals of all regulation variables being set to zero.

Table VI

Leads and Lags *Dynamic Panel Specification* *Dep. Var. is Total Health Share*				
	(1)	(2)	(3)	(4)
Lag Structure:	-1	0	-1/1	-2/2
Lag. DV	1.005***	.998***	.995***	.999***
	(.049)	(.050)	(.048)	(.048)
Regs (T-2) X HMO (1995)				.067
				(.069)
Regs (T-1) X HMO (1995)	.119***		.185**	.078
	(.033)		(.076)	(.103)
Regs X HMO (1995)		.119***	-.097	-.055
		(.034)	(.110)	(.106)
Regs(T+1) X HMO (1995)			.027	.032
			(.092)	(.172)
Regs(T+2) X HMO (1995)				-.001
				(.120)
P-value Leads are Zero			.88	.98
P-value Lags are Zero			.02	.05
Observed level in U.S. (2005)	13.48	13.48	13.48	13.48
Forecast w/o Regulations	11.52*	11.52*	11.77	11.63
90% CI Upper Bound	13.25	13.46	13.52	13.75
90% CI Lower Bound	9.83	9.69	9.71	9.26
Number of Obs.	550	550	550	550
Number of Clusters	50	50	50	50
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes

(VI)

Each column presents results from estimating equation (1) with suitable covariates and state and year fixed effects. Bootstrapped standard errors clustered by state are in parentheses. Data on state regulation of managed care obtained from the NCSL. The regulations variable is the sum of all regulations in force in the given state and year. Data on the percentage of state population enrolled in HMOs in 1995 is obtained from the Statistical Abstract. Data on health expenditures and GSP are from CMS. All regressions contain main effects that are suppressed. To compute the 2005 forecast, I draw 500 independent observations from the distribution of the coefficient vector and dynamically simulate the counterfactuals of all regulation variables being set to zero.

Table VII

Robustness to Other Health Insurance Regulations *Dynamic Panel Specification* *Dep. Var. is Total Health Share*							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Other Reg:		Other Mandated Benefits	Small Group Reform	Indiv. Mrkt. Reform	State Tort Reform	Mcd Simltd Elig.	All Other Regs.
Lag. DV	1.005***	1.040***	.995***	1.041***	1.035***	1.038***	1.034***
	(.049)	(.044)	(.050)	(.046)	(.045)	(.046)	(.034)
Regs (T-1)	-.007	-.022	-.008	-.005	-.007	-.008	-.017
	(.007)	(.014)	(.007)	(.007)	(.007)	(.007)	(.015)
Regs (T-1) X HMO (1995)	.119***	.201***	.121***	.125***	.123***	.126***	.181***
	(.033)	(.050)	(.033)	(.033)	(.032)	(.033)	(.053)
Oth. Reg. (T-1)		.043	.105	-.503*	-.074	.550	
		(.028)	(.128)	(.271)	(.047)	(1.157)	
Oth. Reg. (T-1) X HMO (1995)		-.177	-.270	1.360	.465*	1.105	
		(.123)	(.926)	(3.022)	(.249)	(6.966)	
Observed level in U.S. (2005)	13.48	13.48	13.48	13.48	13.48	13.48	13.48
Forecast w/o Regs	11.52*	10.99*	11.65*	10.73*	11.12*	11.22*	10.94*
90% CI Upper Bound	13.25	12.93	13.41	12.87	13.22	13.19	12.88
90% CI Lower Bound	9.83	8.85	9.92	8.85	9.03	9.37	8.57
Number of Obs.	550	550	550	550	550	550	550
Number of Clusters	50	50	50	50	50	50	50
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

(VII)

Each column presents results from estimating equation (1) with suitable covariates and state and year fixed effects. Bootstrapped standard errors clustered by state in parentheses. Data on state regulation of managed care obtained from the NCSL. The regulations variable is the sum of all regulations in force in the given state and year. Data on the percentage of state population enrolled in HMOs in 1995 is obtained from the Statistical Abstract. Data on health expenditures and GSP are from CMS. Data on mandated benefits, small group reforms and individual market reforms is obtained from Blue Cross Blue Shield's "State Legislative Health Care and Insurance Issues." The mandated benefits variable is the sum of mandated benefits. Following Simon (2000) I consider a state to have passed a small group reform if it has guaranteed issue, guaranteed renewal and rating reform, and the individual market reform is coded similarly. Data on state tort reforms from Avraham (2010). Data on simulated Medicaid eligibility from Kosali Simon. To compute the 2005 forecast, I draw 500 independent observations from the distribution of the coefficient vector and dynamically simulate the counterfactuals of all regulation variables being set to zero.

Table VIII

Robustness to Reparametrizations						
	(1)	(2)	(3)	(4)	(5)	(6)
			Dynamic Panel Specification *Dep. Var. is Total Health Share*			
Lag. DV	1.005*** (.049)	1.036*** (.044)	1.003*** (.051)	1.041*** (.044)	.927*** (.064)	1.026*** (.042)
Regs (T-1)	-.007 (.007)	.008 (.032)	-.148 (.108)	-.005 (.009)		
Regs (T-1) X HMO (1995)	.119*** (.033)	.414*** (.107)	1.739*** (.509)	.155*** (.036)		
Access Regs (T-1) X HMO (1995)						-.118 (.092)
Appeals Regs (T-1) X HMO (1995)						.417 (.463)
Provider Regs (T-1) X HMO (1995)						.504*** (.181)
Mandates Regs (T-1) X HMO (1995)						.456 (.277)
Observed level in U.S. (2005)	13.48	13.48	13.48	13.48	13.48	13.48
Forecast w/o Regulations	11.52*	8.05**	12.21*	10.35**	11.86	11.50*
90% CI Upper Bound	13.25	11.53	13.34	12.55	23.99	13.18
90% CI Lower Bound	9.83	4.78	11.14	8.29	.26	9.77
Indep. Var.		Bills	Indicator Major Passage	Count of Types	Indicators of Types	4 Types Counts
Number of Obs.	550	550	550	550	550	550
Number of Clusters	50	50	50	50	50	50
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes

(VIII)

Bootstrapped standard errors clustered by state in parentheses. Data definitions as in Table III. Column 2 replaces regulations with the cumulative count of years since 1994 in which any regulations were passed in the given state. Column 3 replaces regulations with an indicator that the year in question is after the largest single-year passage of regulations. Column 4 replaces the regulations variable with a count of the number of the 27 regulation categories in which some regulations have been passed in the given state by the given year. Column 5 breaks down the regulation variable into four variables, one for each major regulation category in Table I (main effects are not reported).

Table IX

Other Potential Confounders *Dynamic Panel Specification* *Dep. Var. is Total Health Share*				
	(1)	(2)	(3)	(4)
Other Reg:	Baseline	Add Hospital HHI Expend.	Add Hospital Capacity Log Beds / Capita	Add Fraction Med. Eq. Patents
Lag. DV	1.005***	1.050***	1.038***	1.036***
	(.049)	(.042)	(.041)	(.042)
Regs (T-1)	-.007	-.006	-.004	-.008
	(.007)	(.007)	(.010)	(.006)
Regs (T-1) X HMO (1995)	.119***	.094***	.087*	.126***
	(.033)	(.032)	(.052)	(.032)
Control (T-1)		-.165***	-.752	2.899**
		(.063)	(.754)	(1.424)
Control (T-1) X HMO (1995)		1.150***	-2.227	-16.560*
		(.418)	(3.748)	(8.478)
Observed level in U.S. (2005)	13.48	13.48	13.48	13.48
Forecast w/o Regs	11.52*	11.70*	11.65*	11.15*
90% CI Upper Bound	13.25	13.46	13.19	13.22
90% CI Lower Bound	9.83	9.88	10.09	9.07
Number of Obs.	550	550	550	550
Number of Clusters	50	50	50	50
State FE				
Year FE	Yes	Yes	Yes	Yes

(IX)

Bootstrapped standard errors clustered by state in parentheses. Data definitions as in Table III. Column 2 adds the lagged state HHI (sum of squared expenditure shares) for hospitals and its interaction with HMO penetration in 1995. Column 3 replace the state HHI with log beds per capita. Column 4 replaces the state HHI with the fraction of patents from a given state that relate to medical devices, constructed from the NBER Patent Database on the basis of Clemens (2013).

Table X

Dynamic Panel Specification: Robustness to Regional Disaggregation

Dep. Var. is Hospital Expenditure Share of Unit Personal Income and Total Health Share in Column 1

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Lag. DV	1.057***	1.002***	1.234***	1.021***	.998***	1.078***	1.069***	1.021***	1.021***
	(.090)	(.164)	(.003)	(.030)	(.017)	(.036)	(.038)	(.006)	(.006)
Regs (T-1)	-.007	.000	.006	-.002	-.001	-.001	.001	-.003	.002
	(.006)	(.003)	(.009)	(.002)	(.004)	(.004)	(.006)	(.003)	(.004)
Regs (T-1) X HMO (LB) (1995)	.116***	.041**	.009	.057***	.043***	.033***	.031***	.042***	.018
	(.032)	(.019)	(.041)	(.013)	(.017)	(.009)	(.010)	(.012)	(.017)
No. Observations	550	550	550	1067	1067	4971	4971	24575	24575
No. Units	50	50	50	97	97	455	455	2452	2452
No. Clusters	50	50	50	50	50	50	50	50	50
Observed U.S. level (2005)	16.2	5.44	5.44	5.44	5.44	5.44	5.44	5.44	5.44
Forecast w/o Regulations	13.78*	4.31	3.09*	4.20**	4.51*	4.66	4.30	4.98	4.85
90% CI Upper Bound	15.92	5.51	5.41	5.03	5.42	5.60	5.97	5.73	5.86
90% CI Lower Bound	11.69	3.23	.70	3.42	3.65	3.62	2.65	4.27	3.90
Unit of Analysis	State	State	State	MSU	MSU	MSA	MSA	County	County
State Trends	No	No	Yes	No	Yes	No	Yes	No	Yes

(X)

Each column presents results from estimating equation (1) with suitable covariates and unit and year fixed effects. Bootstrapped standard errors in parentheses, clustered at the state level. Data on regulations as in Table III . Data on the percentage of county population enrolled in HMOs in 1995 (aggregated up when necessary) is obtained from an original dataset compiled by Laurence Baker, originally from Interstudy. Data on hospital total expenditures is obtained from the AHA Annual Survey, and data on county personal income (aggregated up when necessary) is obtained from the BEA. In column (1), the dependent variable is total health spending as a share of state personal income.

Table XI

Hospital Utlization

Dynamic Panel Specification

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Dep. Var.	Total Health Share of GSP	Hospital Expend. Share of GSP	Total Hlth Emplmt. Share of Pop.	Total Hlth Avg. Sal. Share of GSP p/c	Hospital Emplmt. Share of Pop.	Hospital Avg. Sal. Share of GSP p/c	Ambulatory Emplmt. Share of Pop.	Ambulatory Avg. Sal. Share of GSP p/c	Hospital Inp. Days. per Adm.	Hospital Inp. Days. per Capita	HMO Penetration
Lag. DV	1.005***	.969***	1.094***	.889***	.912***	.852***	1.058***	.915***	.569***	.808***	.870***
	(.049)	(.055)	(.061)	(.053)	(.053)	(.073)	(.067)	(.060)	(.155)	(.064)	(.079)
Regs (T-1)	-.007	-.000	-.001	-.008	.000	-.009	-.001	.011	.006	-.002	.079
	(.007)	(.003)	(.001)	(.057)	(.000)	(.069)	(.001)	(.069)	(.004)	(.053)	(.063)
Regs (T-1) X HMO (1995)	.119***	.044***	.012**	.911***	.005	.905***	.000	.931***	.015	.482*	-1.141***
	(.033)	(.016)	(.004)	(.178)	(.004)	(.219)	(.003)	(.225)	(.026)	(.269)	(.297)
Observed level in U.S. (2005)	13.48	4.53	3.61	119.43	1.47	127.02	2.13	114.19	6.48	81.20	22.79
Forecast w/o Regulations	11.52*	3.61**	3.46	104.68***	1.34*	114.15***	2.35	95.73***	6.10**	75.08	34.30**
90% CI Upper Bound	13.25	4.34	3.73	111.64	1.46	122.45	2.55	106.35	6.38	81.56	43.12
90% CI Lower Bound	9.83	2.91	3.22	97.43	1.22	106.25	2.12	85.87	5.84	68.97	26.02
Forecast Growth,	17.01	25.19	4.34	14.09	9.76	11.27	-9.19	19.28	6.17	8.15	-33.52
Number of Obs.	550	550	550	550	550	550	550	550	550	550	550
Number of Clusters	50	50	50	50	50	50	50	50	50	50	50
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

(XI)

See Table III. Data on dependent variables obtained from the AHA Annual Survey, the BEA and the CDC. Data on health expenditures and GSP obtained from CMS.

Table XII

Health Outcomes *Dynamic Panel Specification*				
	(1)	(2)	(3)	(4)
Dep. Var.	Mortality Under 65	Fraction Excellent Health Under 65	Fraction Poor Health Under 65	Fraction Quit Job Because of Health
Lag. DV	.967***	.999***	.359**	.022
	(.044)	(.197)	(.178)	(.295)
Regs (T-1)	-.005	-.035	.005	-.006
	(.079)	(.048)	(.010)	(.011)
Regs (T-1) X HMO (1995)	.590*	.146	-.046	-.036
	(.313)	(.109)	(.042)	(.040)
Observed level in U.S. (2005)	238.37	36.40	2.34	2.58
Forecast w/o Regulations	226.55	37.31	2.43	2.81
90% CI Upper Bound	238.76	45.24	2.74	3.04
90% CI Lower Bound	214.04	29.06	2.11	2.57
Forecast Growth	5.21	-2.44	-3.73	-8.20
Sample Period	1995-2005	1997-2005	1997-2005	1995-2005
Number of Obs.	550	450	450	550
Number of Clusters	50	50	50	50
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes

(XII)

See Table III. Mortality data obtained from the CDC. Self-reported health and job quits because of health obtained from CPS. Note that self-reported heatlh available only for the period 1996-2005, requiring estimation to take place on the sample starting in 1997.

Table XIII

Instrumental Variable Estimates			
	(1)	(2)	(3)
	DPNLS	DPNLS Dem. Insts.	GMM Phys. X Dem. Insts.
Lag. DV	1.005***	.919***	1.023***
	(.049)	(.073)	(.063)
Regs (T-1)	-.007	-.005	-.004
	(.007)	(.010)	(.012)
Regs (T-1) X HMO (1995)	.119***	.065	.129**
	(.033)	(.055)	(.058)
Angrist-Pischke P-val*		.00	.00
Hansen P-val.*		.55	.55
Hausman P-val. vs. Baseline*		.58	.58
P-val. Dem. Exog. Vars.,			.55
Observed level in U.S. (2005)	13.48	13.48	13.48
Forecast w/o Regulations	11.52*	12.83	10.68
90% CI Upper Bound	13.25	14.96	13.78
90% CI Lower Bound	9.83	10.78	7.74
Dem. Insts.	No	Yes	No
Phys. Dom X Dem Inst.	No	No	Yes
Dem. Cntrls. in Stage 2	No	No	Yes
No. Observations	550	550	550
No. Clusters	50	50	50
State FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes

(XIII)

Each column presents results from estimating equation (1) via instrumented DPNLS. Standard errors clustered by state. Statistics marked with a star (*) are computed for the specification in which the coefficient on the lagged dependent variable is imposed to be unity. Data on baseline variables as in Table III. Data on Democratic control from the Statistical Abstract of the US. Data on physician dominance of health interest groups (fraction of health lobby organizations by primary care clinic organizations) from Virginia Gray. Column 3 contains the Democratic controls (with and without interaction with South dummy) included as exogenous controls.

10 Figures

Figure 1 (1)