The Heterogenous Impact of Privatizing Public Health Insurance: Evidence from California's Medicaid Program

Sarah Bögl
Stanford University
sbogl@stanford.edu

Mark Duggan*
Stanford University and NBER
mgduggan@stanford.edu

Craig Garthwaite
Northwestern University and NBER
c-garthwaite@kellogg.northwestern.edu

Clare Stevens**
Stanford University
cbs12@stanford.edu

Adelina Yanyue Wang***
Stanford University
adywang@alumni.stanford.edu

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Abstract

State governments face the classic "make or buy" decision for the provision of Medicaid services. Over the past few decades, virtually all state governments have outsourced the provision of this public health insurance program for most of their Medicaid recipients through Medicaid managed care (MMC). The consequences for children and expectant mothers, who were the first ones shifted to MMC in most states due to their low expected costs and relatively simple health care needs, have been extensively studied. But in more recent years, many states have been shifting their disabled and elderly Medicaid recipients into MMC plans through state and local-level mandates. These beneficiaries account for a disproportionate share of Medicaid spending, and it is plausible that the impact of MMC for these relatively fragile beneficiaries with more complicated health care needs is very different. In this paper, we study California's mandatory enrollment of the senior and persons with disabilities (SPD) population into MMC. We estimate that the mandate-induced shift from Medicaid fee-for-service to MMC increased visits to emergency departments by 3.7 percent and mortality by 7.3 percent, with larger increases for recipients in worse health.

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Since its creation in the 1960s, Medicaid has provided health insurance to an increasing share of Americans, with 79 million (23 percent of) U.S. residents enrolled in the program in January 2025 (CMS, 2025a).[1] This makes Medicaid the largest health insurer in the United States and one of the largest in the world. Unlike other large social insurance programs in the U.S. such as Social Security and Medicare, Medicaid is administered and partially funded by state governments. As a result, the program varies significantly across states in ways that could result in economically meaningful differences in its impact.

One increasingly important dimension of variation across states and within states over time has been the degree of privatization. As Medicaid expanded over the years, many states debated the proverbial "make or buy" decision, i.e. is it optimal for the government to directly provide health insurance or should the state instead outsource these activities to private actors? States have increasingly chosen outsourcing through the creation and expansion of Medicaid managed care (MMC) programs. Under MMC, state governments contract with (often private for-profit) health insurers that offer managed care plans to Medicaid recipients. Payments are made on a "capitated" basis, with the firms receiving a lump sum, risk-adjusted payment. In return, these firms assume financial responsibility for beneficiary health care spending (with certain services sometimes "carved out" of the contract and remaining the responsibility of the government to finance and manage). Firms retain any difference between their capitation payments and their total costs as profits (or suffer losses if their costs exceed the sum of their capitated payments).

The share of Medicaid recipients enrolled in MMC increased rapidly in the 1990s, from 11 percent in 1991 to 55 percent by 1999 (Duggan and Hayford, 2013). This growth was driven by state and local-level mandates that required certain categories of Medicaid recipients to transition from traditional fee-for-service (FFS) Medicaid into MMC plans. These initial mandates generally focused on relatively healthy and low-cost Medicaid recipients including children and expectant mothers. Previous research has examined the effects of the policy-induced shift to MMC during the 1990s, with evidence that MMC reduced either the quality of or the access to health care (Currie and Fahr, 2005; Aizer, Currie, and Moretti, 2007; Kuziemko, Meckel and Rossin-Slater 2018) and increased Medicaid spending (Duggan, 2004).

Despite the substantial enrollment growth in MMC, the share of total Medicaid spending paid to MMC plans remained well below the surging enrollment shares. For example, in 1999, when 55 percent of Medicaid recipients were enrolled in MMC, just 12 percent of Medicaid spending was paid to these plans. This largely reflected states' reluctance to force more complicated Medicaid recipients such as elderly individuals or people with disabilities into these capitated payment arrangements. Because of this

[1] This includes 7 million children enrolled in CHIP (Children's Health Insurance Program). The expansion in Medicaid and CHIP enrollment occurred in several waves, starting with the inclusion of more children and expectant mothers in the late-80s and early 1990s and culminating with the passage of the Affordable Care Act in 2010. Medicaid enrollment as a share of the U.S. population increased from 8.3 percent in 1987 to 12.1 percent in 2000 to 17.5 percent in 2010 to 23.2 percent in 2025 (CMS, 2025b).

reluctance, these high-cost patients were typically exempt from MMC mandates and remained in traditional FFS Medicaid. But proponents of MMC argued that it was precisely these more complicated patients who could stand to benefit the most, and for whom the cost savings might be greatest, from the potentially improved coordination of care provided by MMC plans (Lewin Group, 2009). Consistent with this, Lee (2020) found evidence that MMC reduced costs with no deterioration in health outcomes for very low-birthweight infants in New York City.

With Medicaid placing growing pressure on state government budgets,[2] many states (perhaps motivated by the possibility that MMC could reduce program expenditures) expanded their privatization efforts beyond these initial programs. An important component of this was the expansion of MMC mandates to Medicaid recipients with increasingly complicated health care needs including elderly and disabled Medicaid recipients (Hinton and Raphael, 2025). As shown in Figure 1(a), the share of Medicaid recipients nationally enrolled in managed care plans increased from 56 percent in 1999 to 85 percent by 2022. But in contrast to the MMC enrollment growth during the 1990s, this increase coincided with a correspondingly larger increase in the share of Medicaid spending going to MMC plans, which rose from just 12 percent in 1999 to 60 percent by 2022 (Figure 1(b)).

The policy decision to further expand MMC was likely driven, at least in part, by the existing literature on the initial program. Most of the previous literature examining the effects of MMC on the quality of health care and on health outcomes has focused on children and expectant mothers.[3] This is perhaps unsurprising because these were the primary enrollees in these earliest programs. The focus on these populations has important implications for the external validity of these earlier studies to more recent policy changes. This is particularly true because the majority of Medicaid spending is accounted for by the relatively small number of adult beneficiaries who are either disabled or elderly and who are the focus of states' more recent privatization efforts. It seems plausible that the use of health care by these patients is very different from the enrollees who were affected by earlier privatization efforts. If the effect of these reforms systematically varies across patients based on their health status and their baseline utilization of health care, then the existing literature may not be an accurate guide to the likely impact of these greatly expanded privatization efforts for these much more complicated patients.

In this paper, we aim to help fill this gap in the existing literature by examining the effects of California's decision to mandate MMC enrollment for seniors and people with disabilities (SPD) enrolled in Medicaid that had not already voluntarily enrolled in such programs.[4] The mandates were introduced

[2] The share of state spending accounted for by Medicaid rose from 12.5 percent in 1990 to 19.1 percent in 2000 (MACPAC, 2020).
[3] One exception is the study of MMC in the Texas Medicaid program by Layton et al (2022), which we discuss further below.
[4] This was undertaken as part of the state's preparations for implementing the Affordable Care Act and implemented under the auspices of the "Bridge to Reform" section 1115 waiver. Part of this waiver mandates SPD Medicaid beneficiaries to enroll in MMC in a selection of counties where SPDs previously had the option to enroll in MMC but were not required to do so.

in 16 California counties in 2011 and 2012 and were phased in gradually as a function of Medicaid recipients' month of birth. We estimate the causal effect of mandatory MMC enrollment by exploiting both variation in when Medicaid beneficiaries were required to enroll in MMC and geographic variation in the exposure to the mandates. We also explore whether and to what extent any effects vary by patient health, which appears to have been a reasonable concern for policymakers making decisions about which groups of Medicaid recipients should enroll in MMC.

The optimality of privatizing government services is the subject of a rich theoretical economics literature that extends well beyond healthcare. The primary goal of privatization is often to leverage the fact that employees in private firms have a stronger financial incentive than their counterparts in government to control costs (Levin and Tadelis, 2010). Of course, such cost reduction efforts could have negative consequences, especially in the context of health care and in a world of incomplete contracts (Hart, Shleifer and Vishny, 1997). These authors find that while outsourcing is optimal in many settings, the government should be less likely to outsource where there is a greater concern about adverse consequences resulting from reductions in non-contractible and/or unobservable quality.

In our setting, any cost reductions created by private firms would ideally result from better preventive care, early diagnosis and treatment, and more efficient care management. There is, of course, also a fear that firms may lower costs by reducing quality in ways that cannot be easily governed by a contract with the government. This could occur for multiple reasons. Capitated payments may create inappropriately strong incentives to cut costs in a setting where firms may not be forced to internalize the full impact of lower quality – perhaps because such cost cutting is unobservable and affecting patients who may be less vocal about the adverse consequences (and thus less salient to policymakers). Compounding this concern, firms may find it profitable to provide low quality services to enrollees who are expected to generate higher costs in the future than their capitated payment.[5] This concern is even more salient if higher spending patients were more likely to move across plans in response to low quality care, perhaps because they place greater value on access to higher cost medical services (Shepard, 2022; Geruso, Layton, and Prinz, 2019).

Regardless of the mechanism, it is plausible that the impact of the lower quality caused by cost reduction efforts might be more consequential for patients in worse health who could suffer meaningful harms (up to and including death) from even minor disruptions in their access to care. The insights from Hart, Shleifer and Vishny (1997) suggest that it is precisely such concerns about adverse consequences that

[5] States attempt to counteract this gaming by risk adjusting payments. This was not a feature of many initial MMC programs, and could have led to adverse consequences (Kuziemko, Meckel and Rossin-Slater 2018). That said, even in a system where capitated payments are risk-adjusted, within each risk score there are enrollees who have above average spending that firms may attempt to persuade to switch to another plan (Geruso, Layton, and Prinz, 2019).

may have caused states to continue to directly provide public health insurance to their older, sicker, and most vulnerable Medicaid beneficiaries rather than contracting out these services.

If such concerns about variation in the adverse consequences of cost reductions based on the health of patients were valid, we would predict two patterns in our setting that focuses on the most complicated Medicaid patients. First, the results from privatization in our setting should be no better (and possibly worse) than in the existing literature, which primarily focuses on healthier Medicaid recipients. Second, privatization should differentially impact enrollees *within* our sample based on underlying patient health — with sicker enrollees suffering greater harms. Our empirical results are broadly consistent with both predictions.

We focus on Medicaid recipients in their forties, fifties, and early sixties given that most Medicaid recipients transition to Medicare upon reaching the age of 65. We estimate that requiring California's SPD Medicaid beneficiaries to enroll in MMC caused an immediate and persistent increase in visits to the hospital emergency department (ED), which would be consistent with a change in access to other medical services and/or a decline in the underlying health of patients. This suggests that, on average, the shift to MMC reduced access to and lowered the quality of health care along dimensions that would affect the use of the emergency department. While we do not find an overall increase in hospital inpatient visits, we estimate that the MMC mandates increased mortality by 7.3 percent among the adult Medicaid recipients in our sample.

We then demonstrate the importance of heterogeneity in the impact of privatization by examining systematic differences within our sample based on patient health. We measure patient health based on both the Charlson and Elixhauser comorbidity indices of patient severity.[6] Across both measures, we find the estimated increases in ED visits and mortality were concentrated among sicker beneficiaries, precisely the beneficiaries about whom policymakers were likely most concerned would suffer greater adverse consequences from MMC.

Our results contribute to a better understanding of the optimal system for providing public health insurance to many of America's most vulnerable individuals: low-income adults with disabilities. It joins recent work by Layton et al. (2022) which examines similar MMC expansions to older and disabled populations in the state of Texas. While the details of the Texas expansion are very different from those in California (e.g. inpatient care was carved out of the Texas MMC contracts), the authors find that the expansion there actually increased access to some services and find no evidence of negative health effects.

[6] Both measures are calculated using data from prior to the reform and therefore are not influenced by features of MMC enrollment such as limited access to care or more intensive coding of medical conditions. Previous research has used one or both measures to estimate patient health status (Doyle, 2011; Finkelstein et al, 2016; Gupta et al, 2024).

The divergence in results across these studies of MMC in America's two most populous states is likely at least partially driven by meaningful differences in the specific structure of the Medicaid program across states. Such variation makes it difficult to generalize from the estimated effects of a particular policy choice (Garthwaite et al., 2019). Instead, policymakers, economists, and other stakeholders interested in making such comparisons must consider the specific economic factors that may be similar across contexts.

That said, California is by far the nation's largest Medicaid program with 14.5 million recipients as of June 2024, representing 17 percent of the national total at that time (CMS, 2025c). This is more than twice as high as Medicaid enrollment in New York (7.0 million) and more than three times as high as either Texas (4.4 million) or Florida (4.3 million). Indeed, California's Medicaid enrollment as of June 2024 was approximately equal to the combined total for the 25 least populous states, the District of Columbia, and Puerto Rico of 14.5 million Medicaid recipients in that same month (CMS, 2025).[7]

Our results also contribute to the growing literature about the health and mortality effects of health insurance. Numerous studies found few health effects of increased insurance coverage (Levy and Meltzer, 2008; Finkelstein and McKnight, 2008; Finkelstein et al., 2012). However, more recent work has demonstrated decreased mortality among individuals gaining access to insurance (Sommers, Baicker and Epstein, 2012; Sommers, Long, and Baicker, 2014; Miller et al., 2021; Goldin, Lurie and McCubbin, 2021; Meyer and Wyse, 2025). Our work expands this literature by demonstrating the importance of the form of the insurance contract and not simply the extensive margin of insurance. In this way, it joins the work of Abaluck, Bravo, Hull, and Starc (2021) which finds differential mortality for enrollees based on which firm manages their Medicare Advantage (i.e. voluntary managed Medicare) plans.

I. The Medicaid Program

The Medicaid program was created in 1965 and has provided health insurance for an increasing number of low-income Americans in the years since. By January 2025, enrollment in Medicaid was 79 million (CMS, 2025a) and total Medicaid spending by the federal government and by state governments was $860 billion (CMS, 2025d).[8] Even prior to the passage of the Affordable Care Act in 2010, Medicaid was accounting for an increasing share of expenditures by state governments, from 12.5 percent in 1990 to 20.5 percent by 2008 (MACPAC, 2020).[9] Partly because of the Affordable Care Act's Medicaid expansion, this share increased further to 29.8 percent in 2024 (Peter G. Peterson Foundation, 2025).

[7] The 25 states are (in descending order of 2024 resident population) Kentucky, Oregon, Oklahoma, Connecticut, Utah, Nevada, Iowa, Arkansas, Kansas, Mississippi, New Mexico, Nebraska, Idaho, West Virginia, Hawaii, New Hampshire, Maine, Montana, Rhode Island, Delaware, South Dakota, North Dakota, Alaska, Vermont, and Wyoming (U.S. Census Bureau, 2025).
[8] Medicaid spending data are for the 2023 calendar year.
[9] Considering only state general funds (thus excluding federal funds), the increase from 1990 to 2008 was 9.5 percent to 16.0 percent.

While required to comply with federal guidelines and jointly funded by the federal government, each state administers Medicaid independently and has a considerable degree of autonomy in terms of eligibility requirements, service provision, delivery networks, and reimbursement generosity. Meanwhile, even within each state, Medicaid covers a population with a wide array of health conditions and care intensity, resulting in a highly skewed distribution of costs and utilization of health care services.[10] This results in marked heterogeneity in Medicaid's operations both across and within states.

I.A. Background on Medicaid Managed Care in the U.S.

Increasingly confronted with the challenge of rising and, perhaps more importantly, unpredictable Medicaid expenditures as well as fragmented care delivery systems, states have been shifting enrollees from the traditional fee-for-service (FFS) system to Medicaid managed care (MMC) plans since the early 1990s. As discussed above, the details of plan payment and ownership have varied substantially both across and within states over time. Typically, these arrangements involve a state paying an often risk-adjusted payment per-member per-month to a managed care plan, which then coordinates and finances care for each enrollee. In some cases, the managed care plan covers all health care services, while in others, one or more services are "carved out" of the contract with the managed care plan.

Prior to the implementation of the key provisions of the Affordable Care Act (ACA) in 2014, the share of Medicaid recipients nationally enrolled in MMCs rose from 56 percent in 1999 to 73 percent in 2013. Much of this enrollment growth was driven by state and local-level mandates that required certain categories of Medicaid recipients to enroll in MMC plans. Payments to MMC plans as a share of total Medicaid spending rose steadily from just 12 percent in 1999 to 33 percent in 2013 (CMS, various years). The MMC expenditure share was consistently much lower than the MMC enrollment share both because managed care plans tended to cover healthier Medicaid recipients such as children and expectant mothers (who had relatively low utilization of medical care and health care costs) and because many states carved out certain health care services from MMC contracts.

In the years leading up to and immediately following the significant expansion of Medicaid enrollment in early 2014, the scope of MMC enrollment also grew as these programs covered many newly eligible Medicaid recipients as well as a much wider group of existing Medicaid beneficiaries with more complicated needs and health conditions. As can be seen in Figures 1(a) and 1(b), 85 percent of Medicaid beneficiaries were enrolled in managed care plans by 2022, and payments to these plans had risen to 60

[10] For example, in 2021 (the most recent year with spending data by enrollment group), average Medicaid spending per recipient was $14,434 for seniors and $16,622 for people with disabilities, but only $2,951 for children and $4,471 for other adults. As a result, seniors and people with disabilities account for more than half of Medicaid spending (KFF 2021a; KFF 2021b).

percent of total Medicaid expenditures in that year. It is noteworthy that the 2022 expenditure share was twice as high as the corresponding share in 2013 despite a relatively modest increase in MMC enrollment during the 2013 to 2022 period. This suggests that MMC was differentially enrolling more complicated and expensive Medicaid recipients (for whom managed care plans would tend to receive much higher capitation payments) in recent years than previously.

MMC significantly changed the financial incentives within the Medicaid program. Instead of directly reimbursing health care providers such as hospitals and physicians for all services provided to Medicaid recipients, state governments contract with health insurers offering MMC plans and typically pay a risk-adjusted capitation amount for each covered Medicaid enrollee. The health insurers then manage the purchase of healthcare goods and services for their MMC recipients and bear the marginal cost of each service. One goal of this arrangement is to give health insurers strong financial incentives to eliminate unnecessary health care services, increase efficiency, and improve the coordination of care among different health care providers within their network.

From the perspective of health care quality and coordination, MMC is inevitably subject to the problem of incomplete contracting that can affect many other government services. The more complicated the health condition of enrollees, the more challenging it is for the government to write a complete contract with a health insurer that eliminates its incentives to shirk on difficult-to-verify dimensions of quality. For example, the health insurer may find it profitable to contract with inexpensive, low-quality providers or to limit its network to relatively few providers who are able to treat unprofitable conditions (thereby reducing patients' access). Evidence of such behavior has been seen in insurers operating on the ACA exchanges (Geruso and Layton, 2020). As the residual claimant, holding all else fixed, a health insurer's profits strictly decrease as its costs increase. While certain factors surely discipline health insurers, such as the risk of litigation due to substandard medical care or the concern that a state will not renew the contract if many beneficiaries complain about quality, these factors are almost surely imperfect solutions to the incentive misalignment.

Having the state government directly administer this health insurance through forming its own provider networks, negotiating provider reimbursement rates, and so forth does not necessarily eliminate all problems for complex patients. It seems plausible that the state government does, however, have a lower financial incentive than a health insurer to cut health care costs in ways that could cause adverse consequences for the sickest patients. This could tilt the optimal decision in favor of the government-run fee-for-service Medicaid program for especially vulnerable populations with more complicated health care needs, for whom there may be much greater concerns about adverse health consequences. On the other hand, the potential benefits from one organization coordinating care by many providers (rather than simply

organizing reimbursements) may more than offset this issue. It is ultimately therefore an empirical question whether and to what extent the shift to MMC affected the utilization of care and health outcomes for the most vulnerable Medicaid recipients.

I.B. Existing Evidence on Medicaid Managed Care

A substantial body of previous research has investigated the effect of the relatively early shifts to MMC that typically required enrollment only for healthier populations such as low-income children and expectant mothers. Some studies found no significant impacts of MMC on the health outcomes of newborns (Duggan, 2004; Lee, 2020) and that MMC led to a reduction in the utilization of care and an increase in preventive care among children (Marton, Yelowitz, and Talbert, 2014).[11] Others found evidence for both worse health outcomes due to reduced quality of prenatal care (Aizer, Currie, and Moretti, 2007) and risk selection arguably through differential quality of services for specific populations expected to exhibit higher costs (Currie and Fahr, 2005; Kuziemko, Meckel and Rossin-Slater, 2018).

In more recent research, Macambira et al. (2025) evaluated the effects of MMC by leveraging the randomization of nearly one hundred thousand Medicaid recipients in Louisiana to either MMC or Medicaid fee-for-service. The authors found that enrollment in MMC led to a reduction in Medicaid spending and that this was driven primarily by the MMC plan's management of pharmaceutical treatments and spending. They also found evidence of reductions in the quality of care, with an increase in emergency department visits and reduced access to primary care among those enrolling in MMC. In contrast to those affected by the reform that we study, the sample in Macambira et al. (2025) consisted primarily of children, with an average age in their sample of 9 years, and thus relatively few seniors and people with disabilities.

For many years after their initial shifts to MMC, most states did not require disabled or elderly (aged 65+) individuals, with their relatively more complicated health care needs, to enroll in MMC. The results from most of these earlier studies may therefore not apply for this group. One exception to the prior MMC literature's focus on Medicaid-insured children and relatively healthy adults is a recent study by Layton et al. (2022), who study the effects of Texas requiring adult Medicaid recipients with disabilities to move into MMC in 2007. In contrast to the recent Macambira et al. (2025) study cited above, Layton and co-authors find that the shift to MMC increased Medicaid spending and the utilization of non-inpatient medical care. It is worth noting that this setting differed from ours in two important respects. First, the Texas shift to MMC coincided with another Medicaid policy change in Texas – the relaxation of a

[11] Lee and Vabson (2024) leverage a policy in New York state that prevented newborn infants weighing less than 1200 grams from enrolling in MMC while their counterparts above 1200 grams typically did. Their findings using a regression discontinuity design suggest those infants assigned to MMC are more likely to remain on Medicaid and suggestive evidence this is attributable to higher plan quality.

restrictive cap on the utilization of prescription drugs that only applied to those transitioning to MMC. This also affected Medicaid recipients' utilization of health care services, making it more difficult to disentangle the effect of MMC from the combined effect of MMC and a relaxed drug cap.[12] Second, inpatient hospital utilization was carved out of the managed care contract in the Texas Medicaid program. Given that inpatient care accounts for more than half of hospital spending, it seems plausible that MMC could have a very different effect when the insurer is not at financial risk for this category of health care.

Also relevant for our investigation of the effects of managed care on seniors and persons with disabilities is the prior literature examining the impact of contracting with Medicare Advantage (MA) plans on health care utilization and health outcomes.[13] MA is the voluntary managed care program for Medicare, which provides health insurance to virtually all seniors in the U.S. and adults receiving Social Security Disability Insurance benefits. The MA literature may therefore provide as much insight into the likely effect of requiring MMC enrollment for older and sicker Medicaid beneficiaries as does the prior MMC literature. But before extrapolating from the MA literature, it is important to recognize the substantial differences between the Medicare and Medicaid managed care markets. Perhaps most importantly, MA is a *voluntary* program, where the decision to take part in managed care is made by the individual. There is existing evidence that those signing up for MA coverage are much healthier on average than those remaining in the traditional FFS Medicare program (Brown et al., 2014; Geruso and Layton, 2020).

In certain respects, this makes MA somewhat similar to the initial MMC programs that required younger and healthier enrollees to enroll but that gave disabled and elderly Medicaid beneficiaries the option to do so. Therefore, our paper also complements existing studies of MA by providing insights into the impacts of managed care for the less healthy seniors and persons with disabilities who opted to remain in Medicaid FFS. This may be particularly important as there are policy discussions about a wider use of MA style plans through premium support or other programs (Chandra and Garthwaite, 2019). Such policies would force all Medicare enrollees into managed care without clear evidence of the likely impact on the sickest beneficiaries. Even without such a mandate, the share of Medicare recipients enrolled in MA has grown substantially in recent years, from 13 percent in 2005 to 54 percent in 2024 (KFF, 2011; KFF, 2024). This change likely coincided with a substantial change in the health care needs of MA recipients.

[12] It seems plausible that the political economy of some states, such as Texas, may mean that the only practical way to expand the generosity of Medicaid benefit generosity is in combination with the use of privately managed MMC plans.

[13] For example, exploiting MA plan exits in New York counties, Duggan, Gruber, and Vabson (2018) show that MA plans are associated with a significant reduction in utilization by limiting access to nearby hospitals and reducing elective admissions. However, these authors find no evidence of increased quality or improved health outcomes from this change in utilization. Analyzing Medicare claims data, Curto, et al. (2019) find evidence consistent with MA plans encouraging substitution into less expensive care, though traditional Medicare and MA plans have similar level of spending per encounter/hospital admission.

Taken together, the previous literature provides mixed evidence regarding the effects of managed care in the Medicaid program. Moreover, virtually no research other than Layton et al. (2022) has investigated the effects of MMC on seniors and people with disabilities, who account for the majority of Medicaid spending (KFF, 2025a). We aim to begin to fill this gap with our study of a reform that required most elderly and disabled Medicaid recipients in more than a dozen of California's most populous counties to enroll in MMC plans.

II. California's Medicaid Managed Care Reform for Seniors and Persons with Disabilities

Created in 1966, the Medicaid program in California (known as Medi-Cal) provides health insurance coverage to low-income families, children, pregnant women, seniors, and persons with disabilities. By June 2024, Medicaid was by far the state's largest health insurer and covered 14.5 million individuals (37 percent of California residents) (CMS, 2025c). California's 2024-2025 budget allocated $161 billion in total expenditures to this program (California Legislative Analyst's Office, 2024).

Currently, all 58 counties in California offer MMC plans and have mandated that either certain eligibility groups or all Medicaid beneficiaries enroll in these plans. The types of MMC plans offered and the characteristics of the MMC mandates differ substantially both across counties in California and within counties over time. As of May 2025, there were five different models of MMC in the state covering 91 percent of all Medicaid beneficiaries (California DHCS, 2025).

II.A. Models of Medicaid Managed Care in California

Given the size and scope of California's Medicaid program, it should not be surprising that MMC operates in many forms across the state and over time. MMC was first mandated in California in 1983 with the introduction of a County Organized Health System (COHS) in Santa Barbara County. In the subsequent few decades, ten more counties including Orange, Monterey, and San Mateo, adopted the COHS model, in which all Medicaid recipients (including seniors and persons with disabilities) were mandated to enroll in the single MMC plan administered by the county (with few exceptions such as undocumented immigrants). In these eleven COHS counties, there was no competition since Medicaid recipients had only one MMC option, a health care plan administered by the county. In many cases, these COHS plans sub-contract with a commercial insurer such as Kaiser for certain health care services (Kaiser, 2020). These local, non-commercial COHS plans were similar in structure and ownership type to the MMC plans that were created in many other states, with 38 percent of MMC recipients nationally enrolled in

these local/regional plans in 2021 (KFF, 2025b).[14] As of January 2010, 82 percent of the 0.98 million Medicaid recipients in these 11 California counties were enrolled in MMC.

Another model of MMC used in 12 of California's counties since the early 1990s is the Two-Plan model, in which Medicaid recipients can choose between MMC plans administered by a commercial insurer or by a "local initiative" plan. While the local initiative plan was organized by the county and designed to maintain access to safety net providers, the administration of health care services for their enrollees was often subcontracted to a commercial entity. County-level mandates in the mid-1990s only required that most children and low-income adults in Two-Plan counties enroll in MMC plans. In contrast to the COHS counties described above, seniors and people with disabilities were not required to enroll in MMC in these Two-Plan counties, which included Los Angeles, San Bernardino, and ten other counties. As of January 2010, 54 percent of the 4.9 million Medicaid recipients in these twelve California counties were enrolled in MMC. At that time, 39 percent of MMC recipients in Two-Plan counties obtained their Medicaid coverage through a commercial health insurer with the rest covered by the local initiative plan.

A third model of MMC called Geographic Managed Care (GMC) was adopted in the mid-1990s by just 2 counties: Sacramento and San Diego. In these counties, Medicaid recipients had the choice between four or more plans administered by commercial health insurers that compete with each other for enrollees. The county-level mandates introduced in the mid-1990s were like those in Two-Plan counties, such that children and low-income adults were required to enroll in MMC but seniors and people with disabilities were not. As of January 2010, 54 percent of the 0.72 million Medicaid recipients in Sacramento and San Diego were enrolled in MMC, the same share as in the twelve Two-Plan counties.

MMC was virtually non-existent in California's other 33 (primarily rural) counties as of January 2010. More specifically, of the 0.55 million Medicaid recipients residing in these counties, just 0.4 percent were in an MMC plan then. Each of these counties eventually introduced an MMC mandate. Figure 2 provides a map of California's 58 counties that displays the 11 counties using a COHS model as of January 2010 along with the 12 and 2 counties with Two-Plan and GMC models at that same time.

II.B. California's MMC Mandate for Seniors and People with Disabilities

The policy change that we study in this paper is the 2010 SPD Medicaid managed care mandate for Two-Plan and GMC counties.[15] This policy was intended to reduce costs and improve the coordination

[14] At this same time in 2021, more than half of MMC enrollees nationally were enrolled in plans administered by one of five commercial, multi-state insurers: Centene, United Health, Elevance (formerly Anthem), Molina, and Aetna / CVS.

[15] This policy was part of the Bridge to Reform (BTR) Section 1115 Waiver that started California's transition into the full implementation of the Affordable Care Act. The policy mandates all SPD Medicaid recipients (except for dual eligibles, children in foster care, and beneficiaries in long term care) in the affected counties to enroll in managed care plans.

of health care services for seniors and people with disabilities (SPDs), most of whom were now required to enroll in an MMC plan (SPDs in long-term care or those dually eligible for Medicare were not required to enroll in MMC). It was implemented between June 2011 and May 2012 according to the month of birth for the affected individual (Harbage and King, 2012). The first group to move were those born in the month of May and they were transitioned in June 2011. Next were individuals born in June, then July, and so forth. The final group included those born in April and they were transitioned in May 2012.

The government sent out plan information and enrollment instructions three months before the designated transfer month for each Medicaid enrollee according to her birth month and conducted extensive outreach programs to ensure timely and effective implementation of the transition.[16] Figure 3 shows the variation in MMC enrollment during the reform phase-in by calendar month of birth for Medicaid-insured hospital discharges in SPD reform counties. We restrict attention in this figure to older adults born between 1950 and 1969. As we show further below, this group of older (but not yet 65 during our 2009-2013 study period) adults was differentially affected by the MMC mandates. The figure strongly suggests that the SPD mandates immediately increased the fraction of Medicaid discharges covered by an MMC plan in each group by approximately 30 percentage points.

Results from a survey conducted soon after the SPD reform in late 2012 of about 1,500 SPD Medicaid enrollees who were affected by the mandates found mixed evidence regarding the effects of the policy-induced shift to MMC (Graham and McDonnell, 2016). For example, 40 percent of survey respondents indicated that they needed to get a new primary care physician (PCP) after the shift to MMC and 34 percent reported that their benefits were worse in MMC than in FFS. Strikingly, those in poor health or with more utilization of health care prior to the switch were more likely to report that their benefits were worse with MMC. While informative about Medicaid recipients' satisfaction with the policy-induced shift from Medicaid FFS to MMC, one limitation of this study is that the survey did not include a control group of Medicaid recipients whose enrollment in MMC did not change. Perhaps Medicaid recipients who remained in either FFS or in MMC throughout the period considered by the survey would also have reported in late 2012 needing to shift PCPs or that the quality of their benefits had declined. Additionally, it is possible that the benefits for certain groups shifted to MMC more than offset the costs to those reporting that their MMC benefits were of lower quality and that they had to change PCPs.

Following the introduction of the SPD mandates in California, there was a substantial increase in the share of the state's Medicaid spending going to capitation payments. After remaining steady at about

[16] Enrollees who failed to sign up for MMC plans on their own were automatically signed up in default plans. Approximately 40 percent of affected beneficiaries actively chose a plan. Among those not making an active selection, the state attempted to match them to a plan based on their previous use of medical services. Initially, the majority of those not selecting a plan were assigned to a random default plan. However, over time the state was able to better match beneficiaries based on past providers (Harbage and King, 2012).

15 percent from 1999 through 2010, the share of the state's Medicaid spending going to MMC plans rose to 26 percent by 2013 and accelerated to 52 percent by 2017 (CMS, 2025d). It then trended down slightly to reach 46 percent by 2023 (the most recent year with available data).

II.C. County-Level Analysis of the Effects on MMC Enrollment

To estimate the effect of the SPD mandates on MMC enrollment, we begin with an analysis of monthly, county-level data on the fraction of Medicaid recipients enrolled in MMC. We focus on the 25 California counties shown in Figure 2 that had implemented the COHS, GMC, or Two-Plan MMC models by January 2010.[17] These counties accounted for 91 percent of California's 7.25 million Medicaid recipients in that month and included all of California's 15 most populous counties and 23 of its 25 most populous. We focus on the period through late 2013 given the very large increase in Medicaid enrollment caused by the Affordable Care Act beginning in January 2014 that is apparent in Appendix Figure A.1.[18]

Figure 4 compares the trajectory of MMC enrollment (as a share of Medicaid enrollment) in the 14 "reform" (either Two-Plan or GMC) counties with the 11 COHS counties beginning in January 2010 through December 2013. As the figure shows, MMC enrollment as a share of the Medicaid population increased substantially more in the reform counties than in the COHS counties over this four-year period. To isolate the effects of the SPD reforms on MMC enrollment, we estimate two-way fixed effects (TWFE) difference-in-differences (DiD) specifications of the following form for our sample of 25 (14 reform and 11 control) counties:

$$(1)\quad MMC_{ct} = \alpha_t + \gamma_c + \beta * REFORM_{ct} + \varepsilon_{ct}$$

In this equation, c indexes counties and t indexes year-months. The variable $REFORM_{ct}$ equals 0 in all counties from January 2010 through May 2011. It then increases in increments of 1/12 from June 2011 through May 2012 in reform (Two-Plan or GMC) counties while remaining equal to 0 in COHS counties.

The coefficient of interest in this equation is β, which represents the average differential change in MMC enrollment (as a fraction of total Medi-Cal enrollment) in reform counties relative to COHS counties after the phase-in of the SPD mandate. Due to the ACA-induced Medicaid expansion starting in

[17] Of the remaining 33 California counties, two counties (Kings and Madera) shifted to the Two-Plan model of MMC in March 2011 and three (Ventura, Mendocino, and Marin) moved most of their Medicaid enrollees to COHS managed care in July 2011. The remaining 28 (primarily rural) counties shifted most of their Medicaid recipients into MMC in late 2013.

[18] As the figure shows, from January 2010 through December 2013, Medicaid enrollment in California increased from 7.25 million to 8.61 million. The number of beneficiaries grew rapidly in the subsequent months, reaching 12.17 million by December 2014. It then increased gradually for the next two years and declined gradually during the next three, standing at 12.61 million by December 2019. Medi-Cal enrollment growth then surged again during the pandemic and reached a peak of 16.05 million in May 2023.

January 2014, we focus on the period from January 2010 through December 2013, which gives us 17 months of pre-reform data in the 14 reform counties, 11 months of partial implementation, and 20 months after the reform is fully implemented. Each specification includes 25 county and 48 year-by-month fixed effects and standard errors are clustered by county. The coefficient estimate for β in this equation is 0.120 (with a standard error of 0.008), suggesting MMC enrollment rose by 12.0 percentage points more in the average reform county than in the average control county because of the SPD reforms.

The specification above assumes parallel trends in MMC enrollment in reform relative to COHS counties in the time leading up to the reform. However, a close review of Figure 4 suggests that MMC enrollment was growing more rapidly in reform counties than in COHS counties even before the SPD mandates started to take effect. To account for this potential source of bias, we next estimate TWFE DiD specifications of the following type, which allow for a differential trend for COHS counties:

$$(2)\quad MMC_{ct} = \alpha_t + \gamma_c + \beta * REFORM_{ct} + \mu * COHS_c * t + \varepsilon_{ct}$$

After introducing this trend, the estimate for β declines by about half (to 0.064) but remains highly statistically significant.[19] As expected, given the patterns shown in Figure 4, the estimate for the COHS linear trend is negative (since enrollment in reform counties was trending up) and is statistically significant.

Of course, these specifications include just aggregate county-level MMC enrollment and do not differentiate between those most affected by the reform and those who were largely unaffected. In a companion set of county-level specifications that we describe in the next section, we show that the impact of the mandates on the fraction of Medicaid hospital discharges covered by MMC is substantially larger than on enrollment alone. This is not surprising given that the disabled and elderly Medicaid recipients affected by the SPD reforms utilized significantly more health care on average than the typical Medicaid recipient. The estimated effect is even larger when we consider only Medicaid discharges among those aged 40 to 64, who as we show are the age groups most affected by the MMC mandates that we study.

III. Hospital Discharge and Emergency Department Encounter Data

Our main data sources are the official records of all non-federal hospital patient discharges and emergency department (ED) visits in California from 2008 until 2013. Each year, there are approximately

[19] The corresponding estimates for β in the MMC enrollment specification when weighting by the county's share of total Medi-Cal enrollment in the year-month are very similar at 0.110 and (with COHS trend) 0.057, and both estimates are highly statistically significant.

3.8 million hospital discharges and 10 million ED visits in this data.[20] For each discharge and ED visit, we observe detailed information on the demographics of the patient, main source of payment, diagnoses and procedures, along with admission and disposition routes. Additionally, for the patient discharge data, we observe charges and enrollment status in MMC.[21] Importantly, we can link the two datasets together and track people over time with their (encrypted) social security numbers. This allows us to track patients across health insurers (or if they become uninsured) over time. Therefore, if a patient transitions from Medicaid into Medicare or into commercial health insurance coverage or becomes uninsured, she would still be present and accounted for in our data.[22] This is an important advantage of our data relative to most previous research examining the impact of MMC, which has relied primarily on administrative data from only the Medicaid program.

We augment the patient discharge data (PDD) with official California death records from 2009 until 2013. For all individuals with at least one hospital discharge between 2008 and 2013 and who have died by the end of 2013 (either in hospital or after discharge), we are able to link their death record to our inpatient utilization data through the encrypted social security number. While this provides complete information about mortality both in and out of the hospital, our sample for the mortality analysis necessarily only includes individuals who had at least one inpatient stay (either before or after the reform). These linked death files enable us to construct a sample of panel data that tracks healthcare utilization for each patient over time that accounts for the lack of observed utilization after death. It also allows us to estimate the mortality effects of mandating MMC enrollment for the SPD population of beneficiaries. Having death records (rather than solely hospital discharge data) is very useful for our analysis since approximately two-thirds of the deaths among individuals in our sample occur outside of the hospital.

Using this data, we first estimate a companion set of TWFE county-level specifications to those described in the preceding section for the 14 reform and 11 control counties. In this case, the outcome variable is the fraction of a county's Medicaid discharges covered by MMC in each month between January 2010 and December 2013.[23] Our estimated effects for the effect of the mandate are about 25 percent larger than using the Medicaid and MMC enrollment data. For example, rather than an estimated mandate effect of 12.0 percentage points on the fraction of a county's Medicaid recipients on MMC, we estimate the mandate increased the percentage of a county's Medicaid discharges on MMC by 15.4 percent. This

[20] ED visits that lead to a hospital admission are included in the discharge data only.
[21] Note that Medicaid managed care indicator is only available in hospital discharge data. Thus, in the ED data, we cannot tell apart MMC and Medicaid fee-for-service. Additionally, it appears that some discharge and especially ED visits paid by MMC may have been miscoded as "private" in the datasets and thus may explain why we observe first-stage effect of the mandate on private payers below.
[22] Table 1 shows that, while (by design) no one in the sample had any Medicare covered hospital discharge in 2009Q2-2010Q1, by the end of 2013, about 14 percent of discharges in treatment counties (19 percent in COHS counties) have Medicare as the main payer.
[23] Results are similar if we start one or two years earlier, but we begin in January 2010 for consistency with the county enrollment analyses since January 2010 is the first month for which we could obtain MMC and Medicaid enrollment by county and month.

estimate declines to 8.0 percentage points when allowing for a differential trend in COHS counties, also about 25 percent larger than the analogous 6.4 percentage point estimate with the enrollment data.

As with the analyses using the enrollment data, these TWFE specifications consider discharges for many Medicaid recipients such as infants and other children who were largely unaffected by the MMC mandates. Figure 5 shows that the fraction of Medicaid discharges on MMC increased most in reform counties from early 2011 (just before the mandates) to early 2013 (just after) for the people aged 40 to 64. This is presumably because a relatively large share of Medicaid recipients in this age group has a disability. The estimated effects focusing on just this age group are more than twice as large at 35.9 percentage points in the baseline TWFE specification and 29.2 percentage points with the COHS trend included. These results along with Figure 5 strongly suggest the SPD mandates in reform counties caused a very substantial increase in MMC coverage for older Medicaid recipients relative to those in control counties.

In the empirical analyses that follow, we restrict attention to Medicaid recipients under age 65 since a large share of recipients transition from Medicaid to Medicare immediately upon reaching age 65. As shown in Appendix Figure A.2, the fraction of all California hospital discharges with Medicaid as the primary expected payer is about 20 percent for those aged 63 and 64 versus just 7 percent for those aged 65 and 66 (and declines further at older ages). As a result, Medicaid recipients in their 40s, 50s, and early 60s were much more affected by the mandates than their Medicaid-insured counterparts aged 65 and up.

IV. Construction of Analysis Sample

Our main analysis sample is a person-quarter level linked panel dataset of hospital discharges, emergency department (ED) visits, and mortality. We lack data on California's entire Medicaid enrolled population since Medicaid recipients (and other individuals) appear in our administrative data only if they utilize hospital care. We therefore use this large-scale administrative data set that includes all California hospital admissions and ED visits to construct our analysis sample. Specifically, we first restrict our sample to people with one or more hospital discharges or ED visits covered by Medicaid (and no discharge or ED visit covered by Medicare[24]) between the second quarter of 2009 and the first quarter of 2010. We have selected this 12-month time range for two reasons. First, as the time range is moved further from the policy intervention that begins in the second quarter of 2011, it becomes less likely that an individual insured by Medicaid is still insured by the program when the MMC mandates take effect in reform counties.

[24] To reduce confounding from Medicare policies, especially since dual-eligible beneficiaries were exempt from the MMC mandates. Some individuals in our sample transition into Medicare before age 65 because of enrollment in the Social Security Disability Insurance (SSDI) program. Importantly our data still allows us to capture the health care use of these "dually eligible" (for both Medicare and Medicaid) individuals, though Medicare is typically the primary payer (and Medicaid secondary) for these individuals.

Second, this time range allows us to have at least four quarters of pre-mandate data (following the 2009Q2 through 2010Q1 sample selection period) for every individual in the sample, since the first MMC mandates in the 14 reform counties take effect in June 2011.

We also restrict our sample to those born in or before 1969 to focus on the older adults most likely affected by the policy change and to individuals born in or after 1950 to ensure that all are still below age 65 by the end of our sample period (to avoid the transition via aging into Medicare, when Medicaid would become the secondary payer for most eligible for both programs). This group ranges from age 40 to age 59 by the end of 2009. Figure 6 displays the fraction of Medicaid discharges in MMC by single-year-of-age prior to and following the SPD mandates (in 2011Q1 and 2013Q1) in our 14 reform counties. As the figure shows, older adults in their forties, fifties, and early sixties were much more affected by the MMC mandates than adults in their twenties and thirties. Appendix Figure A.3 shows the corresponding data for COHS counties, where there was little increase in MMC enrollment at any age.[25]

It is worth noting that, prior to the SPD mandates, many SPD beneficiaries in reform counties voluntarily enrolled in MMC (Harbage and King, 2012). The steady decline in the pre-reform share on MMC from about 30 percent in the early forties to about 12 percent in the early sixties shown in Figure 6 suggests voluntary MMC enrollment was lower at older ages. Despite this and as is apparent from Figure 6, MMC enrollment was actually higher after the SPD mandates for this older group, reaching about 60 percent for those in their early sixties by 2013Q1. This share remained substantially lower than 100 percent since certain Medicaid-insured groups in reform counties (such as those in long-term care or who were dually eligible for Medicare) were still exempt from the mandate.

To further focus our analyses on those most likely affected by the reform, we exclude some groups from our sample. First, we restrict to those residing in one of the 12 Two-Plan, 2 GMC, and 11 COHS counties described above that had no other significant changes in MMC policies during our study period.[26] We also exclude all birth or pregnancy-related discharges, a category of beneficiaries that has been researched extensively in the existing literature and were largely unaffected by the MMC reforms for seniors and people with disabilities.[27] We also exclude individuals who are missing an encrypted SSN or who have disagreeing birth dates when linking discharges and ED visits within the same encrypted SSN.

[25] Children were largely unaffected by the SPD mandates since most were already required to be in MMC. We do not consider individuals aged 65 and older given the sharp decline in Medicaid enrollment from age 64 to age 65 and beyond apparent in Appendix Figure A.2.
[26] As shown in Figure 2, the reform counties include: Los Angeles, San Diego, Riverside, San Bernardino, Santa Clara, Alameda, Sacramento, Contra Costa, Fresno, Kern, San Francisco, San Joaquin, Stanislaus, and Tulare. The COHS control counties include: Orange, San Mateo, Santa Barbara, Solano, Monterey, San Luis Obispo, Santa Cruz, Yolo, Napa, Merced, and Sonoma. In our hospital discharge and ED data, 92 and 88 percent, respectively, of observations are for residents in these counties in 2009Q2-2013Q4. The shares are similar when restricting attention to just Medicaid-insured discharges.
[27] More than 95 percent of Medicaid-insured births are to women under the age of 40 and thus are not eligible for inclusion in our sample. We also winsorize length of stay at 365 days in the hospital discharge data since longer stays are very likely to be errors.

We then drop person-quarter level observations after the recorded death, after also dropping individuals with erroneous death records (e.g. individuals with multiple death records or with observed discharge and/or ED visits after recorded death, which occurs for less than 0.03 percent of our observations).

To minimize the confounding effects of other policy changes, most notably the implementation of the ACA's Medicaid expansion starting in January 2014, we restrict the analysis period to April 2009 until December 2013. By construction, each person in the sample has one or more Medicaid-insured hospital discharges or ED visits in at least one quarter from 2009Q2 through 2010Q1. We then populate each person-quarter cell from 2009Q2 through 2013Q4 with the number of hospital discharges and ED visits for the individual in that quarter, dropping all quarters after an individual dies.[28]

Each person-quarter cell contains information on individual characteristics (age, gender, county of residence) and healthcare utilization (total number of hospital discharges by payer, total number of ED visits by payer, total length of stay, and number of procedures associated with hospital discharges). We then construct policy change indicators based on individual birth month, county of residence,[29] and quarter of observation. Table 1 contains descriptive statistics for our sample. Panel A contains data for only individuals living in reform countries while Panel B contains data on those living in COHS counties. Overall, our final sample includes 182,859 patients (156,740 in reform and 26,119 in control counties) and declines to 175,217 in the post-implementation period since individuals exit from our sample as they die.

The first column of Table 1 reveals that, during the sample selection period, individuals in the two groups of counties are similar with respect to average age (49.2 in reform and 48.8 in control), the fraction who are female (63 percent and 61 percent), and the share who are of Hispanic origin (33 percent versus 29 percent). However, the fraction of people in the sample who are Black is more than twice as high in reform counties (22 percent versus 10 percent). As expected, the average age in both groups increases over time as shown in the subsequent three columns while the other averages all remain stable (since death is the only way that individuals are dropped from the sample).

As shown in Table 2, during the sample selection period, the average number of ED visits per quarter is higher in COHS counties (0.58 versus 0.52) while average discharges per quarter are higher in reform counties (0.18 versus 0.15). An important consideration caused by our selection criteria is that

[28] About 40 percent of the individuals in our sample do not have any hospital discharges from 2009 through 2013. For these individuals, we do not know if they die (unless the death occurs in the hospital). For the mortality analyses, we therefore focus only on those with one or more hospital discharges. For other outcomes, we present results for the broader sample but estimate a companion set of specifications with the smaller sample, all of which yield qualitatively similar results.

[29] We assign individuals to their initial county throughout our study period. While some individuals do move counties, in many cases it is from a reform county to another reform county (or a COHS county to another COHS county). For less than 3 percent of discharges in our sample, the individual resides in a COHS county initially and reform county later or vice-versa. This would bias against our finding an effect of the SPD reforms. Another 2 percent of discharges take place in a non-reform, non-COHS county. One limitation of our data is that we do not know if an individual moved out of state.

individuals enter our sample only if they have a Medicaid-insured discharge or ED visit between the second quarter of 2009 and the first quarter of 2010. As would be expected, utilization for these individuals exhibits some degree of mean reversion in subsequent periods. This can be seen in the relatively large decline in utilization between the sample selection and the pre-reform periods summarized in Table 2. For example, in both our reform and COHS counties, the average number of discharges and of ED visits per quarter declines by about 30 percent from the sample selection period.

For three reasons, this is unlikely to cause economically meaningful problems for our empirical analyses. First, we exploit the staggered enrollment of individuals into MMC based on their month of birth. Since mean reversion is occurring for all members of the sample and should be unrelated to birth month, this plausibly exogenous variation should limit any bias. Second, a similar reversion is happening in the COHS county sample that serves as our control group. To the extent there is any relationship between birth month and the timing of mean reversion, this control group will help to address potential concerns about bias in our coefficient estimates. Finally, in our estimates of the effects of the MMC mandates on health care utilization, we do not use data from the sample selection period, so the higher utilization during that time does not directly affect our estimates. In particular, the baseline utilization of medical services that we use to interpret the magnitude of our coefficients is based on pre-reform (beginning in 2010Q2) utilization and not on utilization in the sample selection period in Table 2.

V. Econometric Strategy

To estimate the causal effect of enrollment in Medicaid managed care on healthcare utilization and health outcomes, we exploit two main sources of variation in the implementation of California's SPD mandate. The first source of variation is driven by geography. As discussed above, a large share of Medicaid recipients in our sample were residing in reform counties that were affected by the mandates. In contrast, Medicaid recipients living in COHS counties were largely unaffected, since most had previously been enrolled in MMC. This latter group serves as our control group to account for the possibility that other statewide factors unrelated to the MMC mandate are influencing the utilization of health care and health outcomes among Medicaid recipients. The second source of variation is temporal. Within reform counties, the precise timing of the MMC mandate binding on an enrollee depended on an individual's month-of-birth. As shown in Figure 3, this had a substantial impact on the exact timing of the increase in MMC enrollment, with those born in May shifted to MMC earliest (in June 2011) while those born in April were transitioned last (in May 2012).

Our main TWFE DiD specification estimating the effect of MMC on our outcomes of interest leverages variation both across and within (by month-of-birth) counties as follows:

$$(3)\quad Y_{jct} = \gamma_t + \alpha_j + \beta * MANDATE_{jct} + \varepsilon_{jt}$$

In this equation, j indexes individuals, c indexes counties, and t indexes year-quarters (e.g. 2011Q4). The variable $MANDATE_{jct}$ is equal to 0 throughout our study period for those residing in COHS counties. For their counterparts in reform counties, this variable "turns on" in the quarter that the mandate takes effect. For example, if a person in the sample is born in the month of November (by construction between 1950 and 1969 inclusive) and is thus required to shift to MMC in the fourth quarter of 2011, this variable would equal 0 in the preceding quarters and would be equal to 1 in 2011Q4 and thereafter. The variables γ_t and α_j are year*quarter and individual fixed effects to control for variation over time (e.g. if utilization is trending up throughout the state) and variation in health care utilization across individuals (e.g. some individuals were utilizing relatively more care even before the mandates).[30] Since the implementation of the reform varies by county and month-of-birth, standard errors are clustered at the county-by-mandate quarter level.[31]

Our main identifying assumption is that the timing of the mandate-induced increase in MMC enrollment is orthogonal to other factors that might affect an enrollee's utilization of healthcare services and health outcomes. This assumption seems reasonable since the policy-induced increase in MMC enrollment is based on the calendar month of birth across all ages. For example, a Medicaid recipient living in a reform county born in June of year t (sometime between 1950 and 1969) would be affected by the mandate 9 months earlier than an otherwise similar person born in March of that same year.[32]

For each person in the sample, we include 11 quarterly observations (unless the person died before the end of our sample period, in which case the final quarter would be the quarter of death) to create a balanced panel. For those in reform counties, we include four quarters of pre-mandate data, one quarter during which the mandate takes effect, and 6 post-mandate quarters. Therefore, for a person born in

[30] We do not include county fixed effects since we have individual fixed effects. While a small fraction of our sample does switch counties during our study period, less than three percent of the sample transitions from a reform to COHS county or vice-versa.

[31] There are 25 counties in our sample and the mandate is implemented over five quarters, resulting in 125 clusters. Two individuals would be in the same cluster if the mandate is implemented for them based in the same quarter (based on their month of birth) and if they live in the same county.

[32] There could be a concern that there is a connection between an individual's month of birth and later life outcomes as demonstrated in Buckles and Hungerman (2013). There are at least three reasons that this is unlikely to be a substantial concern in our setting. First, while the literature has demonstrated a statistically significant effect, these differences are not large in magnitude relative to our estimates below. Second, a threat to identification would come not simply from a difference in underlying health but also a change in that health that is coincident with the mandate. There is little reason in the literature that would suggest such an interaction several decades after a person is born. Finally, our inclusion of control individuals residing in COHS counties where there was no SPD mandate should account for any potential changes in health that might otherwise occur among people born in a certain calendar month.

October of 1955, we include the period 2010Q4 through 2011Q3 as the pre-reform period, 2011Q4 as the reform quarter, and 2012Q1 through 2013Q2 for post-reform. The quarters included are analogous for those in COHS counties, such that we would, for example, include 2010Q4 through 2013Q2 for a COHS county resident born in October.

When considering the outcome of mortality, we are obviously unable to include individual fixed effects. We therefore include controls for each individual's gender, age, race, and ethnicity along with fixed effects for county of residence in these specifications. Additionally, as discussed above, we also only have complete mortality information for those with one or more hospital discharges during the 2008 through 2013 period and therefore exclude the 38 percent of observations in our sample without a hospital discharge.[33] We also estimate a companion set of specifications for our health care utilization outcomes with this smaller sample and, as described below, obtain similar results to those from the overall sample.

We also estimate event-study specifications that allow us to examine the time path of outcomes before and after the reform-induced shift to Medicaid managed care in reform counties relative to the control COHS counties. This allows us to more clearly investigate the presence of any differential pre-trends in our outcome variables of interest and the time path of the estimated differential changes after mandatory enrollment. We include a full set of pre (4 quarters prior with the quarter before mandate excluded) and post (7 quarters including mandate quarter) mandate indicator variables that depend on each person's birth month, county of residence, and the quarter observed. When considering mortality, we estimate a modified version of this specification without individual fixed effects as described above.

Our primary outcomes of interest are driven either by where there have been observed effects in the previous literature or where there are reasonable policy concerns about privatization. These include inpatient hospitalizations and ED visits (our two sources of utilization data) along with mortality. For mortality, we have a person-quarter level indicator for mortality (which accounts for death either inside or outside of the hospital) and we drop all observations for individuals after their death.

Given our interest in heterogeneity in the effect of MMC by underlying health status, we also estimate models that allow the effect of the mandate to vary based on the health of the beneficiary. We measure health status using data on an individual's pre-reform use of medical services and associated diagnoses. More specifically and following previous research (Doyle, 2011; Finkelstein et al., 2016), we use the Elixhauser score, which is a count of the chronic conditions that are indicated by a patient's principal diagnosis or co-diagnosis in either the inpatient discharge data or the ED visit data. One disadvantage of this measure is that all conditions are weighted equally in the score. To address this potential limitation, we also use the Charlson Comorbidity Index (as in Gupta et al., 2024), which gives a

[33] These 38 percent have only ED visits during our selection and analysis time periods.

larger weight to conditions that make a larger contribution to estimated ten-year mortality rates. In both cases, higher values of the respective index correspond to worse health.

Finally, we examine the effect of market structure on our outcomes by estimating models that investigate whether the effect of the policy-induced increase in MMC enrollment on health care utilization and mortality varies based on whether enrollees are in Two-Plan counties such as Los Angeles (where one commercial plan competes with a Local Initiative plan) or in a GMC county such as San Diego (where several private commercial insurers compete for MMC enrollees).

VI. The Effect of Medicaid Managed Care on Hospital Discharges, ED Visits, and Mortality

As shown in Figure 5, the share of Medicaid discharges in our analysis sample covered by MMC increased substantially in our 14 Two-Plan/GMC counties relative to the 11 COHS counties. As shown in Figure 3, the timing of this increase coincided with the timing of the mandate, which varied by calendar-month-of-birth within reform counties. This provides clear graphical evidence that the SPD mandate significantly impacted the MMC enrollment of the disabled adults included in our sample.

We now more formally estimate the relationship between the MANDATE variable described above and our outcome variables of interest. We begin with an examination of hospital discharges and follow this with analyses of ED visits and mortality. For each outcome variable, we include 11 observations for everyone (unless an individual dies before their 11th quarter) so that each person in the sample contributes equally to the coefficient estimates.

VI.A. Effect of MMC Mandates on Hospital Discharges

The average number of hospital discharges per quarter in the analysis sample (including both reform and COHS counties) during the pre-reform period was 0.117. While individuals are included in our analysis sample based on their enrollment on Medicaid, not all discharges are necessarily covered by Medicaid. This is the result of substantial churn in individuals' health insurance coverage even during this relatively short pre-reform period. This churn is non-trivial and further demonstrates the value of having the entire universe of California's hospital discharge data. As can be inferred from Table 2, this includes not only discharges covered by Medicaid fee-for-service or Medicaid managed care but also by Medicare, private health insurance, self-pay (uninsured), and all other insurance.

Table 3 shows that, within reform counties, those in our sample who were already in MMC during the selection period (2009Q2 – 2010Q1) differed in important and expected ways from those still in traditional fee-for-service Medicaid. For example, the average number of ED visits and hospital discharges

was higher for those in FFS. Amplifying this difference, the average length-of-stay, average charges, and average number of diagnoses per discharge were higher among FFS recipients than those in MMC. This could of course be driven by different incentives between the two types of Medicaid. But the average health status of those in FFS Medicaid was also significantly lower than their counterparts in MMC as measured by both the Elixhauser and Charlson scores (higher scores for both measures correspond to worse health). These baseline differences suggest that the mandates affected Medicaid recipients who were in relatively worse health, which matches our expectation that policymakers were likely hesitant to expose these medically frail individuals to the potential downsides of privatization.

To facilitate interpretation of the coefficient estimates from the initial specifications, we scale the number of discharges in each quarter by 100 in constructing our outcome variables, which are equal to the total scaled number of discharges in a quarter along with the number of discharges by each major type of insurance (e.g. MMC, Medicaid FFS, and so forth). Thus, the average number of scaled discharges per quarter in the pre-reform period is 11.7 rather than 0.117 as described above. The TWFE specification summarized in the first column of Table 4 suggests there was relatively little change in the overall number of discharges per quarter for Medicaid recipients residing in reform counties because of the MMC mandates. More specifically, the point estimate for β of 0.071 suggests that there was just a 0.6 percent increase in the number of discharges per quarter, and this estimate is not statistically significant. Given the precision of the estimate (with a standard error of 0.124), we can rule out mandate-induced increases of more than 2.7 percent or reductions of more than 1.5 percent in the number of discharges per quarter.

Of course, not all discharges of Medicaid recipients residing in reform counties were directly affected by the MMC mandates. For example, some were already enrolled in MMC plans even prior to the mandates while others remained in fee-for-service throughout (most likely due to waivers from the mandate). The specifications summarized in the next two columns shed light on this, with a large and statistically significant increase in the number of MMC-covered hospital discharges and an even larger reduction in the number of Medicaid fee-for-service discharges. The event study versions of these two specifications are summarized in Figure 7. Given the magnitude of the point estimates relative to the pre-reform mean in the total number of discharges, it appears that almost 20 percent of discharges switched from Medicaid FFS to MMC because of the mandates. This effect would likely be much larger but for the fact (as shown in Table 2) that a non-trivial share of the sample transitions to other types of health insurance (or to being uninsured) before, during, and after the SPD mandates.

Interestingly, the fourth column reveals that the MMC mandate was associated with a substantial increase in (other) privately insured hospital discharges. As we discuss further in the next sub-section while summarizing the results for ED visits, it appears that hospitals in some cases coded those with MMC

as privately insured.[34] Indeed, if one adds the MMC and privately insured coefficients, the resulting sum is almost identical to the corresponding estimate for Medicaid FFS, suggesting little change in the overall number of Medicaid hospital discharges. The estimates for Medicare and self-pay discharges in columns 5 and 6 are comparatively small and statistically insignificant. Interestingly and as shown in the final column, there is a small yet statistically significant increase in discharges with other insurance coverage, primarily driven by the sub-category "other payer" (as opposed to the other sub-categories: workers compensation, other government, county indigent, other indigent, and other federal programs).

This first set of results confirms that the SPD mandates in reform counties significantly increased MMC enrollment but had relatively little impact on total hospital discharges among disabled adults. We next explore whether the type or source of admission was affected by the mandate-induced transition from Medicaid fee-for-service to Medicaid managed care. The second and third columns of Table 5 provide little evidence to suggest that there was a different effect on scheduled versus unscheduled admissions, which is an outcome that one might expect would be of interest to an MMC plan. The next three columns also suggest there was not a differential change for those admitted from a hospital's own emergency department or who were transferred from other hospitals. Taken together, our results suggest that the MMC mandates had relatively little effect on admissions of disabled adult Medicaid recipients to general acute care hospitals.

VI.B. Effect of the MMC Mandates on Emergency Department (ED) Visits

We next explore the effects of the MMC mandates on total emergency department (ED) visits and the number of visits by health insurer. For simplicity, we once again multiply our outcomes by 100, so that the mean number of (scaled) pre-reform ED visits per quarter is 37.0. In contrast to the hospital discharge data, California's ED data does not have a separate category for Medicaid managed care. Our examination of the data suggests that, in some cases, hospitals code those shifting into MMC as Medicaid while in other cases they are coded as privately insured. We therefore focus primarily on the estimates for total ED visits while also summarizing each of the (imperfectly measured) primary insurance categories.

The TWFE specification summarized in the first column of Table 6 shows that the MMC mandates were associated with a large and statistically significant increase in ED visits. More specifically, the coefficient estimate for β is 1.372, representing a 3.7 percent increase from the pre-reform mean, with the 95 percent confidence interval ranging from 1.8 percent to 5.6 percent. The actual effect on those who

[34] This problem has been noted in other areas as well. For example, Kwon et al. (2023) examine the insurance status of individuals in the Pennsylvania statewide cancer registry and find that enrollment in MMC was associated with a statistically significant increase in misclassification as being privately insured.

were required to switch their coverage from Medicaid fee-for-service to MMC because of the mandate is likely much larger, since a large share of the individuals in our sample were either already in MMC, remained in Medicaid FFS, or transitioned to another form of health insurance such as Medicare or other private health insurance.

Figure 8 summarizes the event study version of this specification. While the pattern of coefficients in the four quarters prior to the mandate suggests a modest pre-trend, the figure clearly reveals that ED visits markedly increase in the quarter that the mandate is introduced, increase further in the next quarter, and then remain elevated thereafter. The point estimates in the latter quarters suggest an increase of about 2 ED visits (per 100 recipients) per quarter, or more than 5 percent of the base of 37.0 (per 100 recipients) per quarter. This finding is even more striking when one considers that only a subset of sample individuals in reform counties are directly affected by the reform.

Considering the recent and growing literature on potential concerns with common TWFE difference-in-differences analyses (Goodman-Bacon, 2020; Callaway and Sant'Anna, 2020; Sun and Abraham, 2020; Baker et al, 2021), we investigate the robustness of this result for ED visits using a stacked DiD specification (following Wing, Freedman, and Hollingsworth, 2024).[35] Our results with this alternative approach are quite similar and suggest an even larger effect of the mandate-induced increase in MMC on ED visits, with an estimate for β of 1.77 (versus 1.37 in Table 6). And as shown in Appendix Figure A.4, the results from the event study version of the stacked DiD are very similar to the standard TWFE estimates depicted in Figure 9.

There are multiple possible explanations for the increase in ED visits following the shift to MMC. For example, physician networks may be narrower in MMC than in traditional FFS such that Medicaid recipients have difficulty getting a primary care physician and turn to the ED for some types of medical care. This would be broadly consistent with prior survey evidence (Graham and McDonnell, 2016). Recent research has shown that disruption in an individual's PCP relationship can lead to increases in both ED visits and mortality (Sabety, 2023). In addition, individuals in MMC plans may have more difficulty in obtaining access to prescription drugs, which could also have immediate effects on underlying health.

While we cannot test for this directly, we can explore whether the MMC effect varies by type of visit to the emergency department. In Appendix Table 1, we summarize the results from TWFE specifications for 9 different categories of ED visits.[36] In seven cases, the estimated effects are positive, and six of these seven estimates are statistically significant. The three exceptions are ED visits for injuries,

[35] When implementing our stacked DiD specification, we following Wing, Freedman, and Hollingsworth (2024) and apply the corrective sample weights suggested by the authors.

[36] We classify the types of ED visits according to the ICD-9 groups developed by the NYU Center for Health and Public Service Research that is available at: https://wagner.nyu.edu/faculty/billings/nyued-background

drugs, and alcohol, all of which may be less sensitive to access to primary care than other conditions. The largest proportional increases are found for psychiatric-related visits (7.7 percent of the baseline mean) and non-emergent visits (6.8 percent), both of which may be more affected by reduced primary care access.

As mentioned previously, there is not a separate category for MMC in the ED visit data. It therefore seems likely that MMC visits to the ED are frequently coded as Medicaid. Consistent with this, ED visits with a primary payer of Medicaid appear to have fallen only slightly (by 1.2 percent of the mean), as evidenced by the statistically insignificant point estimate in column 2 of Table 6. In contrast, the number of privately insured ED visits increases significantly, with the point estimate of 1.488 accounting for the entire mandate-induced increase in quarterly ED visits suggested by the first TWFE specification.

The next three columns suggest a statistically significant mandate-induced reduction in Medicare-insured ED visits, an increase (though not statistically significant) in self-pay (uninsured) visits, and an increase in ED visits with other sources of health insurance. This final increase of 0.281 per quarter is almost entirely driven by the sub-category "other federal programs" and is of similar magnitude to the reduction for Medicare (-0.235). As mentioned previously, many individuals in the sample become eligible for Medicare during our study period. It is possible that the shift to Medicaid managed care subsequently makes these individuals less likely to transition to Medicare fee-for-service and more likely to transition to a private Medicare Advantage plan (which may be coded as other federal program).

Table 7 explores the issue of heterogeneity within our sample, specifically whether the effect of the mandates on ED visits was larger for those in worse baseline health status. As mentioned previously, higher baseline values for both the Charlson Score and the Elixhauser Score correspond to worse baseline health. These specifications include a separate set of time (year*quarter) fixed effects for those in worse baseline health to account for the possibility that utilization is trending differently during our study period for this group. The coefficient estimates displayed in the first two columns reveal that the mandate induced increase in ED visits was about 5 times as high for those in worse health by the baseline Charlson score. As a share of the pre-reform average, ED visits increased by 5.3 percent for those in worse health by this metric versus just (a statistically insignificant) 1.7 percent for those in better health. Consistent with this, the next two specifications show that the pattern is similar when measuring health by the Elixhauser score, with a 4.9 percent increase for those in worse health versus a 2.2 percent increase for others. For both measures of health status, our results show a much larger effect for those in worse baseline health and these differences are statistically significant at the 1 percent level.[37]

[37] One potential concern with these estimates is that those in better health may be more likely to be in MMC prior to the mandates, and thus less likely to experience a transition to MMC. Thus, rather than reflecting a differential effect of MMC on utilization, it might instead be capturing the effects of a larger share of recipients transitioning to MMC. In a companion set of specifications, we drop those already in MMC during the sample selection period and obtain qualitatively similar results. For example, we estimate a mandate-induced increase of 3.2 percent in the number of ED visits and this estimate is significant at the 1 percent level.

The results summarized in this sub-section strongly suggest that the mandate induced shift from traditional Medicaid to MMC led to a substantial and statistically significant increase in ED visits among disabled adult Medicaid recipients in Two-Plan and GMC counties. The increase in ED visits was significantly greater among those in worse health. These findings suggest that, relative to traditional Medicaid, MMC in reform counties either reduced access to health care or led to worse health outcomes (or a combination of the two). In the next sub-section, we explore whether the mandates were associated with a change in what is arguably the most important health measure for this vulnerable group – mortality.

VI.C. Effect of the MMC Mandates on Mortality

In this section, we focus on a subset of the analysis sample described above to estimate the effect of the MMC mandates on mortality. As described previously, we can observe mortality (even if it occurs outside of the hospital) for those with at least one hospital discharge during the 2008 through 2013 period. Some of the individuals in our sample are selected solely based on one or more Medicaid-insured ED visits in the selection period (2009Q2 through 2010Q1) and do not have any hospital discharges during this 12-month period or any other time during the 2009 through 2013 period. We therefore focus on the 113,836 individuals (62 percent of the full sample) with at least one discharge when considering mortality as an outcome variable.[38]

The average quarterly mortality rate in this restricted sample is 0.81 percent. This is several times greater than one would expect for a group with an average age of 50 that is predominantly female (60 percent of this sub-sample). More specifically, the average *annual* mortality rate of a 50-year-old man in the U.S. in 2012 was 0.52 percent and for a 50-year-old woman in that same year was just 0.32 percent. This would suggest a quarterly mortality rate of only about 0.1 percent for a random subset of individuals with the same age and gender breakdown, which is about 88 percent lower than in our sample. Put another way, the Medicaid recipients in our analysis sample are in much worse health than the typical American of the same age and gender, with a mortality rate more than 8 times greater.

Table 8 summarizes the results for mortality, in which we also include fixed effects for age, gender, and county (since we are unable to include individual fixed effects). As the first column shows, the MMC mandates are associated with a statistically significant (at the 5 percent level) increase in quarterly mortality. The point estimate of 0.059 percent is 7.3 percent of the baseline mean. Consistent with the results for ED visits, the effects are significantly larger for those in worse health as measured by their baseline

[38] In a companion set of specifications not summarized here, we estimate TWFE specifications for total ED visits and other measures of health care utilization with this more restricted sample and obtain qualitatively similar results. For example, we estimate a mandate-induced increase of 4.1 percent in ED visits per quarter that is statistically significant at the 1 percent level.

Elixhauser and Charlson scores. For example, mortality rates increase by 8.6 percent of the baseline mean mortality rate for those in worse health as measured by the Charlson score versus just 5.0 percent for their counterparts in better health by this same measure.

Figure 10 shows an event study version of the mortality result (with one quarter before the mandate as the omitted category). While none of the quarterly indicators are statistically significant on their own, all 7 of the mandate and post-mandate coefficients are greater than 0 and all are larger than each of the 3 corresponding estimates from the pre-reform period. This strongly suggests that the adverse effects on mortality are not simply temporary but persist for at least 1.5 years after the mandate. And as shown in Appendix Figure 5, the results are qualitatively similar when we estimate a stacked DiD event study of this mortality specification.

VI.D. Exploring Heterogeneity in Effects of MMC by Market Structure

As described above, there are two types of reform counties in California that implemented the transition of many seniors and people with disabilities to MMC in 2011 and 2012. The first group consisted of just two counties (Sacramento and San Diego) that implemented the Geographic Managed Care model, through which four or five commercial health insurers competed for MMC enrollees. The second group implemented the Two-Plan Model, through which one commercial health insurer competed with a "Local Initiative" plan. This latter group included a dozen California counties such as Los Angeles, Riverside, San Bernardino, and Santa Clara. During our sample period, approximately 58 percent of individuals in MMC in Two-Plan counties were enrolled in the Local Initiative plan with the rest in the commercial one.

Theoretically, each MMC model has potential advantages compared to the other. For example, GMC counties provide MMC enrollees with twice as many options, which may provide considerable value to Medicaid recipients. In addition, the competition between plans could result in positive outcomes for enrollees as plans attempt to attract and retain them. On the other hand, it seems likely that individuals in GMC counties with 4 or 5 plans (as opposed to just 2) are more likely to transition plans at the end of the year, which may reduce GMC plans' financial incentives to encourage health care that generates long-term (as opposed to immediate) health benefits. Also, the Local Initiative plan's financial incentives may not be as strong as a commercial plan's, which could have either positive or negative effects on access to and the quality of health care. It is therefore ultimately an empirical question whether either approach is preferable, with the caveat that what works well in Two-Plan counties such as Los Angeles may not work well in GMC counties Sacramento and San Diego (and vice-versa).

To investigate this issue, we estimate specifications in which we introduce a variable GMC_MANDATE that "turns on" only for Medicaid recipients in Sacramento and San Diego counties.

It is identical to the MANDATE variable described above for Medicaid recipients in our sample residing in Sacramento and San Diego but is equal to zero in all periods for all Medicaid recipients residing in Two-Plan counties (and for all individuals residing in the COHS counties, consistent with the MANDATE variable). This allows us to test for any significant differences between the mandate-induced effects of MMC in GMC versus Two-Plan counties.

The final two columns of Tables 7 and 8 summarize the results from specifications for ED visits and mortality. For the mortality specification, we include just a subset of our sample for the reasons described above. The coefficient estimates provide an overall mixed picture. More specifically, our estimates suggest that ED visits increase by more in GMC counties while mortality increases by slightly more in Two-Plan counties, though neither difference is statistically significant. It therefore appears that we do not have sufficient statistical precision to reliably disentangle the magnitude of the average effect of the two types of MMC, which is perhaps not surprising given that only 2 of the 14 reform counties adopted the GMC model.

VII. Conclusion

The results summarized above provide new evidence about the effects of requiring older and disabled Medicaid recipients to enroll in Medicaid managed care plans in more than a dozen large urban counties in America's most populous state. We find that the shift from traditional fee-for-service Medicaid to MMC led to a significant and sustained increase in visits to the emergency department. Our point estimate suggests an overall average increase of approximately 3.7 percent. But because a large fraction of Medicaid recipients in our sample was not directly affected by the mandate – either because they were already in MMC or received a waiver from the MMC mandate – the actual effect of those shifted into MMC from fee-for-service Medicaid because of the mandates was likely much larger. The increase in ED visits may have been driven by a deterioration in health among disabled Medicaid recipients, a reduction in their access to a regular source of health care, or a combination of the two.

We also find that the shift to MMC resulted in a significant increase in mortality among Medicaid recipients. Our estimates suggest an average effect of 7.3 percent from the base mortality rate, with the actual effect likely much larger since a large fraction of Medicaid recipients in our sample were unaffected by the mandate. We find that – for both ED visits and for mortality – the adverse effects were concentrated among those in worse baseline health prior to the introduction of the SPD mandates. This provides validation for the concerns of some policymakers and practitioners about exposing more

medically frail individuals to this form of privatizing government services. It also suggests greater caution may be warranted for other states considering moving their SPD populations into MMC plans.

Taken together, our findings demonstrate substantial heterogeneity in the effects of MMC and show that the sickest patients suffer adverse consequences from the requirement to enroll in MMC. While California is just one state and the effects clearly may vary elsewhere, the "Golden State's" Two-Plan and GMC models share many features with those that exist in many other states. This suggests California and possibly other states may want to exercise caution before further expanding the groups of Medicaid recipients who are required to enroll in MMC. At a minimum, it seems appropriate to harness the benefits of existing administrative data to rigorously evaluate the effects of large-scale reforms like the ones studied here given the substantial number of people affected, the large amounts of government funding at stake, and the paucity of existing evidence.

One limitation of our analysis is that we examine only the relatively short-term effects of the transition from Medicaid fee-for-service to MMC through 2013. We do this because of the very large increase in Medicaid enrollment that began in January 2014, which may have had a different effect across the reform and COHS counties in our sample. It is possible that the longer-term effects of MMC for disabled adults in California differed from the ones we estimate during the first two years. It is also possible that the transition to MMC reduced the strains on those administering the Medicaid program so that they could focus greater attention on the nearly 40 percent increase in program enrollment in 2014 and 2015.

Additionally, we only have hospital utilization data for inpatient admissions and ED visits and thus are unable to observe the effect of MMC on other outpatient visits, the utilization of prescription drugs, physician visits, and other health care services. While this is certainly a limitation of our data, an offsetting benefit of our data is the ability to follow individuals even after they exit the Medicaid program. More research to investigate the effects of MMC on other types of health care services would surely be useful given the reliance of so many state Medicaid programs on MMC.

One could argue that our estimates for the effects of MMC on the utilization of health care and health outcomes are "too late" to influence policy formation given that more than 85 percent of Medicaid recipients nationally are now enrolled in MMC plans and more than 60 percent of Medicaid spending is disbursed to these plans. However, there are multiple states where the Medicaid program does not rely appreciably on MMC, including Alabama, Connecticut, Maine, and Wyoming. Perhaps more importantly, other states might modify their Medicaid programs over the coming decade in response to proposed and/or enacted federal policy changes. These could include meaningful changes to the federal financing of state Medicaid programs along with the eligibility criteria for these programs.

It may therefore be an especially appropriate time for policymakers and the research community to investigate the benefits and costs of previous reforms to help Medicaid provide the maximum possible value for current and future program beneficiaries given the substantial costs of the program, which were $860 billion in the 2023 calendar year. Our results, in combination with a small set of other recent papers, demonstrate there is meaningful variation in the effects of MMC both across states and across different populations. More research is needed to understand the drivers and the implications of this variation as the Medicaid program approaches its sixty-year anniversary later this year.[39]

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[39] Medicaid and Medicare were created by federal legislation signed into law by President Lyndon Johnson on July 30, 1965.

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Tables and Figures

Table 1: Summary Statistics – Demographics

Time Period	Sample Selection 2009Q2–2010Q1	Pre-Implementation 2010Q2–2011Q1	Implementation 2011Q2–2012Q2	Post-Implementation 2012Q3–2013Q4
Panel A: Treatment/Reform Counties				
Number of individuals	156,740	156,740	153,349	150,106
Number of observations	626,960	621,596	760,204	891,508
Average age	49.19	50.17	51.24	52.52
Share female	0.63	0.63	0.63	0.64
Share black	0.22	0.22	0.22	0.22
Share hispanic	0.33	0.33	0.33	0.33
Panel B: Control/COHS Counties				
Number of individuals	26,119	26,119	25,606	25,111
Number of observations	104,476	103,637	126,985	149,333
Average age	48.84	49.82	50.89	52.19
Share female	0.61	0.61	0.62	0.62
Share black	0.10	0.10	0.10	0.10
Share hispanic	0.29	0.29	0.29	0.30

Notes: This table shows summary statistics of the analysis sample in four different time periods between April 1, 2009 and December 31, 2013. The analysis sample includes individuals with one or more hospital discharges and/or ER visits with Medicaid or Medicaid managed care as the primary expected payer during the 2009Q2 through 2010Q1 period. Panels A and B contain data for individuals residing in treatment and control counties, respectively, during the sample selection period. Demographic averages are calculated by taking the average over person-quarter level observations (the unit of analysis for regression specifications).

Table 2: Summary Statistics – Health Care Utilization

Time Period	Sample Selection 2009Q2–2010Q1	Pre-Implementation 2010Q2–2011Q1	Implementation 2011Q2–2012Q2	Post-Implementation 2012Q3–2013Q4
Panel A: Treatment/Reform Counties				
Utilization				
# of discharges/qtr	0.18	0.13	0.11	0.10
# of ED visits/qtr	0.52	0.37	0.35	0.34
Discharge payer shares				
MMC	0.23	0.20	0.35	0.52
MCDFFS	0.67	0.65	0.43	0.23
Medicare	0.00	0.05	0.10	0.13
Private	0.03	0.03	0.05	0.05
Self-pay	0.03	0.03	0.03	0.03
All other	0.04	0.04	0.04	0.05
ED payer shares				
Medicaid	0.83	0.78	0.71	0.65
Medicare	0.00	0.03	0.05	0.08
Private	0.05	0.06	0.10	0.12
Self-pay	0.09	0.09	0.09	0.08
All other	0.03	0.04	0.04	0.06
Panel B: Control/COHS Counties				
Utilization				
# of discharges/qtr	0.15	0.09	0.08	0.08
# of ED visits/qtr	0.58	0.40	0.37	0.34
Discharge payer shares				
MMC	0.58	0.56	0.54	0.52
MCDFFS	0.24	0.21	0.17	0.14
Medicare	0.00	0.05	0.12	0.18
Private	0.03	0.04	0.04	0.03
Self-pay	0.03	0.03	0.03	0.03
All other	0.11	0.10	0.10	0.09
ED payer shares				
Medicaid	0.81	0.71	0.67	0.63
Medicare	0.00	0.04	0.08	0.11
Private	0.07	0.09	0.11	0.10
Self-pay	0.08	0.11	0.09	0.09
All other	0.04	0.05	0.05	0.06

Notes: This table shows summary statistics of the analysis sample in four different time periods between April 1, 2009 and December 31, 2013. The analysis sample includes individuals with one or more hospital discharges and/or ER visits with Medicaid or Medicaid managed care as the primary expected payer during the 2009Q2 through 2010Q1 period. Panels A and B contain data for individuals residing in treatment and control counties, respectively, during the sample selection period. Utilization averages are calculated by taking the average over person-quarter level observations (the unit of analysis for regression specifications). Payer shares are computed using the total number of discharges and ER visits of different payers across all individuals in each of the four time periods.

Table 3: Summary Statistics – FFS versus MMC (Reform Counties)

	Payer in 2009Q2–2010Q1		
	FFS	MMC	Total
Individual-level characteristics			
Age	50.4	48.3	49.8
Share Female	0.52	0.70	0.58
Number of ED visits	2.1	1.7	2.0
Number of Hospital Discharges	2.0	1.5	1.8
Charlson Score	2.2	1.5	2.0
Elixhauser Score	4.0	3.0	3.7
Number of individuals	41,039	16,898	57,937
Discharge-level characteristics			
Length of stay	8.33	4.86	7.52
Charge	57,775	48,582	55,723
Number of diagnoses	4.8	4.3	4.7

Notes: Based on the universe of hospital discharges of individuals with year-of-birth from 1950 to 1969 (inclusive) and residing in reform counties from 2009Q2 to 2010Q1 with one or more Medicaid fee-for-service or Medicaid managed care discharges during this period. Among those with two or more discharges, individuals are included only if their primary expected payer is the same (e.g. excluded if one Medicaid FFS discharge and one MMC discharge). Panel A lists averages at the individual level while Panel B provides averages at the discharge level.

Table 4: The Effect of MMC Mandates on Hospital Inpatient Stays by Health Insurer

	(1) Any Payer	(2) Medicaid Managed Care	(3) Medicaid Fee for Service	(4) Private	(5) Medicare	(6) Self-Pay	(7) Other
Mandate	0.071	2.058***	−2.239***	0.215***	−0.016	−0.010	0.060*
	(0.124)	(0.122)	(0.138)	(0.032)	(0.038)	(0.018)	(0.031)
Time FE	Y	Y	Y	Y	Y	Y	Y
Individual FE	Y	Y	Y	Y	Y	Y	Y
Pre-Reform Mean (*y*)	11.666	2.794	6.951	0.379	0.666	0.340	0.533
Observations	1,946,193	1,946,193	1,946,193	1,946,193	1,946,193	1,946,193	1,946,193

Notes: Outcome variables for total number of discharges in the quarter (column 1) and number of discharges by each payer category in the quarter (columns 2 through 7) are multiplied by 100 for presentation purposes. Each specification includes 11 observations for everyone in the sample (unless a person dies before their 11th quarter), including the four immediately before the mandate quarter, the quarter of the mandate, and six quarters after the mandate. The coefficients are from regressions of each outcome variable on the MANDATE indicator, year-quarter FEs, and individual FEs, with standard errors clustered at the county-by-MQ (mandate quarter) level to yield 125 clusters (25 counties and 5 mandate quarters: 2011Q2 through 2012Q2).

Table 5: The Effect of MMC Mandates on Hospital Inpatient Stays by Type and Route of Admission

		Type of admission		Route of admission		
	Discharge	Scheduled	Unscheduled	Non-ER Transfer	Non-ER Non-transfer	Admitted from Own ER
Mandate	0.071	0.016	0.055	0.060	−0.036	0.047
	(0.124)	(0.032)	(0.114)	(0.039)	(0.047)	(0.100)
Time FE	Y	Y	Y	Y	Y	Y
Individual FE	Y	Y	Y	Y	Y	Y
Pre-Reform Mean (*y*)	11.666	1.215	10.448	0.491	2.551	8.624
Observations	1,946,193	1,946,193	1,946,193	1,946,193	1,946,193	1,946,193

Notes: Outcome variables for total number of discharges in the quarter (column 1) and type and route of admission in the quarter (columns 2 through 6) are multiplied by 100 for presentation purposes. Each specification includes 11 observations for everyone in the sample (unless a person dies before their 11th quarter), including the four immediately before the mandate quarter, the quarter of the mandate, and six quarters after the mandate. The coefficients are from regressions of each outcome variable on the MANDATE indicator, year-quarter FEs, and individual FEs, with standard errors clustered at the county-by-MQ (mandate quarter) level to yield 125 clusters (25 counties and 5 mandate quarters: 2011Q2 through 2012Q2).

Table 6: The Effect of MMC Mandates on Visits to the Emergency Room

	(1) Any ER Visit	(2) Medicaid	(3) Private	(4) Medicare	(5) Self-Pay	(6) Other
Mandate	1.372***	−0.345	1.488***	−0.235**	0.183	0.281***
	(0.358)	(0.348)	(0.172)	(0.091)	(0.128)	(0.091)
Time FE	Y	Y	Y	Y	Y	Y
Individual FE	Y	Y	Y	Y	Y	Y
Pre-Reform Mean (y)	37.020	28.282	2.576	1.241	3.524	1.397
Observations	1,946,193	1,946,193	1,946,193	1,946,193	1,946,193	1,946,193

Notes: Outcome variables for total number of emergency room visits in the quarter (column 1) and number of emergency room visits by each payer category in the quarter (columns 2 through 6) are multiplied by 100 for presentation purposes. Each specification includes 11 observations for everyone in the sample (unless a person dies before their 11th quarter), including the four immediately before the mandate quarter, the quarter of the mandate, and six quarters after the mandate. The coefficients are from regressions of each outcome variable on the MANDATE indicator, year-quarter FEs, and individual FEs, with standard errors clustered at the county-by-MQ (mandate quarter) level to yield 125 clusters (25 counties and 5 mandate quarters: 2011Q2 through 2012Q2).

Table 7: Heterogeneity in the Effects of MMC Mandates on ER Visits

	Full sample	by Baseline Charlson Score		by Baseline Elixhauser Score		by Two-Plan/GMC	
		Low	High	Low	High	Two-Plan	GMC
Effect of Mandate	1.372***	0.471	2.629***	0.554*	2.577***	1.299***	1.779**
	(0.358)	(0.357)	(0.654)	(0.290)	(0.652)	(0.364)	(0.730)
Difference		2.159***		2.023***		0.481	
		(0.718)		(0.634)		(0.708)	
Time FE	Y	Y		Y		Y	
Individual FE	Y	Y		Y		Y	
Pre-Reform Mean (y)	37.020	27.393	49.416	24.881	52.945	37.113	36.405
Observations	1,946,193	1,946,193		1,946,193		1,946,193	

Notes: Outcome variables for total number of emergency room visits in the quarter are multiplied by 100 for presentation purposes. Specifications 2 and 3 investigate variation in the MMC mandate effect by baseline Charlson score, and specifications 4 and 5 explore variation by baseline Elixhauser score. Specifications 2 to 5 are fully interacted with the health score, whereas specifications 6 and 7 only introduce two separate MANDATE variables – one for two-plan counties and another for GMC counties. Each specification includes 11 observations for everyone in the sample (unless a person dies before their 11th quarter), including the four immediately before the mandate quarter, the quarter of the mandate, and six quarters after the mandate. The coefficients are from regressions of each outcome variable on the MANDATE indicator, year-quarter FEs, and individual FEs, interacted as described above. "Difference" is the difference between high/low health score or two-plan/GMC. Standard errors clustered at the county-by-MQ (mandate quarter) level to yield 125 clusters (25 counties and 5 mandate quarters: 2011Q2 through 2012Q2).

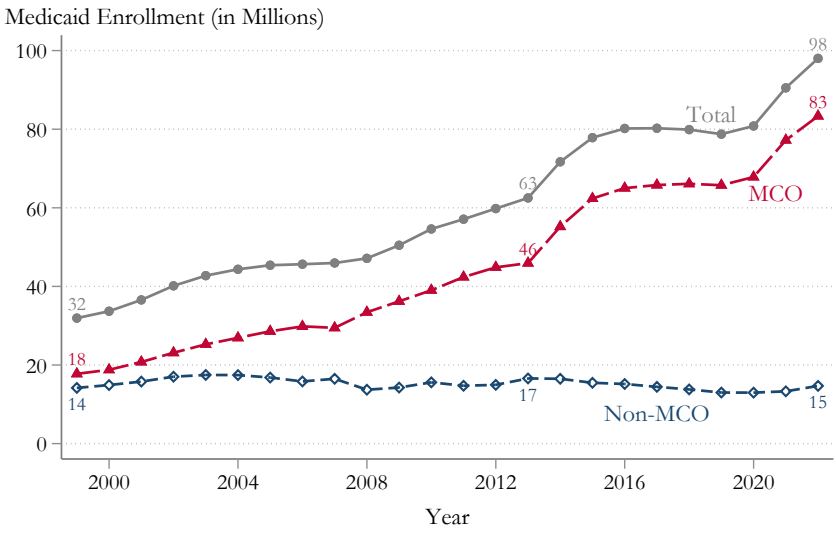
Table 8: The Effect of MMC Mandates on Mortality

	Full sample	by Baseline Charlson Score		by Baseline Elixhauser Score		by Two-Plan/GMC	
		Low	High	Low	High	Two-Plan	GMC
Effect of Mandate	0.0593**	0.0133	0.1051**	0.0077	0.1014**	0.0599**	0.0556
	(0.0265)	(0.0220)	(0.0470)	(0.0268)	(0.0417)	(0.0286)	(0.0378)
Difference		0.0918*		0.0936*		−0.0043	
		(0.0553)		(0.0512)		(0.0400)	
Time FE	Y	Y		Y		Y	
Pre-Reform Mean (y)	0.8104	0.2587	1.2210	0.2651	1.1684	0.8054	0.8438
Observations	1,186,949	1,186,949		1,186,949		1,186,949	

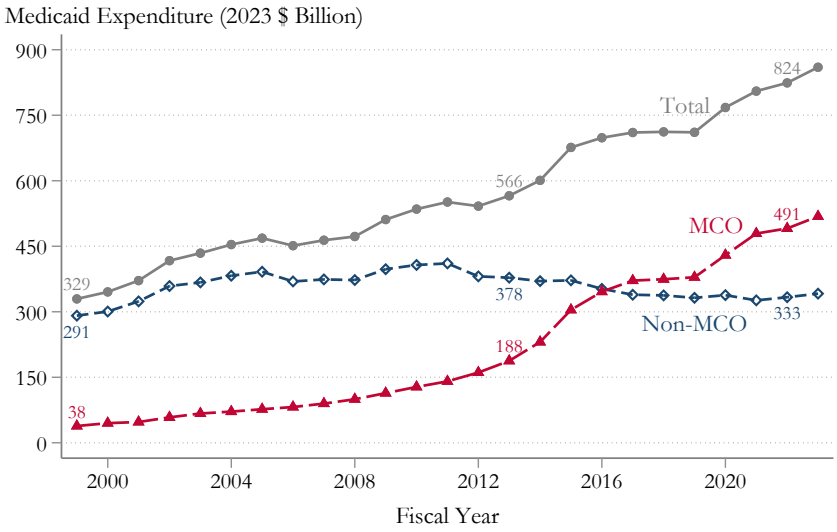
Notes: Outcome variable is equal to 100 if an individual dies in the quarter and is otherwise equal to zero. Specifications 2 and 3 investigate variation in the MMC mandate effect by baseline Charlson score, and specifications 4 and 5 explore variation by baseline Elixhauser score. Specifications 2 to 5 are fully interacted with the health score, whereas specifications 6 and 7 only introduce two separate MANDATE variables – one for two-plan counties and another for GMC counties. Each specification includes 11 observations for everyone in the sample (unless a person dies before their 11th quarter), including the four immediately before the mandate quarter, the quarter of the mandate, and six quarters after the mandate. The coefficients are from regressions of each outcome variable on the MANDATE indicator, year-quarter FEs, county FEs, age FEs, and gender – interacted as described above. "Difference" is the difference between high/low health score or two-plan/GMC. Standard errors are clustered at the county-by-MQ (mandate quarter) level to yield 125 clusters (25 counties and 5 mandate quarters: 2011Q2 through 2012Q2).

Figure 1: Medicaid Manged Care Enrollment and Expenditure in the U.S.

(a) Medicaid Enrollment

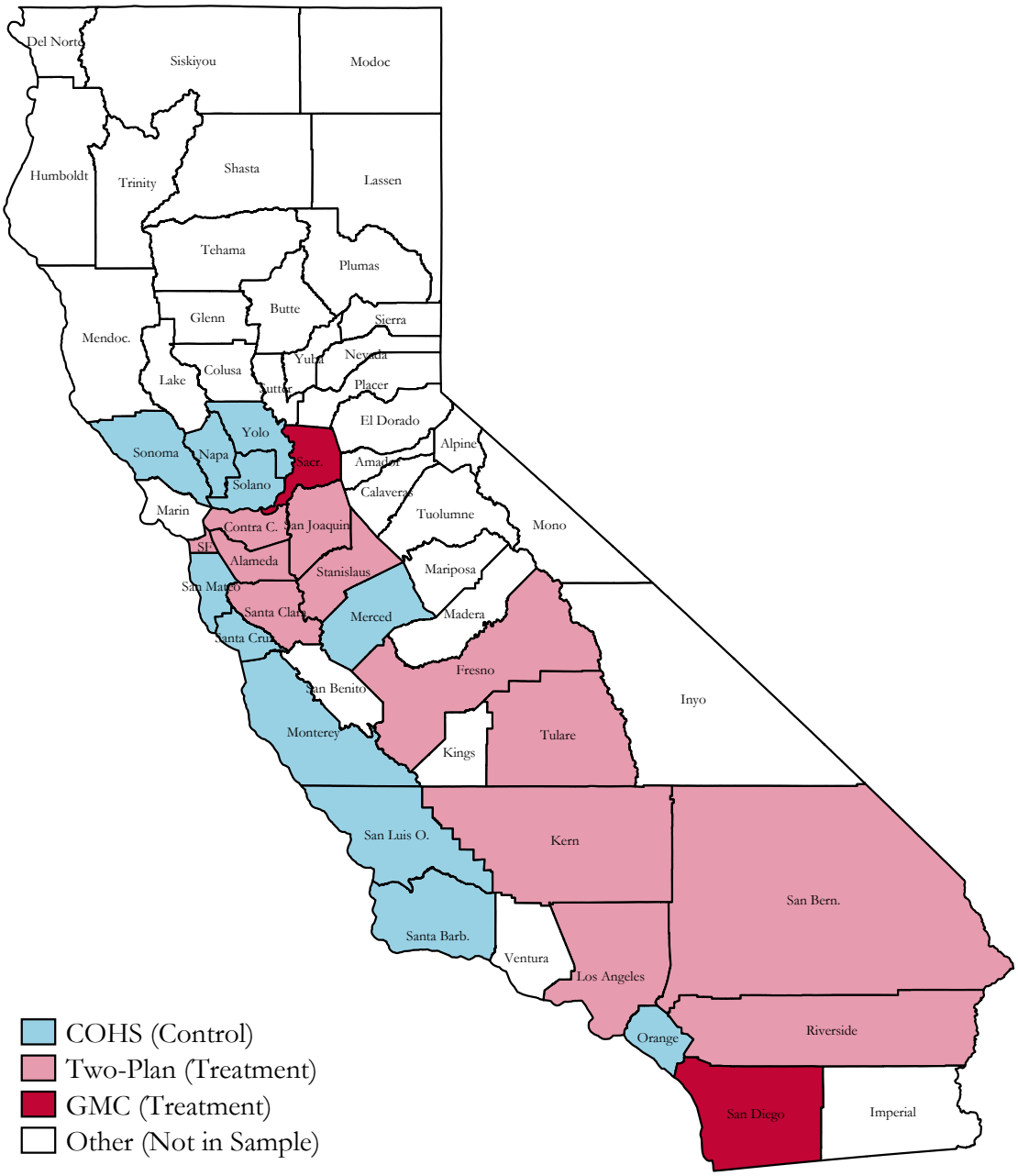


(b) Medicaid Expenditure



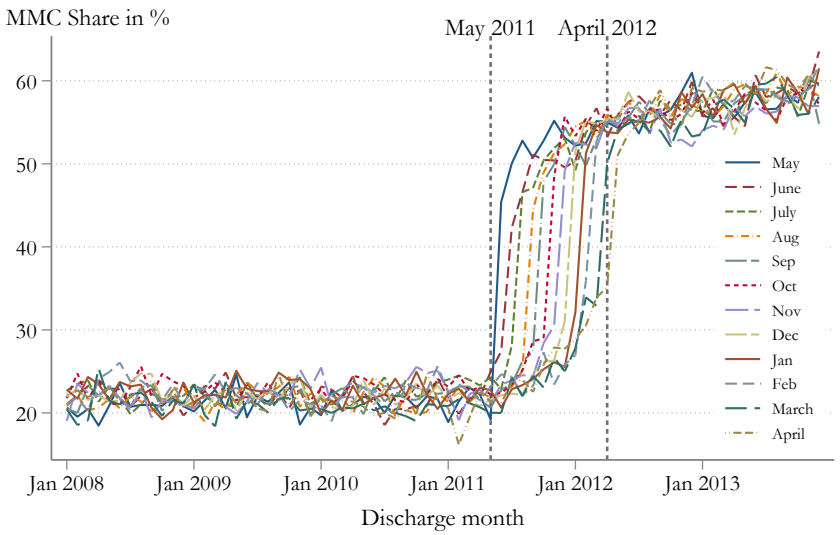
Notes: Panel (a) shows Medicaid enrollment in the U.S. (in millions of enrollees) from 1999 to 2022. The grey solid line is total enrollment, the red dashed line is enrollment in MCO plans, and the blue short-dashed line is enrollment in Non-MCO plans. Panel (b) shows Medicaid expenditure in the U.S. (in 2023 USD billions) from 1999 to 2023. The grey solid line is total expenditure, the red dashed line and blue short-dashed lines are MCO expenditures and Non-MCO expenditures. See Medicaid.gov, Expenditure Reports from MBES/CBES, available at https://www.medicaid.gov/medicaid/financial-management/state-expenditure-reporting-for-medicaid-chip/expenditure-reports-mbescbes. See also, Mathematica Policy Research, Medicaid Managed Care Enrollment and Program Characteristics, various years. See also, Medicaid.gov, Medicaid Managed Care Enrollment Report, various years. See also, KFF, Total Medicaid Managed Care Enrollment, various years. See also, Duggan & Hayford 2013.

Figure 2: Map of MMC Models in California (as of January 2010)



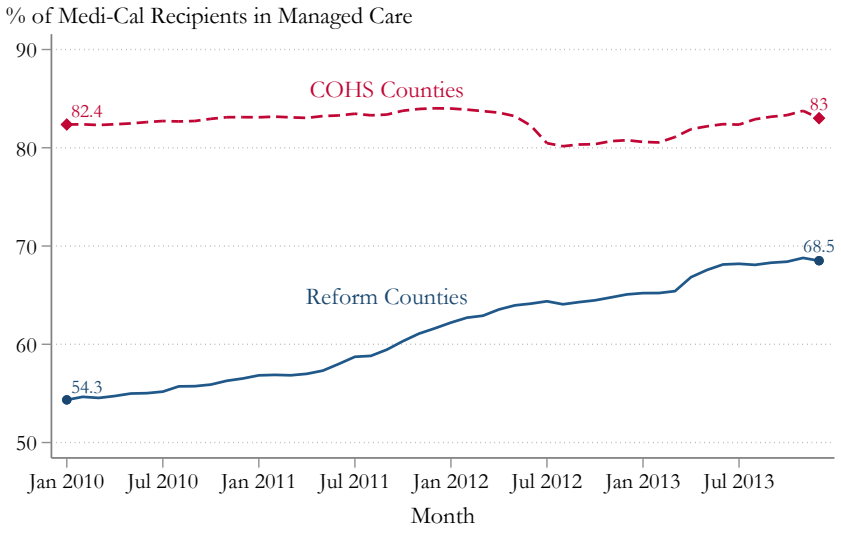
Notes: This county-level map of California highlights the counties that were using the COHS, the Two-plan, and the GMC model in January 2010. This corresponds to the treatment and control group counties in our analysis sample. COHS (control): Sonoma, Napa, Yolo, Solano, San Mateo, Santa Cruz, Merced, Monterey, San Luis Obispo, Santa Barbara. GMC (treatment): Sacramento, San Diego. Two-plan (treatment): Contra Costa, San Francisco, San Joaquin, Alameda, Stanislaus, Santa Clara, Fresno, Tulare, Kern, Los Angeles, San Bernardino, Riverside. Countries that are not colored are not part of our main analysis sample since only a very small share of people in these counties was in an MMC plan in January 2010.

Figure 3: MMC Enrollment by Birth Month



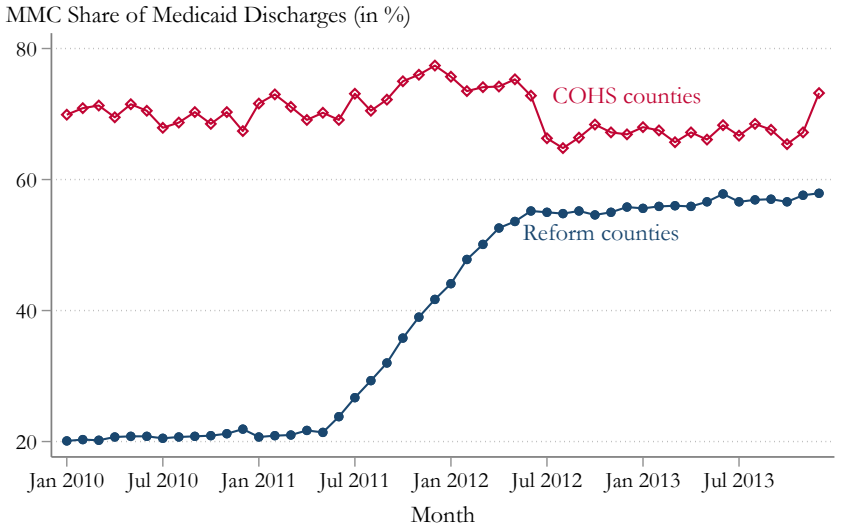
Notes: Figure plots MMC discharges as share of Medicaid discharges for each month from Jamuary 2008 to December 2013, by month of birth of the patient. Discharges are based on universe of hospital discharges in California from that are not birth or pregnancy-related for individuals born between 1950–1969 (inclusive) who live in reform counties. Policy implementation period is from 2011q2 to 2012q2 (indicated by vertical dashed lines).

Figure 4: MMC Enrollment in COHS and Reform Counties



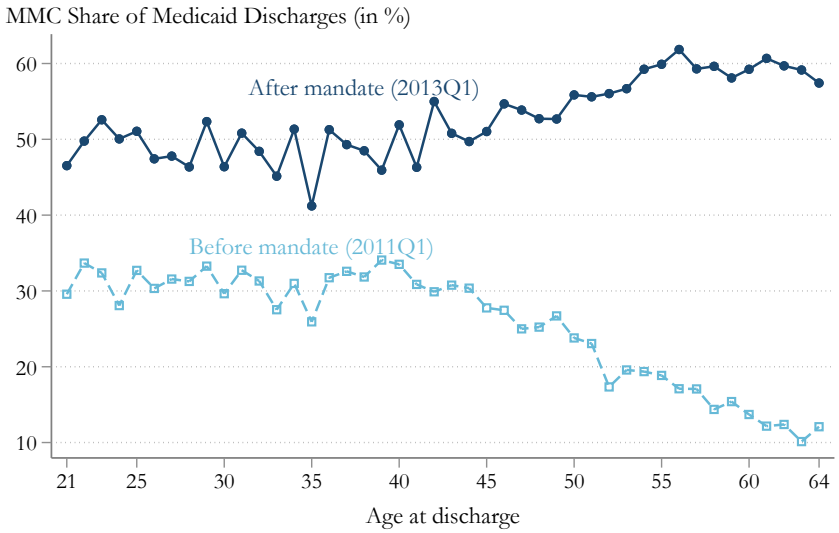
Notes: This figure shows the overall percentage of Medi-Cal recipients in managed care in the reform and COHS control counties between January 2010 and December 2013. See California Department of Health Care Services, Medi-Cal Certified Eligibles Data by Month with Demographics: By Delivery System and Plan, Certified Eligibles, available at https://data.chhs.ca.gov/dataset/medi-cal-certified-eligibles-with-demographics-by-month/resource/2c28bf78-a385-4d0c-88d5-7d1eef09a5ab.

Figure 5: MMC Share of Medi-Cal Discharges around the Mandate (Ages 40–64)



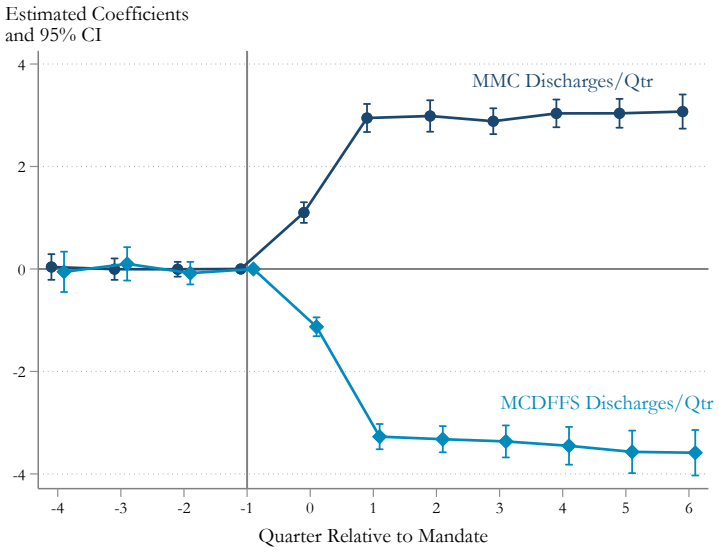
Notes: Figure shows the share of Medicaid hospital discharges in COHS and reform counties in California among people ages 40–64 for discharges between January 2010 and December 2013.

Figure 6: MMC Share of Discharges Before and After the Reform, by Age (Reform Counties)



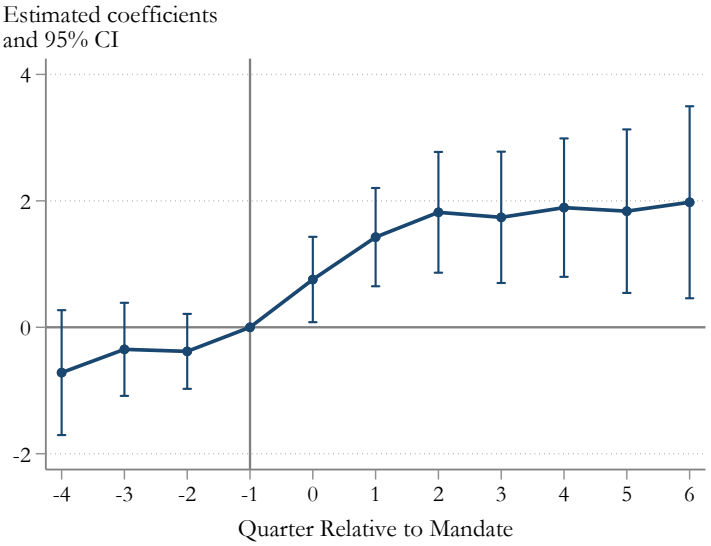
Notes: Figure shows the share of Medicaid hospital discharges by age at discharge (x-axis) that is on MMC. Light-blue solid line depicts data from 2011Q1 (before the reform), dark-blue solid line depicts data from 2013Q1 (after the reform). Only includes discharges for individuals residing in reform counties and excludes birth and pregnancy-related discharges. Figure A.3 shows the same figure but for discharges of people residing in COHS counties.

Figure 7: Effect of the Mandate on Number of Medi-Cal FFS and MMC Discharges



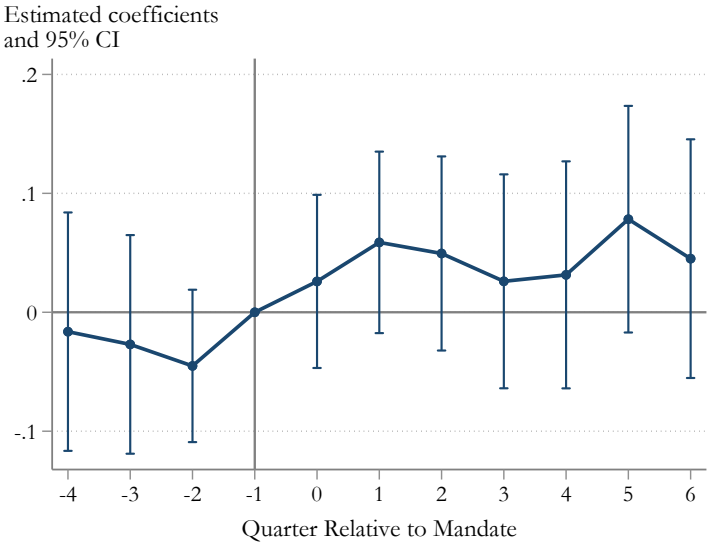
Notes: Figure shows point estimates and 95% confidence intervals from running an event study specification, regressing the number of MMC visits (dark-blue estimates) or FFS Medicaid visits (light-blue on relative time*mandate indicators, controlling for year-quarter and individual fixed effects. Outcome variables are multiplied by 100 for presentation purposes. Standard errors are clustered at the county-by-MQ (mandate quarter) level to yield 125 clusters (25 counties and 5 mandate quarters: 2011Q2 through 2012Q2).

Figure 8: Effect of the Mandate on Number of ER Visits



Notes: Figure shows point estimates and 95% confidence intervals from running an event study specification, regressing the number of ER visits on relative time*mandate indicators, controlling for year-quarter and individual fixed effects. Outcome variables are multiplied by 100 for presentation purposes. Standard errors are clustered at the county-by-MQ (mandate quarter) level to yield 125 clusters (25 counties and 5 mandate quarters: 2011Q2 through 2012Q2).

Figure 9: Effect of the Mandate on Mortality



Notes: Outcome variable is an indicator equal to 100 if an individual dies in the quarter and is otherwise equal to zero. The coefficients are from regression of the outcome variable on relative time*mandate indicators and age, year-quarter, gender, and county fixed effects. Standard errors are clustered at the county-by-MQ (mandate quarter) level to yield 125 clusters (25 counties and 5 mandate quarters: 2011Q2 through 2012Q2).

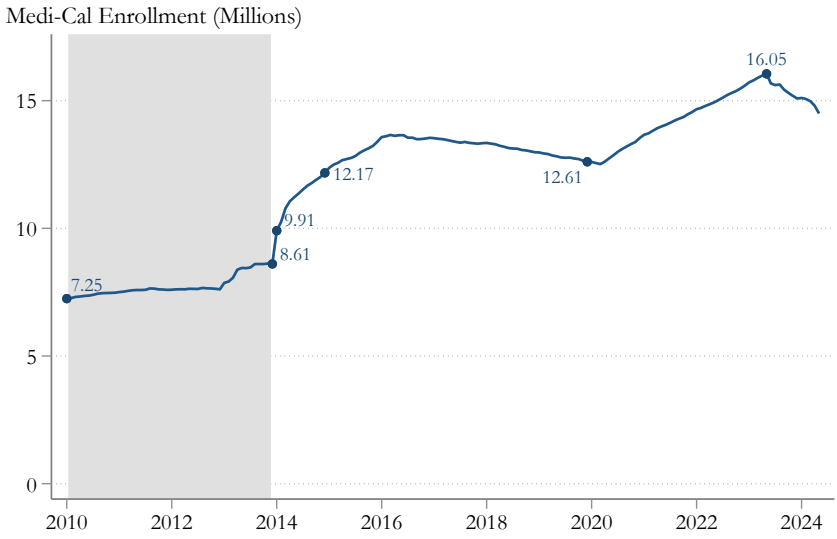
Appendix

Table A.1: Effect of the Mandate on Number of ER Visits, by Type

	(1) ED needed & not preventable	(2) ED needed & preventable	(3) Emergent & PC-treatable	(4) Non-Emergent	(5) Alcohol	(6) Drug	(7) Injury	(8) Psych	(9) Unclassified
Mandate	0.257***	0.119**	0.246**	0.512***	−0.028	−0.003	0.016	0.100*	0.153*
	(0.058)	(0.049)	(0.104)	(0.123)	(0.041)	(0.017)	(0.079)	(0.052)	(0.084)
Time FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Individual FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Pre-Reform Mean (y)	4.461	2.864	8.316	8.519	0.815	0.214	5.010	1.427	5.392
Observations	1,946,193	1,946,193	1,946,193	1,946,193	1,946,193	1,946,193	1,946,193	1,946,193	1,946,193

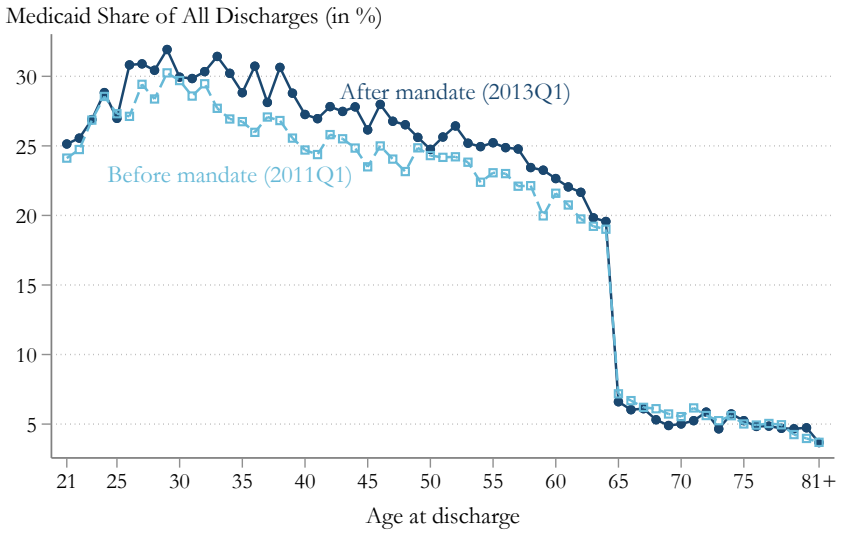
Notes: Table shows regression results from nine TWFE regressions where the outcome is number of ER visits of a certain type (as specified in the header). We classify the types of ED visits according to the ICD-9 categories developed by the NYU Center for Health and Public Service Research that is available at: `https://wagner.nyu.edu/faculty/billings/nyued-background`. Each specification includes 11 observations for everyone in the sample (unless a person dies before their 11th quarter), including the four immediately before the mandate quarter, the quarter of the mandate, and six quarters after the mandate. The coefficients are from regressions of each outcome variable on the MANDATE indicator, year-quarter FEs, and individual FEs, with standard errors clustered at the county-by-MQ (mandate quarter) level to yield 125 clusters (25 counties and 5 mandate quarters: 2011Q2 through 2012Q2).

Figure A.1: Medicaid Enrollment in CA from 2010 to 2024



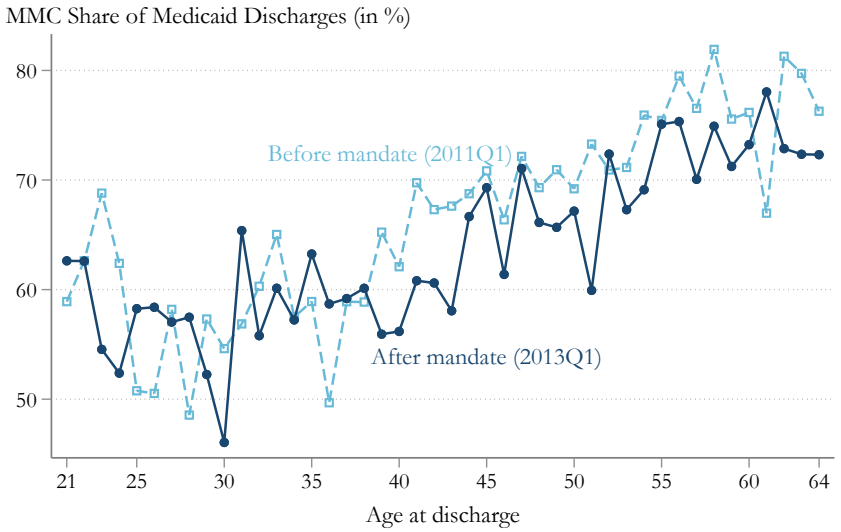
Notes: This graph shows monthly Medi-Cal enrollment in California from January 2010 through May 2024. See California Department of Health Care Services, Medi-Cal Certified Eligibles Data by Month with Demographics: By Delivery System and Plan, Certified Eligibles, available at https://data.chhs.ca.gov/dataset/medi-cal-certified-eligibles-with-demographics-by-month/resource/2c28bf78-a385-4d0c-88d5-7d1eef09a5ab.

Figure A.2: Medicaid Share of Discharges, By Age (Reform and COHS Counties)



Notes: Figure shows the share of all hospital discharges by age at discharge (x-axis) that is on Medicaid. Light-blue dashed line depicts data from 2011Q1 (before the reform), dark-blue solid line depicts data from 2013Q1 (after the reform). Only includes discharges for individuals residing in COHS counties and excludes birth and pregnancy-related discharges. Figure 6 shows the same figure but for discharges of people residing in COHS counties.

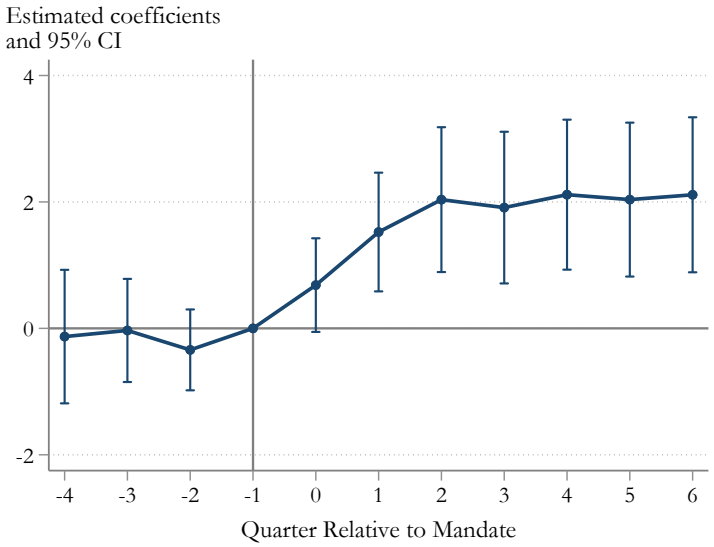
Figure A.3: MMC Share of Discharges Before and After the Reform, by Age (COHS Counties)



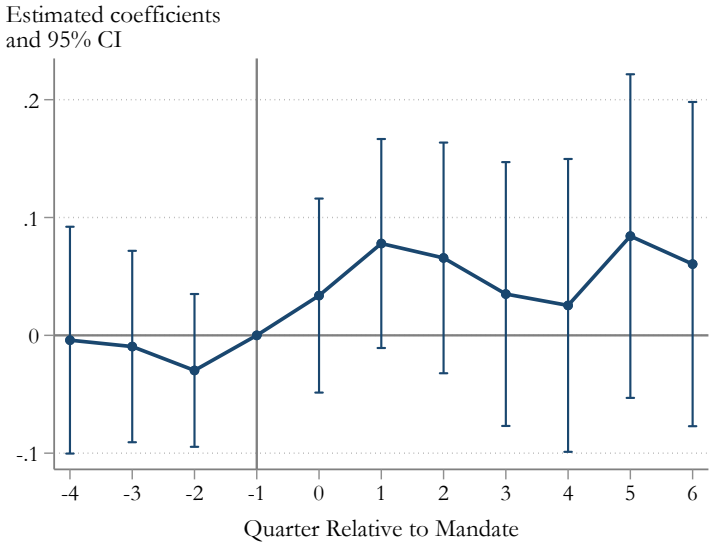
Notes: Figure shows the share of Medicaid hospital discharges by age at discharge (x-axis) that is on MMC. Light-blue dashed line depicts data from 2011Q1 (before the reform), dark-blue solid line depicts data from 2013Q1 (after the reform). Includes discharges for individuals residing in reform or COHS counties and excludes birth and pregnancy-related discharges.

Figure A.4: Stacked DiD Event Studies

(a) ER Visits



(b) Mortality



Notes: Based on stacked analysis sample, constructed by following Wing, Freedman, and Hollingsworth (2024). Includes COHS observations as never-treated control observations. In panel (a), the outcome variable is ED visits per 100. The coefficients are from weighted regressions of outcome variables on treatment*relative time indicators as well as year-quarter FEs, relative quarter FEs and individual FEs. In panel (b), the outcome variable is an indicator equal to 100 if an individual dies in that quarter, and 0 otherwise. The coefficients are from weighted regressions of outcome variables on treatment*relative time indicators as well as fixed effects for calendar year-quarter, relative quarter, age, gender, and county of residence. In both panels, the regressions are weighted using a stack weight of 1 for observations in the treatment group. The stack weight for control observations is calculated as $\frac{N_a^T/N^T}{N_a^C/N^C}$ where N_a^T (N_a^C) is the number of people that are in the treatment (control) group in that stack (the stack that adopts treatment in quarter a) and N^T (N^C) is the total number of treatment (control) units in the sample (which contains all stacks). Vertical lines illustrate 95% confidence intervals and are based on standard errors clustered at the county-by-MQ (mandate quarter) level to yield 125 clusters (25 counties and 5 mandate quarters: 2011Q2 through 2012Q2).