HETEROGENEITY IN THE IMPACT OF PRIVATIZING SOCIAL HEALTH INSURANCE *

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Abstract

State governments face the classic "make or buy" decision for the provision of Medicaid services. Over the past two decades, the majority of states have outsourced the provision of social insurance through Medicaid Managed Care (MMC) programs. These programs have been extensively studied in the literature – with little evidence of large positive or negative effects. However, most states allowed older and sicker enrollees to remain enrolled in the government run fee for service (FFS) programs. It is possible that these more fragile enrollees could have a different experience in managed care. In this paper we study California's mandatory enrollment of the senior and persons with disabilities (SPD) population in MMC. We find this mandatory enrollment caused an increased use of the emergency department and transfers between hospitals. This was not simply a hassle cost for enrollees – we also estimate an increase in mortality for the affected population. These effects were strongest for enrollees who had the greatest use of medical services prior to enrollment in MMC – the types of enrollees that might be expected to have a different experience with managed care. Our results suggest the adverse impact of MMC varies by the health of enrollees, which should inform the optimal outsourcing decision for governments.

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Since its creation in the 1960s, Medicaid has grown from a small program covering roughly 20 million indigent and disabled individuals to its current size of nearly 73 million enrollees (CMS, 2019).[1] This makes Medicaid the largest health insurer in the United States and one of the largest in the world. Unlike other social insurance programs such as Social Security and Medicare, Medicaid is administered and partially funded by state governments and as a result the program varies greatly across states.

One dimension of variation is the degree of privatization. As Medicaid expanded, each state debated its own proverbial "make or buy" decision, i.e. is it optimal for the government to directly provide insurance services or should they instead outsource these economic activities to private firms.[2] Over time, states have increasingly chosen outsourcing through the creation and expansion of Medicaid managed care (MMC) programs. Under MMC, state governments contract with (often for-profit) managed care firms. Payments are made on a "capitated" basis, with private firms receiving a lump sum payment and assuming responsibility for all beneficiary medical spending. Firms retain any difference as profits (or suffer losses if spending exceeds the capitated payment).[3]

MMC's popularity can be seen in Figure 1a which shows that by 2016 the share of Medicaid enrollees covered by these programs stood at nearly 80 percent. These initial MMC programs have been extensively studied, with a resulting literature providing little evidence of either great benefits or great harms from privatization (e.g. Aizer, Currie and Moretti 2007; Currie and Fahr 2005; Duggan 2004; Lee 2020; Kuziemko, Meckel and Rossin-Slater 2018).

Despite the growth of MMC over time, a sizeable share of beneficiaries remained in the traditional government run fee-for-service (FFS) system. These FFS enrollees were primarily sicker

[1] This expansion happened in several waves, starting with the inclusion of more pregnant women in the late-80s, children from higher income families in the mid-90s, and culminating with the passage of the Affordable Care Act (ACA) in 2010. Along with covering more people, the Affordable Care Act (ACA) changed the program from one of categorical eligibility to an entitlement available to all low-income individuals residing in states adopting the expansion.

[2] There is a similar debate in Medicare regarding the creation and operation of the Medicare Advantage program – which is a voluntary managed care program for the elderly.

[3] In most cases these payments are risk adjusted so firms get larger payments for insuring individuals who have medical conditions that are expected to lead to higher spending.

and/or older individuals that policymakers exempted from mandatory MMC enrollment. Though small in number, the complexity involved in treating these patients makes them exceptionally costly. Figure 1b shows that despite the massive MMC enrollment growth, the majority of Medicaid spending remained in the FFS system.

Searching for potential savings on these expensive patients, and perhaps comforted by the lack of meaningful harms from the initial MMC programs covering healthier and younger patients, many states are expanding privatization to include disabled and elderly Medicaid patients. Such policy decisions, however, may be premature. If the effect of privatization systematically varies across patients based on their health status, then the existing literature likely sheds little light on the optimality of these decisions.

In this paper, we directly examine heterogeneity in the effect of privatizing social health insurance. Specifically, we examine California's decision to mandate MMC enrollment for the 240,000 seniors and persons with disabilities (SPD) that had not already voluntarily enrolled in such programs.[4] We identify the causal effect of mandatory MMC enrollment by exploiting both variation in when beneficiaries were required to enroll in MMC and geographic variation in MMC exposure.

The optimality of privatizing government services is the subject of a rich theoretical economics literature that extends well beyond healthcare. The goal of privatization is to leverage the fact that as the residual claimant on spending, private firms have a greater incentive than government employees to control costs. Of course, such cost reduction efforts could have negative consequences. Of particular relevance to this paper, Hart, Shleifer and Vishny (1997) demonstrate that in a world of

[4] While California had previously mandated SPDs to enroll in MMC in 14 of the 30 counties where such programs were available, this policy reform expanded that program to the remaining 16 counties. 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 Medicaid Managed Care in a selection of counties where SPDs previously could voluntarily choose whether or not to enroll in MMC.

incomplete contracts, this potential for negative consequences makes the optimal solution unclear.[9] The authors predict that while outsourcing is optimal in many settings, states will prefer government control of services where there is a greater concern about adverse consequences resulting from decreased quality. Therefore, our question of heterogeneity in the consequences of privatization is central to economics of the government's optimal policy decision.

Consider our setting of privatizing social health insurance. Ideally, the cost reductions created by private firms would 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 covered by the contract with the state. This could occur for several reasons. First, capitated payments may create inappropriately strong incentives to cut costs in a setting where firms are not forced to internalize the full impact of lower quality – perhaps because such cost cutting is unobservable. Compounding this concern, firms may find it profitable to provide low quality services to enrollees who are expected to generate higher costs than the capitated payment.[10] 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, 2016).

Regardless of the mechanism, it is conceivable that the impact of the lower quality caused by cost reduction efforts might be more consequential for high acuity patients who could suffer meaningful harms (up to and including death) from even minor disruptions in access to care. Hart, Shleifer and Vishny (1997) suggests it is precisely such concerns about adverse consequences that drove states to continue to directly provide insurance services to their older and sicker beneficiaries.

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[9] In a complete contracting environment, the state is indifferent between government control and private ownership. After all, it can use these complete contracts to motivate its employees or private firms.

[10] 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). [https://www.aeaweb.org/articles?id=10.1257/pol.20170014]

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. First, privatization should cause greater harms than in the existing literature, which primarily focuses on programs covering largely healthier populations. Second, privatization should differentially impact enrollees *within* our sample based on actual underlying patient health — with sicker enrollees suffering greater harms. Our results are consistent with both predictions.

We first find a meaningful negative effect of MMC on these older and sicker enrollees, effects that were largely absent from the original MMC programs. Requiring California's SPD beneficiaries to enroll in MMC caused an 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 decrease in the underlying health of patients. Examining the conditions that led to increased ED visits, we find evidence that suggests they result (at least in part) from a lack of access to other medical services. For example, we find consistent evidence of an increase in preventable ED visits, non-emergent ED visits, and visits for psychiatric care – visit categories likely to be impacted by more restrictive non-hospital provider networks.

We also observe an increase in the number of inpatient visits that began as transfers from another hospital. This would be consistent with the implementation of stricter provider networks leading to a dislocation in the usual source of hospital care. Such disruption was a common concern expressed by patients and their advocates prior to the reform (Harbage & King, 2012). Further supporting that these transfers are likely the result of differing networks, we find that most of these transfers began as visits to the ED of the transferring hospital.

Consistent with concerns about adverse consequences for sicker patients, we find these changes in patterns of care represent more than just a hassle cost for enrollees. Indeed, we estimate that mandatory MMC enrollment caused a 12 percent increase in mortality for the affected population.

This increase begins immediately after a beneficiary's birth month required them to enroll in MMC. Supporting that a lack of access to care is a potential mechanism driving the immediate mortality increase, we find the most consistent evidence of such an effect for patients suffering from circulatory conditions. These medical conditions, which include hypertension and coronary artery disease, are a plausible source of an immediate mortality effect from a decrease in access to medical services. In contrast, we find no increase in mortality among those with conditions that should be less likely to result in such *immediate* dire consequences from a temporary dislocation in care (such as cancer patients).

We further 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 whether a patient had an above median use of inpatient and ED hospital services and on the nature of the patient's underlying health conditions.[11] Across both of these measures, we find the estimated increase in ED visits, transfers, and mortality was concentrated among sicker SPD beneficiaries, precisely the beneficiaries policymakers were likely concerned would suffer greater adverse consequences from MMC.

Our results contribute to a better understanding of the optimal privatization system for social insurance. It joins work by Layton et al. (2019) which examines similar expansions to older and disabled populations in Texas and New York. These authors find the expansion either had relatively little effect (New York) or generally increased access to services (Texas). They find no evidence of negative health effects in either state.

The divergence in the results across these two studies reflects the economically meaningful variation in the effects of Medicaid policies across states. Such variation makes it difficult to broadly

[11] Both of these 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.

generalize from the estimated effects of a particular policy choice (Garthwaite et al. 2019). Instead, policymakers and economists interested in making such comparisons must consider the particular economic factors that may be similar across contexts.

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. 2019; Goldin, Lurie and McCubbin 2019). 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 (2020) 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 since that time has provided health insurance for an increasing number of low-income Americans in the years since. By 2018, the monthly enrollment for Medicaid was 73 million individuals and the annual total spending was $597 billion (CMS, 2019).

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 payment schemes. Meanwhile, even within each state, Medicaid covers a population with a wide array of health conditions and care

intensity, resulting in a highly skewed cost distribution.[12] This results in marked heterogeneity in Medicaid's operations both across and within states.

I.A. Medicaid Managed Care

Increasingly confronted with the challenge of rising and unpredictable costs 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. Prior to the implementation of the ACA in 2014, the share of Medicaid recipients enrolled in MMCs rose from 10.6 percent in 1991 to 55.6 percent in 1999 and 73.5 percent in 2013. Accordingly, payment to MMC plans as a share of total Medicaid expenditures also rose steadily from 11 percent in 1999 to 30.1 percent in 2013 (Duggan and Hayford 2013; CMS 1999; CMS 2014; CMS 2015). Following the expansion of Medicaid under the Affordable Care Act, the scope of managed care grew as these programs covered both the majority of newly-eligible Medicaid recipients as well as wider groups of existing Medicaid enrollees with more complicated needs and health conditions. As can be seen in Figures 1a and 1b, by 2016 approximately 80 percent of Medicaid beneficiaries were enrolled in managed care, and payments to managed care plans accounted for 45 percent of total Medicaid expenditures.

MMC changes the economic incentives of the social insurance program. Instead of directly reimbursing providers for all expenses generated by Medicaid recipients, under MMC states contract with insurance plans and pay a fixed (usually risk-adjusted) capitation amount for each Medicaid enrollee under the managed care scheme. The various MMC plans then manage the purchase of healthcare goods and services for their enrollees and bear the marginal cost of each service. The goal

[12] For example, in fiscal year 2014 the average spending per recipient (including both full and partial benefit) was $13,063 for seniors and $16,859 for people with disabilities, but only $2,577 for children and $3,278 for adults. In other words, even though seniors (~7.4 million enrollees) and people with disabilities (~11.1 million enrollees) account for around 23% of total Medicaid recipients, they cost roughly 61% of total expenditure due to the complexity and severity of their health conditions (Kaiser Family Foundation 2014a; Kaiser Family Foundation 2014b).

is to give private payers stronger incentives to eliminate unnecessary services, increase efficiency, and improve the coordination of care among different providers within the same network. However, the impacts of managed care are still largely unclear on the fronts of both access and quality improvement and actual cost saving.

From the perspective of care quality and coordination, MMC is inevitably subject to the problem of incomplete contracting that affects many other government procurement programs. The more complicated the health condition of enrollees, the more challenging it is for the government to write a complete contract with the managed care organizations. Having a government agency administer these programs directly does not solve the incomplete contracting problem for complex patients. It does, however, decrease the incentives to cut costs in ways that could cause adverse consequences for the sickest patients. This could tilt the optimal decision in favor of the government run program for populations where there are greater concerns about adverse health consequences (Hart, Schleifer, Vishny, 1997).

I.B. Existing Evidence on Medicaid Managed Care

The initial MMC programs requiring enrollment only for healthier populations has produced a substantial literature examining a variety of outcomes. Some studies find no significant impacts of MMC on the health outcomes of newborns (Duggan 2004; Lee 2020), while others have found evidence for both worse health outcomes likely due to reduced quality of prenatal care (Aizer, Currie, & Moretti, 2007) and risk selection arguably through differential quality of services (Currie and Fahr 2005; Kuziemko, Meckel and Rossin-Slater 2018).

Given that most states did not require seniors or disabled individuals to enroll in MMC, these initial studies are unable to examine whether these programs have different effects for other patient populations. That said, there have been many papers examining the impact of Medicare Advantage

(MA) plans on care utilization and health outcomes.[13] MA is the voluntary managed care program for Medicare, which by definition covers seniors. This may lead some to believe that estimates from the MA literature can provide insight into the likely effect of requiring MMC enrollment for older beneficiaries. Before extrapolating from the MA literature, however, it is important to recognize the substantial differences between these markets in terms of the competitive structure and underlying regulations. At a minimum, MA is a *voluntary* program, where both the decision to take part in managed care and the selection of the plan is made by the individual. There is existing evidence that those signing up for coverage are healthier on average than those that remain in the traditional FFS Medicare program (Geruso & Layton, 2020).

In many ways, this makes MA more similar to the initial MMC programs covering younger and healthier enrollees. Therefore, our paper 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 & Garthwaite, 2019). Such policies would force all Medicare enrollees into managed care without clear evidence of the likely impact on the sickest beneficiaries.

II. California's Medicaid Managed Care Reform

Created in 1966, the Medicaid program in California (aka Medi-Cal) provides health insurance coverage to low-income families, children, pregnant women, and persons with disabilities. By 2016, after decades of reforms and expansions, Medi-Cal had become the state's largest health care

[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.

purchaser and offered health insurance for more than 13 million individuals (roughly 1 out of every 3 Californians), costing more than $83 billion in total government expenditures (CMS 2018a; CMS 2018b).

Currently, all 58 counties in California offer MMC plans and all but one (San Benito County) has mandated either certain groups or all Medicaid beneficiaries enroll in these plans. The types of MMC plans adopted and characteristics of the mandates differ substantially across different counties in California and over time. By July 2016, there were 6 different models of MMC plans covering more than 10.6 million individuals in California, roughly 77 percent of all Medicaid beneficiaries and 50 percent of all Medicaid expenditures (CMS 2018a; CMS 2018b).

MMC was first mandated in California in 1983 with the introduction of a County Organized Health System (COHS) in Santa Barbara County. By 2012, 14 counties had adopted COHS Medicaid managed care plans where 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 COHS counties there is no competition, i.e. enrollees have only one plan choice.[14] In the additional 16 counties that serve as the focus of our study, competition among MMC plans existed and the SPD population was not previously mandated to enroll in these plans.[15] As of 2012, an estimated 140,000 SPD beneficiaries in these counties had voluntarily opted into these MMC plans (Harbage & King, 2012).

In 14 of these 16 counties with competition, the market consisted of only two plans: (1) a "local initiative" plan that was organized by the county and focused on maintaining access to safety net providers and (2) a commercial plan. While the local initiative plan was organized by the county,

[14] In 2013, MMC was expanded into rural counties where 8 counties adopted COHS model, 18 counties adopted regional model, and Imperial and San Benito counties each adopted their own model. In the regional and Imperial model, the government contracts with two commercial plans and mandates MMC enrollment for a subset of Medicaid beneficiaries. In the San Benito model, MMC enrollment is voluntary for all individuals.

[15] Our sample is composed of counties that implemented the Bridge to Reform mandate and had no other concurrent MMC-related reforms during our analysis period.

in some of the counties these services were administered by a commercial entity. Sacramento and San Diego implemented a Geographical Managed Care (GMC) model, in which mandated Medicaid enrollees could choose among several commercial plans contracting with the government and competing with each other for enrollees.

The policy change that we study in this paper is the 2010 SPD Medicaid Managed Care mandate for Two Plan and GMC counties.[16] This policy was intended to reduce costs as well as improve care coordination, and was implemented between June 2011 and May 2012 according to the month of birth for the affected individual (Harbage & King, 2012). For example, all Medicaid recipients born in July were transferred to MMC plans in July 2011, and all those born in January were transferred in January 2012.[17] The government sent out plan information and enrollment instructions 3 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.[18] Figure 2 shows the variation in MMC discharges in our sample based on birth month and demonstrates the impact of this staggered enrollment process on actual enrollment.

III. Data

Our main sources of data are the official records of all hospital patient discharges and emergency room (ER) visits in California from 2008 until 2014. Each year, there are approximately 4 million hospital discharges and 10 million ER visits (excluding those transferred to inpatient care and

[16] 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.

[17] Note that in our data individuals born in May appear to be the first group to move into MMC (as early as May 2011), though by policy documentation they should have been the last group to switch. In our empirical analysis we follow the policy adoption pattern observed in the data to construct policy change indicators, rather than the government documentations.

[18] 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 and as a result a smaller fraction were randomly allocated to a plan and provider network (Harbage & King, 2012).

instead captured by the hospital patient discharge data). For each discharge and ER 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 also observe charges and enrollment status in MMC.[19] Importantly, both datasets contain (encrypted) social security numbers of the patients, which enables us to link the two datasets together and track each individual over time. This includes the ability to track patients across payers over time. Therefore, if a patient moves into Medicare (e.g. through eligibility under the Social Security Disability Insurance Program) or into commercial coverage they would still be present and accounted for in our data.[20]

We augment the patient discharge data (PDD) with official California death records from 2009 until 2013. For all individuals with a hospital discharge between 2009 and 2013 and who have died by the end of 2013 (either in hospital or after discharge), we link their death record to our inpatient utilization data through the encrypted social security number. As a result, our sample for the mortality analysis only includes individuals who had at least one inpatient stay (either before or after the reform).[21] These linked death files enable us to construct a sample of panel data that tracks healthcare utilization for each patient over time that correctly 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.

Our main analysis sample is a person-quarter level linked panel data set of hospital discharges, ED visits, and mortality for the individuals most likely affected by the SPD mandate. In order to

[19] Note that managed care indicator is only available in hospital discharge data. Thus in ER records we cannot tell apart MMC and Medicaid fee-for-service. Additionally, it appears that some discharge and especially ER 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 as well.
[20] Table 1 shows that, while no one in the analysis sample had any Medicare covered hospital discharge in 2009Q2-2010Q1, by the end of 2013, around 14 percent of hospital discharges in treatment counties (19 percent in COHS counties) have Medicare as the main payer.
[21] We run a robustness check of the mortality analysis limiting to just those with an inpatient stay prior to reform. The main results are qualitatively similar, though more transitory with the more restrictive sample. We also find smaller differences between the high vs. low baseline utilizers, likely driven by the smaller differences in baseline health condition when compared with the larger sample.

determine initial insurance status, we restrict our sample to individuals who ever had one or more hospital discharges or ER visits covered by Medicaid (and no discharge or ER visit covered by Medicare[22]) between the second quarter of 2009 and the first quarter of 2010. We also restrict our sample to those who were born before 1969 to focus on the older individuals most likely affected by the policy change and to individuals who were born after 1950 to ensure that all individuals are below age 65 (to avoid the transition via aging to Medicare) in our sample period. Individuals in our sample do transition into Medicare because of disability status. Importantly our data still allows us to capture the health care use of these individuals.

To further restrict our sample to those most likely affected by the reform, we exclude several groups from the data. The largest excluded group includes all birth or pregnancy related discharges, a category of beneficiaries which is discussed extensively in the existing literature and were largely unaffected by the reform.[23] To minimize the confounding effects of concurrent policy changes, most notably the implementation of the ACA since January 2014, we restrict the analysis period to April 2009 until December 2013, and restrict residence to Two-Plan, GMC, and COHS counties that had no other concurrent changes in MMC policies.[24] After merging the PDD and ED data through the unique identifier, we drop individuals with disagreeing birth dates. We then drop person-quarter level observations after the recorded death, after dropping individuals with erroneous death records (e.g. individuals with multiple death records or with observed discharge and ER visit after recorded death).

[22] To reduce confound from Medicare policies, especially since dual-eligible beneficiaries are exempt from the mandate.

[23] We also drop observations with missing social security number and those with multiple birth dates associated with the same social security number to improve accuracy and comparability with the analysis sample. We also winsorize length of stay at 365 days in the hospital discharge data since stays longer than 365 days are very likely to be errors. The constructions of working samples are identical for PDD and ED, except that the restriction on length of stay is not applicable to the ER visits.

[24] In the 16 Two-Plan and GMC counties that implemented the mandate, 3 counties (Fresno, Madera, and Kings) are excluded from the analysis sample since they formed Regional Two-Plan model in March 2011. In the 14 COHS counties that implemented MMC before 2012, 5 counties (Ventura, Sonoma, Merced, Marin, and Mendocino) are excluded from the analysis sample since they adopted COHS MMC in 2009-2011. Hence the treatment counties in our analysis sample include: Los Angeles, San Diego, Riverside, San Bernardino, Santa Clara, Alameda, Sacramento, Contra Costa, Kern, San Francisco, San Joaquin, Stanislaus, and Tulare. The COHS control counties in our analysis sample include: Orange, San Mateo, Santa Barbara, Solano, Monterey, San Luis Obispo, Santa Cruz, Yolo, and Napa. In our raw hospital discharge data, 86 % of observations are for residents in these counties in 2009Q2-2013Q4 (similar ratio when restricting to those with Medicaid as primary payer). In our raw ER visit data, 84 % of observations are for residents in these counties in 2009Q2-2013Q4 (80% when restricting to those with Medicaid as primary payer).

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 ER 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,[25] and quarter of observation. Table 1 contains descriptive statistics for our sample. Panel A contains data for only individuals living in treatment countries while Panel B contains data on those living in COHS counties. Overall, our final sample begins with 132,635 patients and declines over time to 128,245 with individuals exiting our sample as they die.

An important consideration caused by our selection criteria is that individuals enter our sample only if they have an inpatient hospital or ER visit between the second quarter of 2009 and the first quarter of 2010. As would be expected, these individuals exhibit some degree of mean reversion. This can be seen in the relatively large decline in utilization between the sample selection and the pre-reform time periods detailed in Table 1. For three reasons this is unlikely to cause economically meaningful problems for our analysis. 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, similar mean reversion is happening in the COHS county sample in Panel B – which serves as an additional control in our second specification. To the extent we were concerned about a relationship between birth month and the timing of mean reversion, this additional control group should help to address that issue. Finally, in our empirical analyses, we do not use data from the sample selection period, so the higher utilization during that time period does not directly affect our estimation.

[25] Note that there are around 5% individuals in the PDD analysis sample and 10% individuals in the ED analysis sample who have been observed in multiple counties. We drop all individuals who have been observed in more than one county throughout our analysis period.

In particular, the baseline utilization of medical services that we use to interpret the magnitude of our coefficients is based on the pre-reform and not the sample selection period in Table 1.

IV. Demographics of the Seniors and Persons with Disabilities Fee-for-Service Population

The primary contribution of our paper is estimating whether there is heterogeneity in the effect of privitization based on patient health. In particular, we are interested in whether there are adverse consequences for the sicker and more vulnerable populations originally exempted from mandatory MMC enrollment. We next demonstrate those affected by the reform in our data were in fact different from California's original MMC enrollees on many dimensions that may influence the effect of privatization.

Prior to the reform studied in this paper, SPD beneficiaries in reform counties were not required to enroll in MMC. While approximately 140,000 beneficiaries did voluntarily enroll, an additional 240,000 remained in FFS (Harbage and King, 2012). Our treatment group is comprised of these FFS enrollees. Table 2 contains data on three groups of enrollees: beneficiaries in MMC before and after the reform, those switching from FFS to MMC because of the reform, and those who remained in FFS before and after the reform.[26]

Table 2 documents that individuals moving from FFS to MMC were meaningfully older than those already enrolled in MMC. We then examine measures of health status including the average length of stay in the hospital and total hospital charges (which provide an approximate measure of the intensive margin of the use of hospital services). Overall, individuals who moved from FFS to MMC had longer lengths of stay and greater hospital charges prior to the reform than those who were already in MMC.

[26] The mandate allows some groups such as dual-eligibles (i.e. individuals eligible for both Medicare and Medicaid) and those in long-term care to remain in FFS.

V. Econometric Strategy

In order to estimate the causal effect of enrollment in managed care on healthcare utilization and health outcomes, we exploit two main sources of variation in the implementation of California's program. Our main specification is a differences-in-differences analysis that leverages the fact that individuals were mandated to enroll in MMC based on their birth month. Specifically, we estimate:

$$y_{it} = \beta_0 + \beta_1 mandate_{it} + \gamma_t + \alpha_i + \varepsilon_{it} \qquad (1)$$

in which $mandate_{it}$ is a variable that measures exposure to the mandate based on the individual's birth month and quarter of observation, γ_t are quarter fixed effects, α_i are individual fixed effects, and ε_{it} is an idiosyncratic error term. Standard errors are clustered at the individual level. The variable $mandate_{it}$ codes whether individuals are coded as being exposed to the mandate if their birth month was required to enroll in that quarter.[27]

Our main identifying assumption is that the timing of MMC enrollment is orthogonal to other factors that might affect an enrollee's utilization of healthcare services and health outcomes. This assumption is reasonable since enrollment is based on the calendar month of birth across all ages. Of course, there could still be two concerns about identifying the effect of MMC in this way. The first is that there is an underlying relationship between month of birth and the use of healthcare services. While we are unaware of any such factor, we attempt to address this concern by estimating event-study specifications that allow us to examine the time path of outcomes before and after the reform-induced shift to managed care. This allows us to more clearly examine both the pre-trends in our

[27] This $mandate_{it}$ variable is coded as either 0, 1/3, 2/3 or 1 based on what fraction of the quarter of observation an individual was exposed to an MMC mandate. For example, an individual born in August was required to enroll in August of 2011. For those individuals, during the third quarter of 2011 $mandate_{it}$ is coded as 2/3 reflecting the individual was required to enroll in MMC for 2 of the 3 months of that quarter. Similarly, an individual born in September would be coded as 1/3 during that quarter. In each subsequent quarter, the $mandate_{it}$ variable value for those individuals is coded as 1. Note the special case for individuals born in May: should shift to MMC in May 2012 but shown to have shifted to MMC in May 2011; coding the mandate indicator according to the data pattern for the beneficiaries born in May.

outcomes of interest and the time path of the estimated changes. Specifically, we estimate the following specification:

$$y_{it} = \sum_{r\leq -3}^{r\geq 4} \theta_r \mathbb{I}(t = mandate\ quarter_{it} + r) + \gamma_t + \alpha_i + \epsilon_{it}. \quad (2)$$

We include a full set of pre (up to 3 quarters prior to) and post (up to 4 quarters post) mandate indicator variables (with the quarter before indicator excluded) for each individual that depends on his/her birth month, county of residence, and the quarter observed.[28,29]

There could also be concerns about broader changes in the provision of healthcare services that occur at the same time as the mandate. These could include changes in the supply of healthcare in response to the reform that could occur for all enrollees regardless of their birth month. To address this concern, we estimate a second specification in which we include data from enrollees that live in the COHS counties. These individuals were enrolled in MMC programs prior to the reform and therefore their experiences can control for these secular trends. For these individuals, $mandate_{it}$ is always coded as zero as they are unaffected by the mandate.

Our primary outcomes of interest are inpatient hospitalizations, ED Visits, and mortality. For inpatient hospitalizations, we are interested in both the total number of visits and the source of admission. For mortality, we have a person-quarter level indicator for mortality (both in and out of the hospital) and we drop all observations for individuals after their death.

As discussed above, we use data from the second quarter of 2009 through the first quarter of 2010 to select our sample and to construct the baseline characteristics for our heterogeneity analysis.

[28] Note that in the event study specification without COHS control groups, we only include indicator for T-3 instead of T-3+ to address the collinearity concern of time FE, relative time FE, and individual FE.

[29] Note that the indicator for quarter of mandate implementation (r=0) is scaled by the fraction of the quarter (1/3, 2/3, or 1) exposed to an MMC mandate. For example, for an individual in the treatment group born in August, T-3+ indicator is coded as 1 in 2010Q2, 2010Q3, and 2010Q4 and 0 in all other quarters29, T-2 indicator is coded as 1 in 2011Q1 and 0 in all other quarters, T-1 indicator is coded as 1 in 2011Q2 (but excluded in actual regression with coefficient normalized to 0), T indicator is coded as 2/3 in 2011Q3, T+1 indicator is coded as 1 in 2011Q4, T+2 indicator is coded as 1 in 2012Q1, T+3 indicator is coded as 1 in 2012Q2, and T+4+ indicator is coded as 1 in 2012Q3 – 2013Q4.

We then estimate the effect of mandatory MMC enrollment using data from the second quarter of 2010 through the fourth quarter of 2013 (i.e. we do not use data from the sample selection period in our analysis). This allows us to have a meaningful length of time after the reform while avoiding the primary impact of the 2014 ACA insurance expansions, which could have effects on both the operation of Medi-Cal but also on the operations of providers. Starting the analysis period in the second quarter of 2010 also limits the impact of mean reversion from our sample selection process.

Given our interest in heterogeneity in the effect of MMC, we then estimate models that allow the effect of the mandate to vary based on the beneficiary's health status. We measure health status based on the pre-reform health of individuals. We begin by classifying individuals based on their pre-reform use of healthcare services. Specifically, we group patients based on whether they had an above or below median number of inpatient hospitalizations and ER visits from the second quarter of 2009 to the first quarter of 2010. Our second measure of health status is based on an enrollees underlying condition. This is measured using an Elixhauser score which is a count of the chronic conditions that are indicated by a patients principle diagnosis or co-diagnosis in either the inpatient discharge data or the ED visit data.[30]

Finally, we examine the effect of market structure on our outcomes by estimating models that allow the effect to vary based on whether enrollees are in markets with a large local initiative plan (i.e. a two-plan county) or in a market with a large number of private commercial competitors (i.e. a GMC county).

[30] These Elixhauser scores are unweighted, i.e. all conditions count equally to the score. An alternative measure of sickness would by a Charlson Comorbidity Index, which weights conditions based on their contribution to an individual's ten-year mortality. Appendix Tables A10 and A11 replicated our Elixhauser score results using the Charlson index as the measure of morbidity. These results are remarkably similar to our main estimates.

VI. The Effect of Medicaid Managed Care on the Use of Healthcare Services

We begin by looking at the effect of the reform on MMC enrollment. After confirming that the reform suddenly shifted a large portion of our sample from FFS to MMC, in section VI.B we estimate the change in the use of hospital services for those who moved into MMC and in VI.C we examine heterogeneity in the magnitude of these changes. In Section VI.D we examine the mortality effects of mandatory enrollment in MMC and then in VI.E we estimate whether our estimates vary based on the structure of the Medicaid managed care market.

VI.A. Effect of Reform on the Share of SPD Patients Enrolled in MMC

Our analysis hinges on the assumption that the requirement that SPD beneficiaries in the reform counties enroll in MMC based on their birth month beginning in June 2011 actually impacted the insurance status of the patients in our sample. Panel A of Figure 3 shows the share of total discharges in reform counties covered by Medicaid MMC and FFS over time. Prior to June 2011, MMC discharges are largely stable at approximately 5 percent. Similarly, FFS discharges were a relatively flat 14 percent of total discharges. Beginning in June 2011, the share of MMC swiftly increases over the next year until it stabilizes at approximately 10 percent of all hospital discharges. Over the same time period, the FFS discharge share declines until it stabilizes at approximately 10 percent of all discharges.

Panel B of Figure 3 shows the share of MMC discharges by age cohort. As would be expected, the pre-existing carve-out for older and disabled Medicaid recipients caused the share of pre-reform discharges covered by MMC to be decreasing in age. For example, in January 2011 the share of MMC discharges was around 4% for the cohort born 1950-1955 and 9% for the cohort born between 1971 and 1985. However, after the reform there was far less variation in the enrollment share by beneficiary age – with the gap in discharges between the oldest and youngest cohort shrinking from 5 to -0.2 percentage points in January 2013.

Table 3 contains our estimates of the change in MMC discharges in our sample caused by the reform. Panel A displays estimates from our main specification. The first two columns contain estimates from the hospital inpatient discharge data and the data are intended to directly measure MMC status, albeit with some degree of measurement error. The first column confirms the mandate increased the number of discharges in the sample covered by the Medicaid MMC system. As a result of the reform, there was an increase of 0.023 discharges per person/quarter covered by MMC. The second column shows a corresponding decrease of 0.026 in the number of per person/quarter discharges covered by Medicaid FFS. These changes are similar in magnitude, which demonstrates that the reform largely moved people from FFS to MMC. As shown in the third column, we also find a modest increase in the number of privately insured hospital discharges, which could reflect classification errors if hospital officials mistakenly code some commercial MMC patients as privately insured.[31]

Figure 4a contains the event study estimates for our main specification that leverages only the month of birth of the beneficiary. Coefficients prior to the mandate are generally flat and close to zero – supporting our identifying assumptions. After the mandate, there is a swift increase in MMC enrollment and a decline in the number of discharges in our sample covered by the FFS system.

Our second specification relies on COHS counties serving as an additional control group. Panel A of Figure 5 shows the share of MMC discharges across different county groups. Prior to the reform, all counties have a broadly flat trend. Beginning in June 2011, the share of MMC escalates quickly in the reform counties and remains high in the COHS counties. After the reform is fully implemented, the share of discharges in reform counties is flat. In the COHS counties there is a modest one-time decline in the share of MMC, which is otherwise stable during our study period. Panel B of Figure 5 contains the share of Medicaid visits that are covered by MMC. Starting shortly

[31] It could also conceivably represent individuals moving from Medicaid into private insurance as a result of the mandate.

after the reform there is a discrete drop in this share. We are unaware of any reason for this one-time change, but note that it does happen in all of the COHS counties.

Panel B of Table 3 contains estimates of the change in insurance states for discharges from our second specification that includes beneficiaries in the COHS counties as an additional control group. Broadly speaking these estimates are quite similar to those in Panel A of the same Table. For inpatient discharges, we estimate that the mandate substantially increased the number of discharges covered by MMC. This is accompanied by a similarly large decline in the number of Medicaid FFS discharges (and increase in privately insured discharges) – which would be expected given our policy reform. Similarly, Panel B of Figure 4 contains the event study coefficients for the change in insurance status for this specification. These are largely similar to those in Panel A.

As the fifth column of this same table shows, there is a substantial decline in the number of Medicaid FFS visits to the ED. As discussed above, in contrast to the hospital discharge data, MMC ED visits are apparently coded as "private" in the ED data. The sixth column shows an even larger increase in this measure.

VI.B Effect of Medicaid Managed Care on Use of Hospital Services

We next examine the impact of mandatory MMC enrollment on the use of hospital services. Panel A of Table 4 contains our primary estimates of the impact of this shift in insurance coverage using only variation in the birth month for beneficiaries in the reform counties. As shown in the second column of Table 4, we do not find that MMC enrollment changed the overall number of inpatient discharges. We do, however, find that it caused a 2.4 percent increase in the number of ED visits compared to what would have occurred if the SPD beneficiaries had remained in the FFS system. Panel B contains the estimates for our specification that also uses beneficiaries in COHS counties as a set of controls for changes in the provision of medical care in the absence of the reform. For this specification, the pattern of estimates is largely the same.

Figures 6a and 6b contain the event study estimates of the change in ED use for both specifications. In both figures, the pre-trends prior to the mandate are relatively flat and close to zero. This provides support for our identifying assumption that the imposition of the reform was unrelated to trends in the pre-reform use of the ED. For both specifications, the increase in ED visits occurs in the quarter immediately after an individual's birth month required them to enroll in MMC. For our main specification, the pattern of estimates suggests the increase in ED visits is largely transitory as the estimated effect is trending back towards zero over time. This could be evidence that the change in ED visits is temporary and is perhaps the result of individuals moving into new and more restrictive insurance networks for the first time. However, the specification using COHS counties as a comparison group shows a more persistent increase in the use of the ED because of enrollment in MMC. This suggests some of the transitory nature of the change in ED visits in the main specification may be the result of the lack of a comparison group in the first specification to control for the evolution of healthcare services after the reform is fully implemented.

To further understand the nature of the increase in ED visits we also examine the underlying reason for the visit. An increase in the ED could reflect many things. First, the visits could reflect a difficulty in receiving non-emergent medical services in an outpatient setting or physician's office. Second, it could reflect poor primary care management resulting in emergency services that should have been avoidable. Finally, it could reflect particular types of medical services that may be difficult to access in a more restricted network. In Appendix Table A1[32] we find evidence that suggests the increase in ED visits reflects all of these channels. Across both specifications we find an increase in visits that required the ED but were preventable, were non-emergent and were for psychiatric

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

conditions.[33] Taken together, this analysis of visit types suggests that at least part of the increase in ED visits is the result of reduced access to healthcare services outside of the hospital.

We also find that MMC enrollment is associated with a large and statistically significant increase in the number of inpatient admissions that began as transfers from other hospitals. Figures 7a and 7b contain the event study coefficients for both specifications and contain a pattern of estimates that demonstrates the increase is largely a transitory shock. This would be consistent with SPD beneficiaries requiring time to adjust to the new provider networks associated with managed care plans. Supporting the role of provider networks, we note that many of these transfers could have begun in the emergency room of the initial hospital. If that hospital were out of network for the private plan the patient would be transferred to an in-network hospital after being stabilized for their inpatient admission. Appendix Table A2 contains estimates based on the initial hospital admission that demonstrates that most of these transfers are individuals that entered the hospital through the ED of the transferring hospital.[34]

Looking at other types of hospital visits, we find little evidence of other changes in the use of the inpatient hospital or the ED. For example, we estimate negative but not statistically significant coefficients for non-transfer non-ED hospitalizations and scheduled hospitalizations. If MMC firms were expending efforts to reduce "unnecessary" hospitalizations, these are categories of visits where we might expect to see a change in use. This provides some evidence of limits in how much of a change in the use of hospital services the MMC firms achieved in the SPD population. We also find no evidence that the shift to MMC increased the number of inpatient visits admitted from each hospital's own ED.

[33] In our specification that includes COHS counties we also found an increase in visits for conditions that were non-preventable and emergent, however we did not find evidence of this in our main specification.
[34] We can identify the original admission for approximately 60 percent of the transfers.

It is possible that rather than decreasing the quantity of visits, MMC firms implement selective networks to steer patients to lower price or higher value facilities. To examine this possibility, we next examine changes in discharges at various types of hospitals. These estimates are contained in Appendix Table A3 and Appendix Figures A1 – A8. We observe an increase in discharges at for-profit hospitals and a decline in discharges at non-profit and government hospitals, which may reflect the characteristics of providers in the MMC networks. While we observe an increase in ED visits at most types of facilities, the increase in most pronounced at non-profit and government hospitals. We also see a decline in discharges from teaching hospitals. Given that these hospitals tend to be more expensive, these estimates are indicative of cost-reducing measures of the MMC plans. That said, we do not find precise estimates across both specifications.

VI.C Effect of Medicaid Managed Care on Mortality

If the increase in ED visits and hospital transfers simply represents a learning or transaction cost for new MMC enrollees, it might not be particularly concerning. After all, every policy has some degree of transition cost during implementation. However, particularly sick individuals could suffer meaningful adverse health consequences from even relatively minor disruptions in access to care (Buchmueller, Jacobson and Wold 2006; Baum et al. 2019). Notably, this includes difficulty in accessing not just the hospital visits we observe but also primary care physicians, specialists, and other non-ED outpatient facilities that would also be affected by a more restrictive managed care network. In addition, MMC firms can impact the use of pharmaceuticals which could impact the health of enrollees (Layton et al. 2019).

These very concerns about differential adverse consequences drove the initial decision of California (and other states) to exempt the SPD population from mandatory MMC enrollment in the first place. Obviously, the most consequential health outcome an enrollee could experience is death, which we observe through our linked mortality files. Table 5 contains estimates of the effect of the

reform on mortality. Panel A contains the estimate from our main specification[35], which shows that the MMC mandate increases mortality by a statistically significant 0.09 percentage points. This is a nearly 12 percent increase over baseline mortality. Panel B contains the estimate from our specification including COHS counties. This estimate suggests a smaller, but still statistically significant, 8.8 percent increase in mortality.

Figures 8a and 8b contain the event study coefficients for the outcome of mortality for the two specifications. Prior to the mandate, the estimated coefficients are generally small and not statistically distinguishable from zero. There are also no demonstrable pre-trends in these estimates. However, after the reform the estimates are uniformly positive and larger. While the estimates are not always statistically significant, the pattern of coefficients suggests our estimate of increased mortality in Table 5 is not simply a statistical artifact.

To further investigate whether the estimated mortality increase is caused by mandating MMC enrollment, we next present a series of exercises intended to demonstrate both the robustness and the plausibility of our estimates. First, we note that there could be a concern that since mortality is a relatively infrequent outcome, the linear probability model is an inappropriate choice for estimating the effect of MMC on this outcome. To address this concern, we estimated a logit model and plotted the average marginal effects in Appendix Figure A9. Reassuringly, the average marginal effects from the logit model follow an almost identical pattern as the OLS event study estimates.

There could be an additional concern that our estimated mortality increase is simply reflecting an unrelated change in a particular locality rather than a widespread response to mandatory MMC enrollment. To examine this possibility, Appendix Table A4 contains the estimates from a leave one

[35] Note that the main specifications and samples are slightly modified for the mortality analysis. We exclude individuals without any hospital discharges in 2009Q2-2013Q4 from the analysis sample, since we can only track deaths outside hospitals for those with discharge records. We run linear regressions of outcome variable (0/1 death indicator) on mandate indicator(s), quarter FEs, age FEs, and gender indicator with standard errors clustered at county level, instead of controlling for individual fixed effects and clustering standard errors at individual level. We also run robustness checks by additionally controlling for county FEs and get qualitatively similar results.

out analysis at the county level. Each coefficient in the table is the estimate from a specification that leaves out the respective county. For comparison purposes, the overall estimate is included in the first row. These estimates provide no compelling evidence that the change in mortality is a statistical artifact driven by a particularly county.

Finally, to examine the plausibility of our estimated mortality increase we consider whether the mortality increases were concentrated in individuals with medical conditions where temporary disruptions in care could plausibly result in such adverse health consequences. Table 6 contains the estimated change in mortality for samples based on whether an individual had an inpatient hospital visit for a particular medical condition (defined at the level of ICD-9 codes). The estimates in this table are from our main specification (in Appendix Table A5 we provide similar estimates for the specification including beneficiaries in COHS counties).

In Panel A of Table 6, we find the largest and most consistently estimated effects for individuals suffering from circulatory conditions. These conditions include hypertension, ischemic heart disease, heart failure, and other cardiac conditions. It is plausible that for individuals suffering from these conditions even limited disruptions in care could cause an increased risk of death. For these individuals, mandatory enrollment in MMC causes an approximately 27 percent increase in mortality. The magnitude of this effect is broadly consistent across our two specifications.

In Panel A of Table 6 we also estimate statistically significant increases in mortality for Infectious and Parasitic Diseases and Diseases of the Blood and Blood-forming Organs.[36] However, the estimates for these other conditions are generally smaller and are not consistently estimated across the specifications. These estimates also include the effect of individuals with comorbidities of cardiac conditions and these other outcomes. In Panel B of Table 6 and Appendix Table A5, we provide

[36] In Panel A of Appendix Table A5 (including COHS control counties), we estimate statistically significant increases in mortality for Infectious and Parasitic Diseases, Diseases of the Digestive System, Diseases of the Genitourinary System, Diseases of the Musculoskeletal System and Connective Tissue, and Injury and Poisoning.

estimates for these medical conditions after removing all individuals with circulatory conditions. After removing those individuals, we find little evidence of an increase in mortality among individuals suffering from other medical conditions. Finally, Table 7 contains estimates comparing the change in mortality for individuals with a circulatory condition compared to all other individuals in our sample. This shows a large and statistically significant increase in mortality for those with circulatory conditions and no change in mortality for the rest of the sample.

VI.D Heterogeneity in the Effect of Privatization by Patient Health

Compared to the existing literature, our estimates provide initial evidence that the consequences of privatization varies across patients. We next look for additional evidence of such heterogeneity within our sample of SPD beneficiaries, which was itself not homogenous in health status. To that end, we split our sample based on two measures of health status. Our first measure is based on whether an individual had an above or below median use of inpatient and ED hospital services prior to the reform.[37] Our assumption is that those with a greater use of hospital services prior to the reform represent the type of sicker population that might suffer more adverse consequences from mandatory MMC enrollment. There could be a concern that the use of hospital services provides an incomplete measure of health. Therefore, our second measure of health status relies on the severity of the underlying conditions that caused individuals to seek treatment at the hospital. The severity measure is based on whether an individual had an above or below median[38] number of baseline chronic conditions (the Elixhauser score), calculated from the principal and co-diagnoses of all their inpatient hospitalizations and ED visits in 2009Q2 – 2010Q1.

[37] We classify an individual as heavy utilizer if the total number of hospital discharge and ER visits in 2009Q2 – 2010Q1 is greater than or equal to the median, which is 2 in our sample.
[38] The median baseline Elixhauser score is 1. Thus an individual is classified as high severity patient if baseline Elixhauser score is greater than or equal to 2.

Table 8 contains estimates where the effect of mandatory enrollment in MMC is allowed to vary by these measures of health status. We find that the overall change in ED visits discussed above is almost entirely driven by less healthy SPD enrollees. For example, we find that the population with an above-median use of hospital services prior to the reform had a 4 percent increase in ED visits as a result of the reform. This estimate is statistically distinct from the below median utilizers, who had a change in ED visits that was negative and not statistically distinguishable from zero. This was also true for our specification including COHS counties. Moreover, Appendix Table A6 shows almost identical patterns of MMC effect heterogeneity, using the severity measure based on baseline Elixhauser scores. Figure 9 and Appendix Figure A10 contain the event study coefficients from both specifications, using the utilization measure and severity measure. These event studies provide compelling evidence of a casual effect for the sicker patients and no meaningful change in ED visits for those with lower baseline utilization or severity across both specifications.

Looking at non-ED transfer hospitalizations, we also see this is primarily driven by less healthy enrollees. Similarly, in Appendix Tables A7 and A8, we see that the changes in discharges and ED visits by facility types are mainly driven by the high utilizers.

Finally, to further analyze the causal role of mandatory MMC enrollment on mortality, we next examine heterogeneity based on health status. Recall that the estimated change in ED visits and hospital transfers are concentrated among those who had either an above median use of hospital and ED services or an above median co-morbidity prior to enrollment in MMC (i.e. those who we assume are the sicker members of the sample). If the estimated mortality increase was driven by mandatory MMC enrollment, then it would be reasonable to expect this change in mortality is also driven by these potentially sicker individuals. To examine this question, Column (2) of Table 5, contains estimates that allow the effect of MMC on mortality to vary by the pre-reform use of hospital services. These estimates demonstrate that the increase in mortality is driven almost entirely by patients with

an above median use of hospital services prior to the reform. Similarly, Figures 10a and 10b provide the event study estimates allowing the effect to vary by whether the individual was a high or low utilizer of hospital services prior to the reform. The fact that the mortality effects were driven by the same groups that experienced changes in the use of the hospital services provides further evidence that the effect is caused by mandatory MMC enrollment for this population rather than another unobserved confounder. Appendix Table A9 and Appendix Figure A11 contain similar analysis based on the patients comorbidity score and finds similar estimates.

VI.E. Heterogeneity in the Effect by Market Structure

The results above demonstrate that the concerns about the adverse consequences of the incentives to reduce costs have some basis. However, it is also possible that a more competitive environment among MMC firms could ameliorate some of these outcomes by changing the firms' incentives. In order to examine this point, we next take advantage of the variation in the structure of California's Medicaid program.

As described above, within the reform counties in our sample there are two dominant market structures: GMC and two-plan counties. Enrollees in a two-plan county had two choices for insurers: a government local-initiative plan run by the county or a commercial plan. The majority of enrollees in these counties enrolled in the local-initiative plan – thus while they are in the MMC program they are not served by a *private firm* who is the residual claimant on the capitated payment. However, two counties were organized as GMC counties where enrollees could pick between several privately run MMC plans but did not have an option for a government run plan.

If the competition between privately run plans changes the incentives and actions of the private firms, we should observe differences in the use of hospital services by enrollees that were mandated to enroll in MMC in GMC counties compared to Two-Plan counties. Table 9 contains

estimates of the effect of the reform where the estimates are allowed to vary by county type. Results are presented for both the use hospital services and health outcomes.

Overall, we estimate little difference in the effect by county type. The only meaningful difference is that enrollees in GMC counties had a greater reduction in scheduled inpatient hospital services. These types of visits are a category where we might expect a managed care firm to implement utilization management techniques to decrease the use of low value care. This provides some limited evidence that market structure may affect the actions of Medicaid managed care firms – though the identifiable scope of this impact in our setting is quite small.

VII. Conclusion

Our estimates provide new evidence about the effects of requiring more complicated patients to enroll in privately operated managed care plans. On the whole, these estimates demonstrate heterogeneity in the effects of MMC and show that the sickest patients suffer adverse consequences from the requirement to enroll in MMC. Specifically, we find that individuals who were sicker (as indicated by a higher use of hospital services while in the FFS system or by a greater disease burden) experienced an increase in the use of ED services, hospitals transfers, and mortality. We found limited evidence of an overall reduction in the use of hospital services or other indications of reduced spending from the hospital sector. This suggests states may want to exhibit caution before broadly expanding the set of patients required to enroll in MMC. It also may also cast doubt on the desirability of requiring mandatory managed care enrollment in other programs such as Medicare Advantage.

Our results demonstrate that the effects of insurance extend beyond simply the extensive margin of coverage. Changes in the form of the insurance contract can impact both the use of healthcare and mortality. In that way, this study contributes to an emerging literature about the mortality consequences of health insurance coverage. For example, both Miller et al. (2019) and Goldin, Lurie and McCubbin (2019) find that the increases in insurance coverage caused by the ACA

decreased mortality among affected populations. Similarly, Abaluck, Bravo, Hull, and Starc (2020) estimate mortality increases across MA plans.

Our results stand in marked contrast to those of Layton et al. (2019) which also examines the question of MMC for older and sicker populations in other states. Examining managed care in Texas, these authors found MMC allowed for an increase in the use of prescription drugs and a reduction in inpatient use. The types of inpatient use where they see a reduction are visits that are likely responsive to an increased use of prescription drugs. Looking at New York they found a large reduction in the use of inpatient services. They posit that this reduction is likely the result of stinting by the private firms that are residual claimants on healthcare spending. While we find no reduction in the use of inpatient services overall, if we examine the markets where commercial firms are competing for business we do find suggestive evidence of limited declines in inpatient admissions. For some outcomes these declines are statistically significant and overall we cannot rule out fairly large declines in admissions that are either scheduled or did not begin in the ED.

Rather than viewing the difference in results across the two studies as a point of contention, we believe it demonstrates the important local nature of state Medicaid systems. Examining our results in combination with those of Layton et al. (2019) serves to reinforce the oft-stated maxim, "if you've seen one Medicaid program, you've seen one Medicaid program" (Adams, 2013). As a result, policymakers and economists should exhibit meaningful caution in attempting to generalize results across both states and types of patients.

At a minimum, the generosity and structure of the FFS program is important. For example, the largely positive effects for Texas appear to be driven by a rather stringent cap on pharmaceuticals in the FFS program – restrictions which do not exist in our setting. In addition, the lack of large negative health effects (such as the mortality effects we find) could be driven by the exclusion of

inpatient spending in the MMC program for disabled residents in Texas, which is again not a feature of the California expansion.

We also would caution anyone from drawing any broad conclusions from our estimates about the overall welfare implications of California moving complicated patients into MMC. We show that for the most complicated patients, MMC appears to have adverse consequences in terms of both an interruption in the usual source of care and an increase in mortality. However, there were many less complicated patients that were included in the SPD population and appear to be largely unaffected by the policy change. It is possible, that there were great savings or benefits for these individuals from the move to MMC that could offset the costs to the more complicated patients. At a minimum, our results demonstrate that the potential costs of such a move are meaningful and cannot be predicted by the experiences of either younger or healthier patients in California or of similar patient populations in other states.

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