Disruptions to the Patient-Provider Relationship and Patient Utilization and Outcomes: Evidence from Medicaid Managed Care[☆]

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Abstract

The patient-provider relationship is considered a cornerstone to delivering high-value healthcare. However, in Medicaid managed care settings, disruptions to this relationship are disproportionately common. In this paper, I evaluate the impact of a primary provider's exit from a Medicaid managed care plan on adult beneficiary healthcare utilization and outcomes. Using an event study approach, I estimate a 5% decrease in the number of beneficiaries with primary care visits in the year following the exit, with slightly larger effects in terms of percentage points for patients with chronic conditions. Additionally, I observe a nearly 50% increase in the number of beneficiaries with a chronic condition who are hospitalized following a disruption.

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1. Introduction

The relationship between a patient and their doctor is widely regarded as a critical component in achieving good health outcomes (Institute of Medicine, 2001). Patient-provider relationships that are longitudinal and uninterrupted have been associated with promoting patient engagement in primary and preventive care (O'Malley et al., 1997; Saultz and Lochner, 2005) and minimizing risks of adverse health events, particularly in at-risk patients such as medically complex individuals and those with chronic conditions (Bayliss et al., 2015; Bindman et al., 1995; Hussey et al., 2014). However, in Medicaid managed care, which covers over 80% of Medicaid beneficiaries via state-contracted managed care plans (Mathematica Policy Research, 2019), institutional factors such as relatively limited provider networks and relatively high provider churn out of networks suggest disproportionately high rates of truncated patient-provider relationships (Ndumele et al., 2018). Further, there is sparse empirical evidence regarding the effects of these disruptions on patient utilization and wellbeing, which can be challenging for policymakers and providers who may need to consider interventions that trade off improving care delivery and access with disrupting existing relationships.

In this study, I estimate the causal effect of disruptions to the patient-provider relationship in Medicaid managed care by evaluating the effect of a provider's exit from a Medicaid managed care network on their patients' utilization and health outcomes. This approach is similar to several recent papers that have exploited changes in provider availability to the patient, like retirement or practice closures, to estimate the relationship between disrupted relationships and outcomes (Kwok, 2019; Sabety et al., 2021; Simonsen et al., 2019; Zhang, 2018). However, these investigations have featured settings with a single insurance provider or health system, and where alternative providers are relatively accessible. By focusing on disruptions in Medicaid managed care networks, I aim to fill in the gap in our understanding of disrupted patient-provider relationships in more restrictive settings in which a provider's exit from a plan may significantly limit a patient's ability to access care.

I use a robust administrative Medicaid claims database, the Medicaid Analytic eXtract (MAX), to evaluate the impact of disruptions to the patient-provider relationship among Medicaid managed care enrollees after their primary provider exits their plan's network. Because managed care organizations discourage the use of out-of-network providers, this exit likely represents a truncation of the relationship. I focus on the exit of a beneficiary's "key provider," a physician or advanced practice nurse of any specialty who is responsible for the plurality of the patient's outpatient encounters in their first six months of enrollment. This definition is intended to flexibly identify the provider who is most important to the patient, and whose exit represents a significant emotional and/or clinical disruption to care.

I use an event study to estimate the effect of an exit on a patient's utilization of primary, inpatient, and emergency care in the following year. To improve the precision of my estimates, and to recover a single summary number, I also estimate a pre-post model on the four quarters before and after the exit. I find an average 5% decrease in the number of patients who have a primary care visit following a provider's exit, and no significant effect on hospitalizations or emergency care use.

I then explore heterogeneity in the treatment effects for a particularly vulnerable and high-risk subset of the population, patients with chronic conditions, for whom continuity in patient-provider relationships has been shown in associational studies to be particularly beneficial. Among this population, I find a slightly higher percentage point decrease in the probability of a primary care visit after a provider's exit (4.2 versus 3.4 percentage points for chronic and non-chronic enrollees, respectively), and a significant increase of 2.1 percentage points in the probability of hospitalization following a provider's exit, or an increase of approximately 50% of the pre-exit mean. Finally, I examine how these effects differ along a measure that is analogous to the degree of continuity a patient has with their provider. I find evidence of increasing effects of disruptions on outcomes of interest as continuity increases, consistent with prior associational observations.

This paper contributes to three areas of research. First, it builds on and extends existing literature on access to providers in Medicaid. Medicaid beneficiaries disproportionately experience disrupted care through a variety of different pathways in addition to provider exit, including an individual's own churn into and out of Medicaid due to state eligibility thresholds. Banerjee et al. (2010) found that 43% of Medicaid enrollees changed coverage annually between 2000 and

2004, while Sommers et al. (2016) found that even after states expanded access to Medicaid through the Affordable Care Act, approximately 25% of Medicaid beneficiaries had changed coverage within twelve months of being surveyed. Ji et al. (2017) established a link between hospitalizations and gaps in Medicaid coverage through disenrollment among specific populations such as adults with major depression, though this study did not include the Medicaid managed care population. In addition to disruptions, Medicaid enrollees face disproportionate challenges in gaining access to providers. Evidence suggests that low reimbursement rates make providers reluctant to treat Medicaid enrollees in general (Decker, 2012), and that, relative to other insurers such as Medicare and private insurance, provider participation in Medicaid is significantly lower (Health Affairs Blog, 2019). Furthermore, there is evidence that not all providers reported as participating in a Medicaid managed care network will actually treat Medicaid enrollees (Office of Inspector General, 2014; Wallace et al., 2020).

Second, this paper contributes to the ongoing discussion on the importance of the provider-patient relationship. Recent research in health economics, in which there is a growing interest in determining the relative contributions of patients and physicians on variations in healthcare utilization, has established a significant influence of primary care physicians' (PCP) practice style on an individual patient's spending and utilization, estimated from patients switching PCPs (Fadlon and Van Parys, 2020; Kwok, 2019). Additionally, researchers have found significant but mixed evidence of PCP exit on patient-level utilization. In Medicare, PCP retirement has been linked to short-term reductions in primary care utilization, with evidence of substitution towards urgent care and emergency departments (Kwok, 2019; Sabety et al., 2021; Zhang, 2018). In Denmark, practice closures have been found to increase chronic condition diagnoses and slightly increase emergency department utilization (Simonsen et al., 2019). These studies, which significantly contribute to our understanding of disrupted care, cannot necessarily be externalized to Medicaid, which features more vulnerable beneficiaries and more restrictive provider networks.

Third, the paper adds to our understanding of the efficacy of interpersonal continuity in care. Interpersonal continuity of care describes a patient-provider relationship that is "characterized by loyalty and trust" (Saultz and Albedaiwi, 2004), and generally is cultivated over a sustained period of time. Prior research has established consistent associations between continuity in relationships and positive patient experience, including an increase in patient satisfaction with care (Fan et al., 2005; Saultz and Albedaiwi, 2004; Tingley, 2018) and patient trust in their physician (Mainous et al., 2001). Studies that examine vulnerable populations have reported correlations between continuity and often small decreased risks of inpatient and/or emergency department admissions among seniors (Bayliss et al., 2015; Nyweide et al., 2013), veterans (Chaiyachati et al., 2014), Medicaid enrollees (Gill and Mainous, 1998; Gill et al., 2000), and other vulnerable populations (Tingley, 2018). Turnover among PCPs has been linked to decreased patient satisfaction in the Veterans Health Administration (VHA), but no significant effect on ambulatory care quality (Reddy et al., 2015). While these findings have contributed to our understanding of the importance of continuity in patient-provider relationships, there have been challenges in differentiating between association and causation of continuity and outcomes of interest (Saultz and Albedaiwi, 2004; Saultz and Lochner, 2005).

This paper proceeds as follows. In Section 2, I briefly discuss relevant institutional details of Medicaid managed care. I provide details on my data sources and sample construction in Section 3, and discuss key measures relevant to the study in Section 4. I discuss my empirical approach in Section 5, and present results and robustness checks in Section 6. Section 7 concludes with a discussion of the findings, and a back-of-the-envelope calculation of the costs of disruptions to the most affected populations.

2. Medicaid Managed Care

One in five Americans are currently covered by Medicaid, representing the most economically and socially vulnerable children, elderly, disabled, and low-income adults. Over the past several decades, states have shifted from providing Medicaid coverage in a largely fee-for-service approach to contracting with managed care plans who assume responsibility for their enrollees' healthcare. Delivery of Medicaid services through managed care plans has become the predominant form of coverage for Medicaid enrollees, growing from 9.5% (2.7 million enrollees) of all Medicaid in 1991 to 82% (66 million enrollees) in 2017 (Kaiser Family Foundation, 2001; Mathematica Policy Research, 2019). These plans are a mix of for-profit and not-for-profit, Medicaid-only and commercial insurance plans. Under the most common arrangement of comprehensive (risk-based) managed care, states pay the managed care organizations a capitated rate in exchange for the plan assuming some or all of the financial risk for covering the Medicaid beneficiary's health care. My study sample focuses on beneficiaries enrolled in these comprehensive managed care (CMC) plans. At their time of enrollment, beneficiaries either choose or are assigned to plans and/or providers. Assignment mechanisms and frequencies vary by state, and are largely unknown with respect to providers, though slightly more is known about plan mechanisms and rates (Smith et al., 2015).

Once a plan is selected (or auto-assigned), CMC enrollees have access to the network of providers contracted by the managed care organization, though not all providers listed as participating in the network may actually be willing or available to see Medicaid enrollees. A 2014 report by the Office of the Inspector General found that less than half (49%) of providers in a random, representative sample of Medicaid managed care participants were available to new Medicaid patients, suggesting that "Medicaid managed care enrollees may not be able to make appointments with as many as half of the providers listed by their plans" (Office of Inspector General, 2014). State contracts with managed care plans generally impose provider network adequacy thresholds to ensure some minimum level of access to care for beneficiaries, though states have flexibility on determining those standards and in enforcing them (Hinton et al., 2019). For example, in 2018, eight states required a PCP-to-beneficiary ratio from between 1 to 1,200 to 1 to 2,500 (Rosenbaum et al., 2018). Providers may exit plan networks for a variety of reasons that include retirement, relocations, or preferences, as well as plans' efforts to manage spending and quality (Flynn et al., 2002; Howard, 2014). Ndumele et al. (2018) find an annual "churn" rate of PCPs out of CMC's of approximately 12%, compared to a 9% annual churn in the VHA (Reddy et al., 2015), with five-year churn significantly higher in narrow networks (by approximately 20-percentage points) that employ less than 30% of physicians in the plan's market.

3. Data Sources and Sample

3.1. Medicaid MAX Data

I use Medicaid claims data from the Medicaid Analytic eXtract (MAX) for six regionally diverse states—Arizona, Indiana, Kentucky, New Jersey, New Mexico, and Washington—for 2009 through 2014. Despite being the sole national database of Medicaid administrative claims, this data has historically been underused due to concerns regarding integrity of the state-reported managed care claims, or "encounter" data. Given that the vast majority of beneficiaries are enrolled in a CMC, the usefulness of this dataset has thus been limited. However, Mathematica Policy Research (MPR) has published two reports identifying states whose encounter records are sufficiently usable (i.e., have minimal data quality concerns) for 2007–2011 (Byrd and Dodd, 2012, 2015). The six states chosen for this study were selected due to their high usability according to these briefs. While similar reports on the usability of 2012 through 2014 data are not available, both the consistent usability of these states in 2007–2011 and the general trend in improved data reporting quality suggest that it is reasonable to use these additional unvetted years (in these states) in order to generate a longer panel of available claims data. Additional information on how I clean the sample is available in Appendix H.

In addition, through developing my study sample, I employ my own validation checks and thresholds to ensure that a sufficient amount of non-missing information is available for enrollees, providers, and plans for all states in this study period. Through these validation exercises, I drop six additional states that had originally met the MPR validation thresholds for adequate encounter data integrity, but that ultimately had a large amount of missing information for providers, whom I identify using the National Provider Identifier (NPI).[1] In the remaining states, a substantial amount

[1] These states are: California, Georgia, Kansas, Michigan, Virginia, and Oregon

of missing or incorrectly reported NPI's remain, particularly since some states identify providers using a state-specific legacy provider identifier (LPI) that is different from the NPI and not necessarily consistently linked to one provider across state and time. I undertake a three-step process to replace the LPI's with NPI's when possible, which includes the use of a crosswalk between NPI and LPI created by Mathematica for 2009–2011; matching the LPI to the "other provider identifier" in the National Plan and Provider Enumeration System (NPPES) downloadable file; and the creation of a crosswalk between LPI's and NPI's across years to fill in any remaining missing NPI's, when appropriate. This exercise reduces missing NPI's in a given state-year from 100% in some state-years, to generally less than 12% (with the exception of Washington in 2009, which remains missing at 20%, down from 85% missing). Detailed information on this process, and on rates of missing NPI's at each step, is available in Appendix J.

While the MAX data offer a wealth of information regarding beneficiaries, their providers, and the plans they enroll in, I use supplemental resources to further identify provider and plan characteristics. I obtained information on provider gender and age through Doximity, a professional medical network for clinicians, and supplemented age information with year of graduation obtained from the publicly available Physician Compare files. I also utilize the NPPES Downloadable File produced by the Centers for Medicare & Medicaid Services (CMS), which contains information on all providers with a National Provider Identifier (NPI), such as name, gender, specialty, and location of practice. I use the Managed Care Crosswalks published by MPR to flag the plan identifiers in the MAX data that belong to managed care organizations, ultimately limiting my sample to enrollees in these plans and dropping those beneficiaries enrolled in non-CMC plans. Used in conjunction, these datasets provide an additional level of detail that can be used to bolster information on provider and plan characteristic and fill in gaps in data as they exist, as well as to provide additional checks on data quality.

3.2. Patient Sample

My study sample includes all CMC enrollees in the six sample states who were eligible as adults (18-65) from 2009 to 2014, and who were continuously enrolled in a CMC for at least 15 months at some point during this time.[2] I exclude pediatric beneficiaries who may have less agency in who their key provider is (i.e. whose key provider may reflect the preferences of their parent), and exclude any individuals who are also dually enrolled in Medicare, and for whom I would not have access to their full set of claims. I identify enrollment episodes during which time a beneficiary was recorded as being enrolled in a CMC plan. I restrict the sample to beneficiaries who were enrolled in one managed care plan throughout their entire enrollment episode in order to avoid having to disentangle discontinuities resulting from provider exits from discontinuities in plan switches. Notably, 19% of beneficiaries switch to a different plan during a given enrollment episode. For the majority of adult Medicaid enrollees in these states during these years, CMC enrollment was mandatory. See Table C.19 in Appendix C for additional details.

In a subsample analysis, I focus on patients with chronic conditions. Chronic conditions are disproportionately common among Medicaid enrollees as compared to the general non-Medicaid population (Holahan et al., 2010; Kaiser Family Foundation, 2012), and individuals with these conditions are believed in particular to benefit from a continuous relationship with a provider (Gill and Mainous, 1998; Cabana et al., 2004). I use the diagnostic definitions provided by the Chronic Condition Warehouse to flag patients with at least one of four relatively common chronic conditions: chronic heart failure (CHF), chronic obstructive pulmonary disease (COPD), diabetes, and hypertension.[3] In my final study sample, between 25%-30% of the study sample has at least one chronic condition. The estimated 30% prevalence in the study sample is consistent with a recent literature review (Chapel et al., 2017), in which the authors report the following rates of chronic illness among adult Medicaid enrollees: 17.2–27.4% had hypertension (compared to my estimated 18–22%); 7.5–12.7%

[2]The MAX files broad eligibility reporting limits my ability to identify women who became eligible for Medicaid coverage through pregnancy (as differentiated from the general adult eligibility category). Using a flag for whether the individual was entitled to only restricted benefits for pregnancy-related services, I find that less than 5% of my final sample fall into this category during the study period. Notably, this approach is a lower bound for pregnancy-eligible women, since it does not allow me to identify women who qualify through pregnancy and who also receive full benefits. Table A.1 provides additional detail on the frequency of this flag by the study cohorts I describe in more detail below.

[3]Definitions from the Chronic Condition Warehouse are available: `https://www2.ccwdata.org/web/guest/condition-categories`.

had diabetes (compared to my estimated 9–12%); and 55.7–62.1% had one or more chronic conditions (including more than the four I focus on).

4. Measures

4.1. Key Provider

I focus on the truncated relationship between a beneficiary and their "key provider" ("KP"). I define key providers as the physician (Medical Doctor, MD; or Doctor of Osteopathy, DO) or advanced practice nurse (Nurse Practitioner, NP; or Physician Assistant, PA) who has at least three outpatient encounters with a beneficiary within the first six months following their first outpatient encounter (defined below) in a managed care plan, and who is associated with the plurality of a beneficiary's claims in that period. As a test of the sensitivity of this time frame with respect to identifying a key provider whose exit is significant to the beneficiary, I also identify a cohort of beneficiaries with a 12-month key provider definition period. I compare characteristics of the 6-month and 12-month cohorts in Appendix D, and discuss results for the 12-month cohort as a robustness check in Section 6.

To identify plurality, I calculate the share of encounters associated with a particular provider out of all encounters a beneficiary has with any provider during the 6-month period. This definition is similar to the Bice-Boxerman Continuity of Care index that measures the degree of continuity that a patient has with an individual provider or group of providers (Bice and Boxerman, 1977). Additional details on this definition are available in Appendix I. I define outpatient encounters as any claim that has 1) a place of service code indicating an office, walk-in retail clinic, independent clinic, federally qualified health center (FQHC), or rural health clinic; 2) a type of service code indicating the service was provided by an individual clinician, specifically: physicians, other practitioners, nurse midwife services, or nurse practitioner services (to avoid flagging an NPI or legacy provider identifier associated with an institution such as a pharmacy or lab as the key provider); and 3) a type of claim code that classifies the visit as an encounter for a service rendered through a managed care plan.

Other studies that have flagged key provider equivalents tend to use Evaluation and Management ("EM") visits as a denominator. The definition I use is intended to more flexibly include all settings that might capture a Medicaid beneficiary's potentially "non-traditional" and/or non-regular utilization of primary care services. I also evaluate the effect of a key provider's exit on both outpatient encounters and EM visits as representative and relevant measures of primary care, though note that EM visits represent more conventional measures of primary care utilization. For simplicity, I refer to outpatient encounters as "encounters" below.

The share of encounters associated with a key provider, as mentioned above, is related to a continuity of (relational) care index, such that larger shares indicate that the key provider was responsible for more of the patient's encounters during the initial 6 month period, which may be analogous to a higher degree of continuity in the relationship. Table A.2 reports the share of encounters associated with each treated patient's key provider, for the study sample and for beneficiaries with chronic conditions. The average share of encounters is approximately the same at 0.75 and 0.74 for the study sample and chronic condition sample, respectively. Less than 10% of patients have fewer than approximately 44% of their encounters with their key provider in the 6-month period, and more than half have at least 75% of their encounters with their key provider.

4.2. Provider Exits

To identify a provider's exit from a managed care plan, I adopt a conservative approach with the goal of avoiding the mis-characterization of providers who intermittently or irregularly see Medicaid patients as exiters. Specifically, I impose the following five restrictions. First, the provider must see a rolling average of at least three patients per year-month for all time periods prior to a potential exit, and exactly no patients thereafter. Second, the potential exit date must be at least three months prior to the end of the final year for which that state's data is available. Third, the provider must be observably participating in the plan (per Step 1) for at least five months prior to the exit. Fourth, the provider's potential exit date cannot be the same year as a plan's exit date. Fifth, the potential exit date must be in a year in which the plan has no recorded data quality concerns. Additional details of this approach, as well as in my construction of provider networks, are available in Appendix J.

4.3. Outcomes

I evaluate the effect of a key provider's exit on several measures of primary care, inpatient hospitalization, and emergency care that are generally believed to be improved through non-disrupted patient-provider relationships. Table A.3 shows the frequency and distribution of the outcomes of interest in the pre-exit period, including the share of patients that have at least one record of the outcome in a given quarter, my preferred measure.

I include two measures of primary care visits: encounters, and evaluation and management ("EM") visits. Encounters are as defined above, and are meant to flexibly capture the potentially non-traditional type of primary care that Medicaid enrollees may have. To identify EM visits, I use Berenson-Eggers Type of Service (BETOS) codes. These types of visits are commonly used as a measure of a patient's access to primary care, and vary across intensity of visit (by time and effort contributed by the physician) as well as reimbursement level. I focus on EM visits conducted in an office for either new or established patients.

I include a measure of all-cause hospitalizations ("hospitalizations"). As noted previously, prior research has established a negative association between continuous care and hospitalizations (Hussey et al., 2014; Bayliss et al., 2015; Tingley, 2018). An alternative measure is hospitalizations that are specifically attributable to (preventable) ambulatory care sensitive conditions (ACSCs). Such hospitalizations are considered preventable with adequate preventive care, and thus represent a measure of the quality of the preventive care a patient has access to (Bindman et al., 1995; Mkanta et al., 2016). ACSCs have been used widely to evaluate the quality of care provided to patients with chronic conditions in particular (Chopra et al., 2016). Ideally, I would be able to estimate the impact of a key provider's exit on condition-specific ACSCs for each of the four chronic conditions I include. However, these events are relatively rare in terms of overall hospitalizations—a study by Mkanta et al. (2016) found that out of 25,581 hospitalizations among Medicaid comprehensive managed care enrollees in 2009, ACSC hospitalizations attributable to diabetes (and related complications), COPD, and heart failure accounted for only 2.53%, 0.34%, and 1.00% of all primary diagnosis codes, respectively. I similarly observe very low levels of hospitalizations for ACSCs. Among the beneficiaries in my sample with chronic conditions, only 1% of these beneficiaries ever have an ACSC hospitalization in the four quarters prior to exit. Given these low frequencies, it would be difficult to interpret a meaningful effect of disruptions on this outcome, and thus I focus on all-cause hospitalizations, which are more frequent.

I define emergency department ("ED") visits based on the place of service code on the claim. There is some evidence that continuous care with a provider leads to decreased use of the ED in some populations (Rosenblatt et al., 2000), while PCP retirement has been linked to a slight increase in ED use in Denmark (Simonsen et al., 2019). An increase in ED visits following a provider's exit among Medicaid beneficiaries might reflect a decrease in patient wellbeing, or may reflect this particular population's propensity to seek care (primary or emergent) in non-outpatient settings, as shown by Taubman et al. (2014), who observed an increase in ED utilization among individuals who were randomly given access to Medicaid coverage through the Oregon Health Insurance Experiment. Regardless of type of care needed (emergency or otherwise), ED use is generally more expensive to Medicaid agencies and enrollees than office-based primary care, and therefore any increase in ED utilization can be interpreted as a potentially avoidable increase in spending.

I include several additional outcomes of interest. I use the BETOS measure of testing to capture any additional diagnostic efforts that might follow a disruption to care. This testing category includes lab tests for routine venipuncture, urinalysis, glucose, and blood counts, and is also used in other studies as a measure of primary care utilization or potential redundancies due to information loss after a disruption (Finkelstein et al., 2016; Kwok, 2019). I use place of service codes to flag beneficiary use of urgent care facilities and federally quality health centers (FQHC). Utilization of urgent care facilities or FQHCs is not necessarily a flag for inadequate primary care or adverse health events. A report by the Centers for Studying Health System Change proposed that urgent care centers may represent a cost-saving alternative to more expensive ED use among Medicaid enrollees (Yee et al., 2013). Falik et al. (2001) found a decreased likelihood in hospitalizations and ED visits for ACSCs among Medicaid beneficiaries in traditional (non-managed care) Medicaid who received more than 50% of their primary care from FQHCs. Thus, any change in utilization of these places of service are somewhat ambiguous with regards to the implications on beneficiary health and access.

Finally, I use prescription fills and number of prescriptions as a measure of access to prescription drugs. In particular, in an attempt to capture longer-term drug coverage for which a decrease in access might represent a clinically concerning gap in coverage, I focus on prescriptions that are supplied for at least 30 days. These measures are intended to capture general changes in access to prescription drugs, though a more thoughtful examination of the effects of disrupted relationships on changes in patients' access to clinically appropriate drugs is merited.

See Appendix I for additional details on how I identify and define these outcomes in the data, and Appendix G for a discussion of the prescription drug measures in particular.

5. Empirical Approach

5.1. Event Study

This study was approved by the Dartmouth Institutional Review Board (Committee for the Protection of Human Subjects Number STUDY0030069). My goal is to estimate the effect of a disruption to the patient-provider relationship on a patient's utilization of primary care and health outcomes. To do this, I use the plausibly exogenous exit of beneficiary i's key provider j from their network at time $t' = 0$. Beneficiaries are "treated" if their key provider exits, and "control" otherwise. My underlying assumption is that if not for the key provider's exit, beneficiary i's pattern of utilization and outcomes would be similar in trends to beneficiaries whose key providers don't exit at $t' = 0$. This assumption would be supported by a lack of significant differences in trends in the pre-exit period. Additionally, I assume that the reason for the key provider's exit is not due to an unobservable factor that also influences a particular beneficiary's outcomes around the time of the exit. For example, if low-quality providers are more likely to exit, this assumption is less likely to hold. While I cannot directly test for balance among exiters and non-exiters in terms of provider quality, I show below that other observable characteristics of providers are balanced. I also include a robustness check in which I estimate the model using only treated beneficiaries, which will increase the likelihood that treated and controls (i.e. the treated beneficiaries, before their key provider exits) are more similar in the quality of their providers.

A potential threat to my identification strategy would be if a provider's exit from a CMC does not effectively truncate the patient-provider relationship, which may be plausible if there is no financial penalty to the beneficiary for seeking out-of-network care. However, while financial risk for emergency out-of-network care is assumed by the CMC, non-emergency out-of-network care is generally not covered, as in any other (non-Medicaid) managed care arrangement. A 2009 report by the Lewin Group (The Lewin Group, 2009) noted that while Medicaid beneficiaries themselves cannot legally incur charges from providers, Medicaid health plans have little leverage to enforce this rule when care is provided outside of a pre-arranged contract, particularly among providers that are not Medicaid-certified. Additionally, plans and state Medicaid agencies are highly incentivized to discourage out-of-network care among their Medicaid enrollees since, in the absence of any federal law governing reimbursement for out-of-network non-emergency services, clinicians providing the out-of-network care tend to bill their "usual and customary" charges, which are often significantly higher than those that Medicaid fee-for-service would reimburse. Given the significant relative cost of out-of-network care to the managed care organization, state agency, and even the enrollee, it is reasonable to expect that there are sufficient financial disincentives to deter enrollees from seeking care outside of their network of providers. I observe fewer than 11 enrollees who continue to access their provider following the provider's exit from their managed care plan.

My main specification is presented in Equation 1, where i indexes a beneficiary-episode (of enrollment), t indexes calendar time (year-quarter), and $\mathbb{1}\{t' = q\}$ is an indicator for quarter from exit t' equal to q, where I focus on the 10 quarters before and after a key provider's exit:

$$y_{it} = \alpha_i + \beta_t + \sum_{q=-10, q\neq -1}^{10} \theta_q \mathbb{1}\{t' = q\} + \varepsilon_{it} \tag{1}$$

where ε_{it} is mean-zero conditional on covariates. In my preferred specification, I estimate this model using a definition of "Ever y_{it}," which is equal to 0 when the beneficiary had no record of y in a particular quarter, and 1 if the beneficiary had at least one record of that outcome in quarter t. This variable is intended to capture the change in the share of beneficiaries that had any utilization in a particular quarter following a key provider's exit, and is similar to the measure used by Hussey et al. (2014) to determine the relationship between increased continuity of relationships and a patient ever having a hospitalization or ED visit in a particular year. I prefer this definition as it provides a more straightforward interpretation of the effect of disruptions on the number of beneficiaries affected, and not simply level changes in utilization that may be difficult to interpret in terms of policy implications.

Time $t' = 0$ is defined as the last quarter in which I observed the key provider treating patients in a given plan, and can be considered a "partial" quarter since the exact exit date is unknown. For this reason, the relevant omitted relative time period is $t' = -1$, the last full quarter in which providers were observed treating patients in a plan. For treated beneficiaries, exit at time $t' = 0$ is defined only outside of the 6-month key provider identification period. I estimate Equation 1 using both the treated and control cohorts in order to more precisely estimate the effect of calendar time on outcomes of interest, setting all indicators for quarters from exit equal to 0 for controls. Treated beneficiaries must be enrolled in a plan for at least $t' \in [-1, 1]$ (in addition to the 6-month key provider identification period), but I do not restrict the sample to a balanced panel.[4] Analogously, control beneficiaries must be enrolled for at least three quarters (excluding the initial 6 month key provider identification period). I cluster standard errors at the patient level, which is where I believe any serial correlation will occur. I drop the key provider definition period from this analysis (i.e., the 6 month period in which the key provider is determined).

5.2. Pre-Post

I use a pre-post approach to recover a single estimate of the effect of key provider exit on outcomes of interest. This approach also provides increased precision in my estimates. The estimated model is similar to Equation 1, but with one indicator for the post-exit period ("Post Exit") instead of individual indicators for each quarter relative to exit ("Post Exit" is set to 0 for all controls). Observations are at the beneficiary-episode-quarter level. I estimate this model on the 4 quarters before and after a key provider's exit in order to obtain a single estimate of the short-term effect of the provider exit, informed by where the effect is most pronounced after estimating Equation 1. I omit $t' = 0$ as a partial quarter.

$$y_{it} = \alpha_i + \beta_t + \delta\{\text{Post Exit}\}_{it} + \varepsilon_{it} \tag{2}$$

where ε_{it} is mean-zero conditional on covariates.

In Equation 3, I test for heterogeneous treatment effects among beneficiaries with chronic conditions:

$$y_{it} = \alpha_i + \beta_t + \delta_1\big[\{\text{Post Exit}\}_{it} \times \{\text{No CC}_i\}\big] + \delta_2\big[\{\text{Post Exit}_{it}\} \times \{\text{CC}_i\}\big] + \varepsilon_{it} \tag{3}$$

where $\{\text{No CC}_i\}$ and $\{\text{CC}_i\}$ are indicators that the beneficiary does not and does have at least one chronic condition of interest, respectively. This analysis responds to concerns that a pooled pre-post approach may obscure the heightened sensitivity that patients with chronic conditions may have to disrupted care. I estimate this model instead of estimating Equation 2 on the subsample of treated beneficiaries who have a chronic condition (including all controls) for two reasons.

[4]In Appendix E, I test the robustness of my results to different balance requirements in the quarters around the exits, and observe largely the same effects for samples with balance for 2, 3, and 4 quarters before and after the exit.

First, this approach allows me to compare heterogeneous treatment effects for beneficiaries who have chronic conditions against those who don't. Second, this approach allows me to include all beneficiaries in the estimation; since in an event study, beneficiaries in the period prior to the main event (key provider exit) act as controls as well, including the full sample and allowing the non-chronic beneficiaries in their pre-exit period to act as controls is more consistent with the main (non-heterogeneous) specification. Notably, estimating Equation 2 on the subsample of treated chronic condition beneficiaries (and full controls) returns the same results as the specification in Equation 3.

Given prior evidence that increasing continuity in relationships is associated with improved patient outcomes, I also estimate heterogeneous treatment effects across quartiles of the share of encounters a key provider is responsible for in the 6-month definition period to determine if there is any relationship between increasing continuity and outcomes of interest in my sample. To do this, I estimate Equations 2 and 3 separately on each subset of beneficiaries whose key provider's share of encounters falls into the relevant quartile.

6. Results

6.1. Descriptive Statistics

There are nearly 2 million unique adult beneficiaries enrolled in CMC in the six sample states from 2009 to 2014, representing nearly 3 million beneficiary-enrollment episodes (the "full sample") (Table 2). My study sample includes 103,969 beneficiaries (105,548 beneficiary-episodes), of which 101,487 beneficiary-episodes do not include a key provider's exit ("control") and 4,061 do ("treated"). The majority of beneficiaries are female and white, with an average age at enrollment between 32 to 35. Approximately 12% of beneficiaries in the full sample have one of the four chronic conditions of interest. This percent is slightly over twice as large (25%) in the study sample, with 30% of the treated cohort having a chronic condition, a difference that is significant at the 0.01% level, relative to the control cohort. Notably, the prevalence of chronic conditions (overall and by specific condition) in the study sample is more in line with recent estimates of the burden of chronic disease in this population, as discussed in Section 3. While beneficiary fixed effects will control for any unobservable, time-invariant differences between treated and control cohorts that might be reflected in the slightly higher prevalence of chronic conditions in the former, I address any limitations to the external validity of my results that might be suggested by the different rates of chronic disease between the full and study samples in Section 7.

Beneficiaries in the full sample have enrollment periods of an average length of 16 months, whereas beneficiaries in the study sample are enrolled for about twice that length, at 30 months, given that included beneficiaries must be continuously enrolled for at least 15 months in order to identify their key provider and allow for a sufficient post-exit period. In the study sample, the majority of beneficiaries live in metropolitan areas. Around 70% of beneficiary-episodes have male key providers, with treated beneficiaries slightly less likely to have a male key provider than control (67% v. 68%). Over 90% of beneficiary-episodes have a key provider who is an MD/DO, and approximately 90% of key providers have a primary care specialty. Average key provider age at enrollment is 48 and 49 for treated and control, respectively.

Table 1 provides additional details on key providers at the individual provider level. The full sample in this table represents all providers linked to an NPI that have an "individual" entity type code, and who are ever recorded as treating beneficiaries (of any eligibility category, in managed care or traditional Medicaid) in the "Other" (outpatient, emergency department, and miscellaneous claims) MAX files in the six states in my sample. Of the 226,504 unique providers that I identify from the Other Files, 15,376 are included in my study sample as key providers. Of these, 875 are flagged as exiters, and 15,107 are identified as non-exiters.[5] Exiters are slightly more likely to be male, though this difference is insignificant. Over 90% of key providers are MD/DOs, with approximately 8-9% being NP/PAs, and the vast majority

[5]Note that these numbers will not add up to the total, since a provider can be an exiting key provider in one plan and/or in one beneficiary's enrollment episode, and a non-exiter in another plan or enrollment episode.

have a primary care specialty.[6] Average age at the time of the beneficiary's enrollment is 48.[7] For most characteristics, the relatively small values of t-statistics indicates that the exiting key providers are not significantly different from the non-exiting key providers, based on observable features. The t-statistics indicate that the differences in the share of key providers who are MD/DOs or NP/PAs, as well as the share of MD/DOs who are PCPs, is significant, though the actual magnitude of the difference (one percentage point for all three) suggests these differences are not economically meaningful. Additionally, while the t-statistic associated with the difference in average key provider age indicates that this difference is significant, the difference in years is less than one.

Table 1: Description of Beneficiary Characteristics

Beneficiary Characteristic	Full Sample	(SD)	Study Sample	(SD)	Treated	(SD)	Control	(SD)	T-stat
Share Male	.27	(.44)	.21	(.41)	.24	(.43)	.21	(.41)	4.55
Share White	.56	(.5)	.56	(.5)	.56	(.5)	.56	(.5)	-.62
Mean Age at Enrollment	32	(10)	34	(10)	35	(11)	34	(10)	5.21
Share Chronic Condition	.12	(.32)	.25	(.43)	.3	(.46)	.25	(.43)	6.89
Share CHF	.01	(.08)	.01	(.11)	.02	(.13)	.01	(.11)	2.47
Share COPD	.02	(.13)	.04	(.18)	.05	(.22)	.03	(.18)	5.02
Share Diabetes	.04	(.2)	.1	(.29)	.12	(.32)	.09	(.29)	4.77
Share Hypertension	.08	(.27)	.18	(.39)	.22	(.41)	.18	(.39)	5.49
Mean Months per Episode	16	(15)	30	(12)	37	(13)	30	(12)	41.93
Median Num. of Episodes	2	(1.2)	1	(.78)	1	(.68)	1	(.78)	-6.59
Share Metropolitan			.89	(.31)	.87	(.34)	.89	(.31)	-4.84
Share Micropolitan			.07	(.25)	.08	(.27)	.07	(.25)	2.02
Share Small/Rural			.04	(.2)	.06	(.23)	.04	(.2)	5.1
Share Male KP			.68	(.47)	.67	(.47)	.68	(.47)	-.59
Share OP Encounter			.76	(.22)	.75	(.22)	.76	(.22)	-1.83
Share MD/DO KP			.93	(.25)	.92	(.27)	.93	(.25)	-1.8
Share PCP			.89	(.31)	.9	(.3)	.89	(.31)	1.65
Share NP/PA KP			.07	(.25)	.08	(.27)	.07	(.25)	1.8
Share PCP			.9	(.3)	.91	(.29)	.9	(.3)	.59
Mean KP Age at Enrollment			49	(9.5)	48	(10)	49	(9.4)	-5.23
Total Beneficiary-Episodes	2928685		105548		4061		101487		
Total Beneficiaries	1837044		103969		4061		99962		

Characteristics are calculated at the unique beneficiary-episode of enrollment level. Full Sample represents all beneficiaries from the sample states who were ever in a comprehensive managed care plan. No key providers were assigned to the Full Sample. Study Sample represents all beneficiaries in the final study sample (Treated and Control). The Treated and Control columns provide information on beneficiaries whose key provider exited in a given episode of enrollment, and whose key provider did not exit, respectively. 'Share Chronic Condition' represents the share of beneficiaries that had evidence of at least one of the four chronic conditions of interest. T-statistics are calculated by regressing the characteristic on a treated indicator, and represent the significance of the difference between Treated and Control.

[6] Notably, due to scope of practice laws and incident-to billing practices that allow (or require) advanced practice nurses to use a supervising physician's NPI to bill for a claim, the estimated frequency of NP/PAs may undercount the true prevalence of key providers that are advanced practice nurses. Being able to accurately identify advanced practice nurses in claims data is a challenge that deserves additional consideration and research, though is outside the scope of this paper.

[7] I calculate average age at enrollment for the full sample by using the median year of enrollment (2010).

Table 2: Description of Key Provider Characteristics

Key Provider Characteristic	Full Sample	(SD)	Study Sample	(SD)	Exiters	(SD)	Non-Exiters	(SD)	T-stat
Share Male	.6	(.49)	.65	(.48)	.66	(.48)	.65	(.48)	.12
Share MD/DO	.68	(.47)	.92	(.27)	.91	(.28)	.92	(.27)	-2.4
Share PCP	.39	(.49)	.87	(.34)	.88	(.32)	.87	(.34)	2.71
Share NP/PA	.14	(.34)	.08	(.27)	.09	(.28)	.08	(.27)	2.4
Share PCP	.31	(.46)	.9	(.3)	.91	(.29)	.9	(.3)	.22
Mean Age at Enrollment	44	(10)	48	(9.6)	48	(10)	48	(9.5)	-2.86
Share OP Encounter			.7	(.21)	.7	(.21)	.7	(.21)	.16
Total	226504		15376		875		15107		

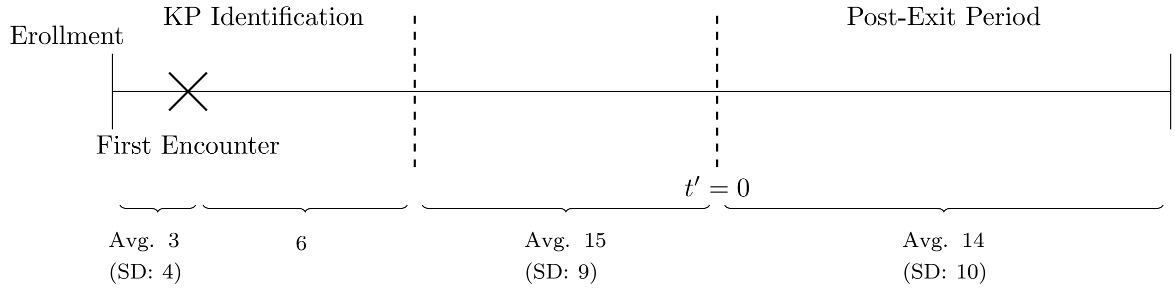
Characteristics are calculated at the unique NPI level. Full Sample represents all NPIs in the provider network cohort, which consists of any individual provider with a non-missing NPI who was recorded in the OT files as treating a Medicaid beneficiary (of any eligibility, in any type of plan) in one of the sample states during the study period. Study Sample represents all key providers in the final study sample. The Exiters and Non-Exiters columns provide information on key providers who exited in a beneficiary's given episode of enrollment (i.e. in the Treated sample), and those who did not exit (i.e. in the Control sample), respectively. Age at enrollment for the Full Sample is calculated using the median year of enrollment in the Full Sample (2010). Totals from the Exiters and Non-Exiters column will not add to the total in the Study Sample due to providers being counted as exiters and non-exiters, dependent on beneficiary and episode. T-statistics are calculated by regressing the characteristic on a treated indicator, and represent the significance of the difference between Exiters and Non-Exiters.

Appendix Tables A.4 and A.5 provide descriptive details on the length of time between key events in the study period, and Figure 1 plots the study timeline with average months between the key events. Beneficiaries are enrolled for an average of 3 months prior to their first encounter, and an average of 3 months prior to their first encounter with the clinician that will become their key provider. On average, there are 15 months in the pre-exit period (following the 6-month key provider identification period), and beneficiaries remain enrolled for 14 additional months following their key provider's exit. Prior to their key provider's exit, treated beneficiaries have encounters with any provider (not just their key provider) every 28 days on average (median: 14 days). The average length of time between their final encounter with their key provider and their next encounter (with another provider) is 105 days (median: 46), though 706 (17%) have no more encounters following their key provider's exit (Table A.6). Notably, the last encounter a beneficiary has with their key provider is on average 8 months prior to the key provider's exit date (median: 6 months), with 77% of final key provider encounters taking place within 12 months of the exit. This is similar to the average 8 months between the last time a beneficiary sees their key provider in any setting (not just an encounter) and their exit date (median: 6 months). On average, there are 73 days between the actual exit date of the key provider and a beneficiary's next encounter (median: 34 days), which is over twice the length of time between encounters in the pre-exit period (though still within one standard deviation of the pre-exit encounter time length). This suggests that while a patient's relationship with their key provider may have been truncated prior to the actual exit date, this exit date still has a meaningful impact on the beneficiary's primary care utilization; I test for this more rigorously in the event study and pre-post estimations below. Following their key provider's exit, 45% of beneficiaries have their next encounter with a provider who they had previously seen, suggesting some degree of continuity of the care team treating the patient.

6.2. Estimates of the Effect of Exits for the Main Study Sample

Figure 2 plots the θ_q's estimated from Equation 1. I observe little evidence of pre-trends, supported by the magnitude of the F statistics obtained from estimating my model on the pre-period, and that are associated with testing the null hypothesis that all pre-exit coefficients are 0 (Appendix Table A.7). While some of the point estimates in the pre-period (primarily, $t' \in [-10, -6]$) for EM visits and encounters are negative, suggesting that treated enrollees may have relatively fewer visits during this time than control enrollees, these estimates are generally statistically indistinguishable from 0, as indicated by the 95% confidence intervals, and relatively small in magnitude compared to the estimated effects of

Figure 1: Study Timeline, In Months



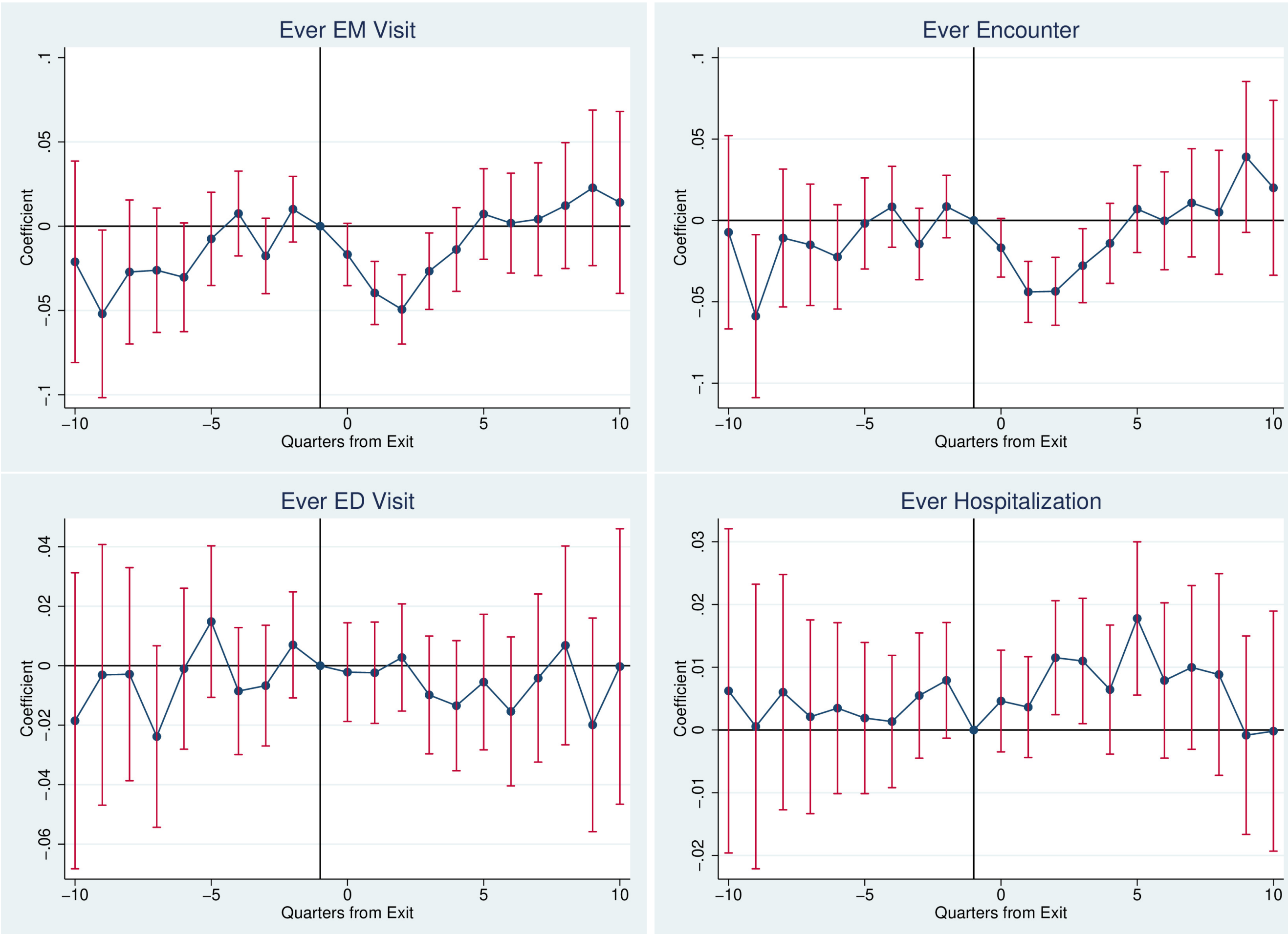
The length of time between enrollment and first encounter is reported as the average for all (treated and control) enrollees. The length of time in the pre-exit and post-exit periods is reported for the treated enrollees.

a provider's exit on the probability of a primary care visit in the post-period (with the exception of $t' = -9$, which is comparable in magnitude to these effects, and significantly different from 0 at the 5% level). Notably, given the unbalanced panel, there are substantially fewer treated beneficiaries in the quarters farther from the switch, which contributes to the decreasing precision with which these point estimates are measured. Furthermore, any difference in trends between the treated and control cohort in the pre-period is largely in the opposite direction (i.e. increasing) of the difference in post-exit trends (which are decreasing). However, during the four quarters before and after the exit (the pre and post period used in the pre-post estimation), utilization differences between treated and control cohorts in the pre-exit quarters much more closely approximate zero. Using a pre-post specification that includes quarters $t' \in [-10, -1]$ in the pre-period, instead of limiting the pre-period to $t' \in [-4, -1]$, I estimate largely equivalent coefficients associated with the post-exit indicator for all outcomes, as well as comparable levels of significance, as reported in Appendix Table B.14.

In the exit quarter ($t' = 0$), there is a marginally significant decrease of approximately 2 percentage-points for both EM visits and encounters (-1.7 and -1.7, respectively). Notably, due to my inability to identify the exact date of exit, a beneficiary may still be able to access her key provider during this quarter. In the first and second quarters following the exit quarter, there is between a 4 to 5 percentage point decrease in the share of beneficiaries who have an EM visit (-4.0 and -4.9, respectively) or encounter (-4.4 and -4.4, respectively). Relative to pre-exit rates of utilization (reported in Table 3), this translates to a decrease of between 5.8% and 7.3% of beneficiaries who ever have an EM visit or encounter following a key provider's exit. With a treated sample of 4,061, that is approximately 161 and 201 fewer beneficiaries who have an EM visit in the first and second full quarters after a key provider's exit, respectively. In the third quarter, the difference in visits between the treated and control cohorts begins decreasing to 2.7 percentage points and 1.3 percentage points below the reference period for EM visits and encounters, respectively. By the fourth post-exit quarter, utilization trends return to be in parallel to beneficiaries without an exit. Thus, while significant and meaningful, the impact of a key provider's exit on access to primary care appears to be most important in the short-term. Estimating the pre-post model in Equation 2, I find an average decrease of approximately 5.4% in the number of beneficiaries who have an EM visit in any quarter in the year following a key provider's exit, and a decrease in the number of beneficiaries who have an encounter post-exit of a similar magnitude (5.6%) (Table 3, Panel A).

Figure 2 exhibits some significant increases in all-cause hospitalizations around the time of the exit, though it is not clear that we can interpret these coefficients as meaningful. Indeed, the pre-post estimates in Table 3 (Panel A) reports no significant change in hospitalizations in the post-exit period for the study sample. Similarly, I observe no effect on ED utilization. In Appendix Figure A.1, I report no change in testing or urgent care use following a key provider's exit, and some evidence of an increase in FQHC utilization, though interpreted based on a very low pre-exit mean of less than 1% of beneficiaries visiting an FQHC in a given quarter, it's not clear that this effect is meaningful.

Figure 2: Event Study, Study Sample



This figure plots the θ_q's from Equation 1. Clockwise, the plots show the difference in the share of beneficiaries with at least one EM visit, an encounter, an ED visit, or a hospitalization between beneficiaries whose key provider exited in $t' = 0$ and those whose key provider doesn't exit, in a given quarter relative to the quarter prior to the exit ($t' = -1$).

6.3. Estimates of the Effect of Exits for the Chronic Condition subsample

Beneficiaries with chronic conditions in particular are encouraged to have regular primary care visits (though specific recommendations of frequency vary by patient); thus, a decrease in the number of chronic condition beneficiaries who have an EM visit or encounter may be particularly concerning for patient wellbeing. Figure 3 plots the θ_q's estimated on the chronic condition subsample from Equation 1. Both the generally non-significant pre-exit coefficients and a joint F-test (Appendix Table A.8) indicate no evidence of pre-trends for primary care utilization measures. The share of beneficiaries with an EM visit decreases by 5.6 and 7.2 percentage points in quarters $t' = 1$ and $t' = 2$, respectively, and the share of beneficiaries with an encounter decreases by 4.1 and 4.9 percentage points, respectively. Relative to baseline pre-exit means (given in Table 3, Panel B), these percentage-point changes represent decreases of between 5% to 9% in the number of beneficiaries with EM visits or encounters, larger in terms of percentage points and, in some quarters, percentages than the percentage decreases observed for the pooled study sample. Panel B in Table 3 reports the estimates of Equation 3 for the chronic condition subsample. As might be expected in a population who is encouraged to engage in regular primary and preventive care check-ups, the means indicate that pre-exit primary care utilization is higher for beneficiaries with chronic conditions, compared to those without. The share of beneficiaries with a chronic condition who ever have an EM visit in a given quarter decreases by approximately 5.3% following their key provider's exit, which is slightly smaller in percent terms than the decrease in the share of beneficiaries without a chronic condition (5.4%), though larger in terms of

Table 3: Pre-Post

	(1) Ever EM Visit	(2) Ever Encounter	(3) Ever ED Visit	(4) Ever Hospitalization
Panel A. Study Sample				
Post Exit	-0.037***	-0.038***	-0.004	0.004
	(0.007)	(0.007)	(0.006)	(0.003)
Observations	702113	702113	702113	702113
Adjusted R^2	0.279	0.291	0.228	0.066
Pre-Exit Mean	.679	.683	.212	.036
Panel B. Chronic Condition Subsample				
No Chronic Condition	-0.034***	-0.041***	-0.008	-0.004
	(0.009)	(0.009)	(0.007)	(0.003)
Chronic Condition	-0.042***	-0.032**	0.005	0.021***
	(0.011)	(0.012)	(0.010)	(0.005)
Observations	702113	702113	702113	702113
Adjusted R^2	0.279	0.291	0.228	0.066
Pre-Exit Mean, No Chronic	.63	.636	.202	.032
Pre-Exit Mean, Chronic	.794	.793	.235	.045

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

percentage points (4.2 versus 3.4, respectively). Similarly, there is a 5.0% and 4.5% decrease in the number of beneficiaries who have an encounter in the post-exit period among beneficiaries with and without chronic conditions, respectively. Given the convention of using EM visits as a measure of primary care utilization, I interpret these findings as suggestive of a larger reduction of primary care visits among beneficiaries with a chronic condition (in terms of percentage points), while overall outpatient encounters appear to be more greatly affected among the non-chronic condition population.

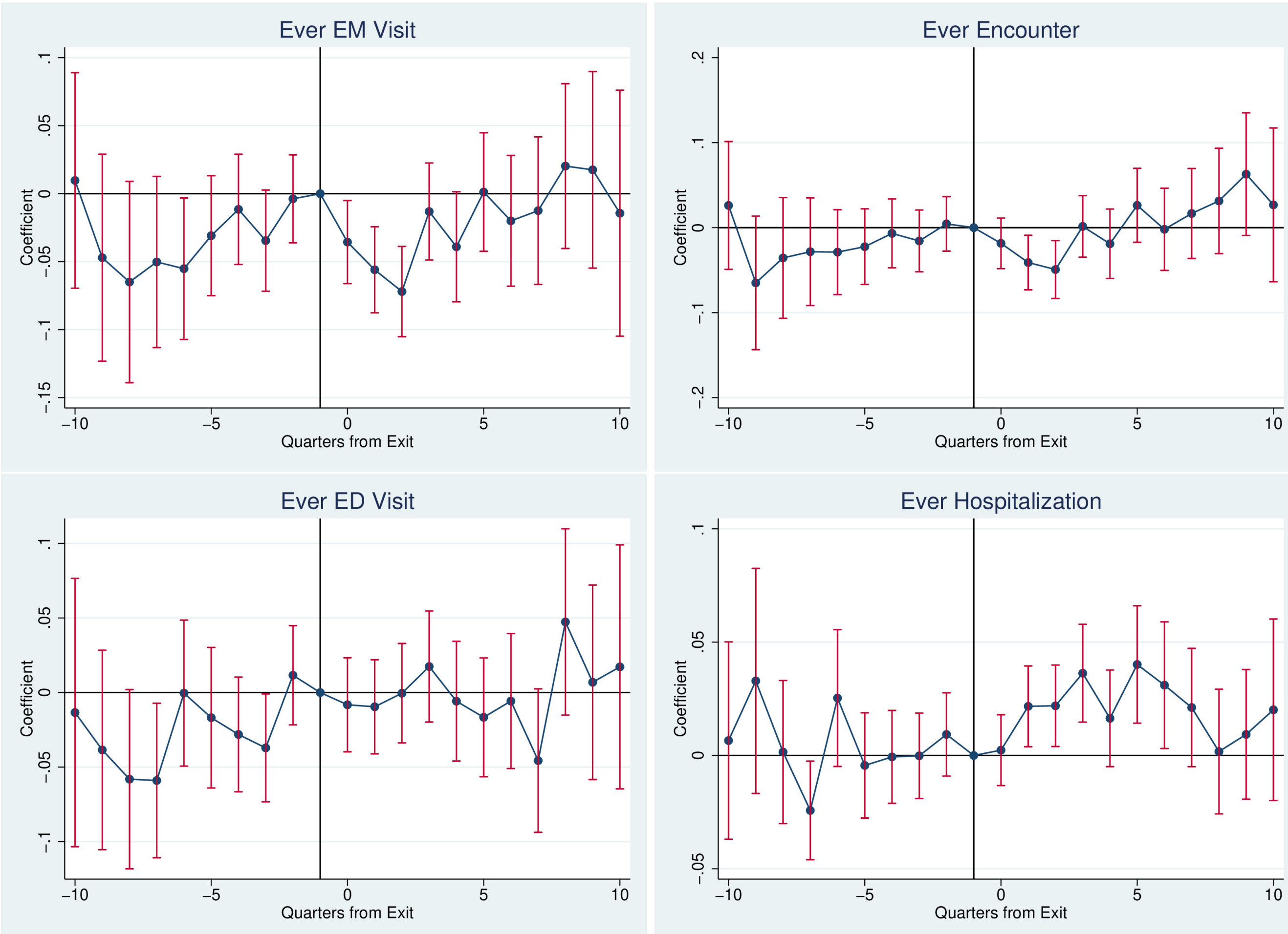
While there is no clear change in ED visits around the exit, the share of beneficiaries who are hospitalized increases significantly by between 2 to 4 percentage points in several post-exit quarters (1 through 6, except for $t' = 4$).[8] Table 3 corroborates these observations. Speaking to concerns of the particular sensitivity to disrupted care among populations with chronic conditions, I find a post-exit increase of approximately 47% (significant at the 0.1% level) in the number of beneficiaries with a chronic condition who ever have a hospitalization in a post-exit quarter, from 4.5% of beneficiaries in the pre-exit period to approximately 6.6% of beneficiaries in the post-exit period. In my sample size of 1,209 treated beneficiaries with a chronic condition in the study sample, this amounts to approximately 25 additional beneficiaries per quarter who have a hospitalization following the exit, or 102 additional hospitalized beneficiaries in the post-exit year. While these level effects are relatively small, they still represent potentially avoidable costs, both financially and in terms of health, to an already exceptionally vulnerable population. Furthermore, my approach only captures one type of (readily observable) disrupted care in a snapshot of six states, and thus represents a lower bound of the number of beneficiaries with chronic conditions whose likelihood of hospitalization increases after disruption.

6.4. Estimates of the Effect of Exits by Increasing Continuity of Care

I estimate heterogeneous treatment effects among quartiles of the share of encounters for which a key provider was responsible in the initial 6-month identification periods. As shown for the chronic condition subsample in Figure 4 (and for the full study sample in Figure A.2), heterogeneous effects exist along this dimension, and largely in the expected

[8]Notably, Table A.8 reports a marginally significant pre-trend for ED visits, with an F-statistic of 1.995. The F-statistic associated with the pre-trend test for hospitalizations is 1.862, which, while not significant at conventional levels of 5% in terms of p-values, is significant at the 10% level. However, the event study plot of hospitalizations in Figure 3 does not necessarily suggest any systematic pre-trends.

Figure 3: Event Study, Chronic Condition subsample

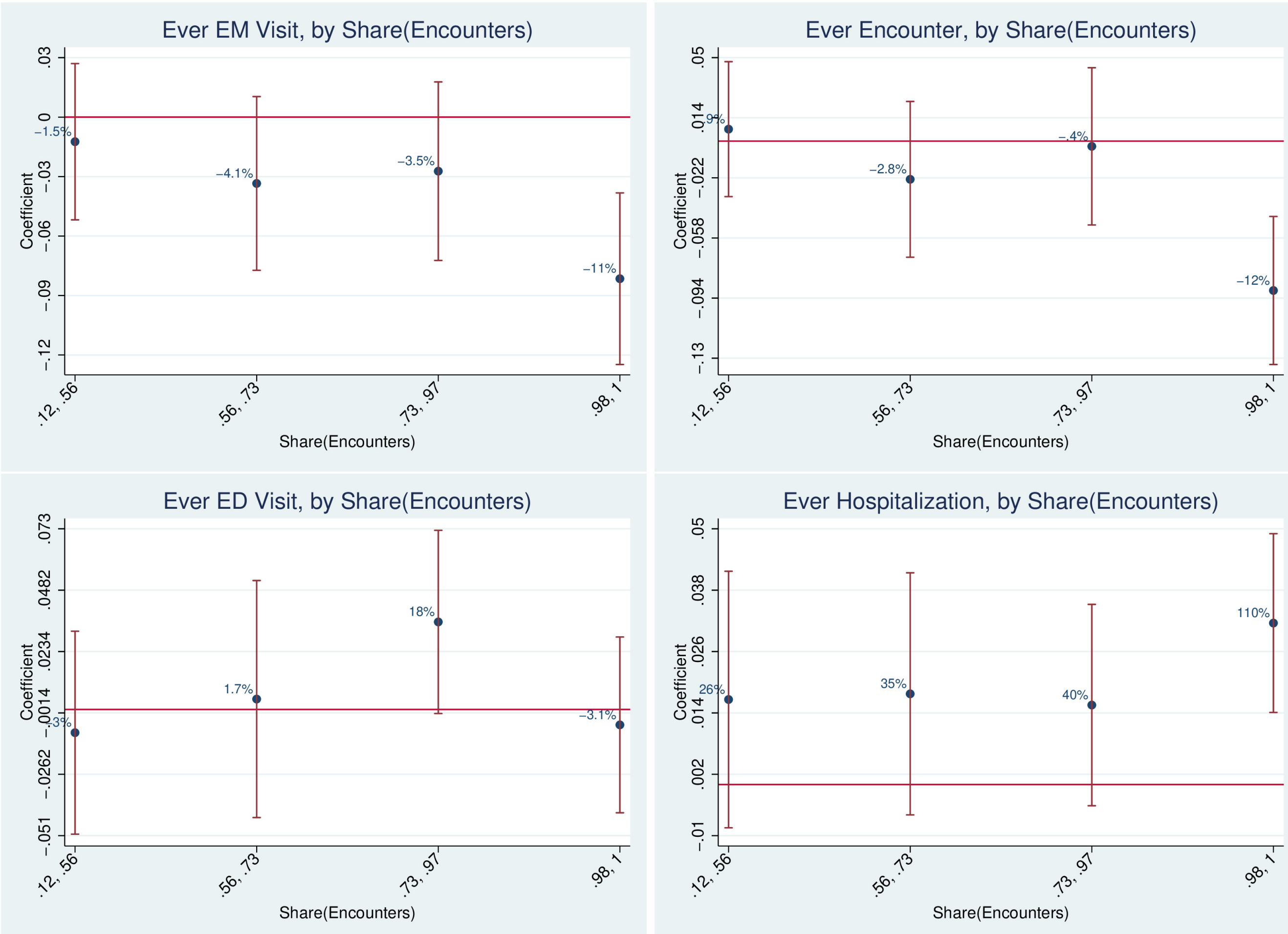


This figure plots the θ_q's estimated on the chronic condition subsample from Equation 1. Clockwise, the plots show the difference in the share of beneficiaries with at least one EM visit, an encounter, an ED visit, or a hospitalization between beneficiaries whose key provider exited in $t' = 0$ and those whose key provider doesn't exit, in a given quarter relative to the quarter prior to the exit ($t' = -1$).

direction, such that the effect of a disrupted relationship increases as share of encounters increases. Specifically, the effect of an exit on EM visits and encounters is most pronounced in the fourth quartile of share of encounters. Similarly, the effect of an exit on hospitalizations in this cohort is positive for all quartiles, but largest (and significant) in the fourth quartile. After a Holm-Bonferroni correction for Type I errors due to multiple comparisons, the effect in the fourth quartile remains significant at the 1% level. These observations are consistent with the belief that enrollees with the highest degree of care continuity are most vulnerable to disruptions.

In this case, Figure 4 would suggest that the significant effect of a key provider's exit on hospitalizations observed in Table 3 is largely being driven by beneficiaries whose key provider is responsible for more than 98% of encounters (fourth quartile). Notably, testing whether the coefficients for hospitalizations in the third and fourth quartiles are significantly different returns an F-statistic of 1.42. This indicates that the effects between quartiles may not actually be significantly different from each other, and suggests that the relatively smaller sample size in the third quartile may result in less precise estimates that obscure significance. Thus, a generalized interpretation of the results given by both the study sample and chronic condition subsample is that increased continuity with one's provider increases one's sensitivity to disruptions. Appendix Tables A.9—A.12 report the underlying data, including the pre-exit means and number of beneficiaries associated with each threshold.

Figure 4: Coefficients from Pre-Post by Share of Encounter Thresholds, Chronic Condition subsample



This figure plots the δ_2's obtained from estimating Equation 3 by quartiles of the share of encounters associated with the key provider during the 6-month key provider identification period. Clockwise, the plots show the difference, by quartile of share of encounters, in the share of beneficiaries with at least one EM visit, an encounter, a hospitalization, or an ED visit (per quarter) between beneficiaries whose key provider exited in $t' = 0$ and those whose key provider doesn't exit, in any of the four quarters following the exit (the post-period) relative to the four quarters prior to the exit (the pre-period), for beneficiaries with a chronic condition.

6.5. Estimates of the Effect of Exits for the 12-Month Key Provider Cohort

The estimated effects of a provider's exit on primary care utilization, ED visits, and hospitalizations estimated for the 12-month cohort are remarkably similar to the effects found in the main cohort in terms of magnitude and significance, which is reassuring regarding the (lack of) sensitivity of my results to the key provider definition period. Notably, the coefficients in the 6-month cohort are generally more precisely estimated than in the 12-month cohort, suggesting that the slightly larger sample size captured in the 6-month specification served to increase statistical power. In the study sample associated with the 12-month cohort, I estimate an average decrease of 4.6% in the number of beneficiaries per quarter who have an EM visit in the year following a key provider's exit, and a decrease of 4.7% of the number of beneficiaries per quarter who have an encounter post-exit (Panel A in Table 4), similar in terms of both percentage point change and percent change estimated in these two outcomes in the main cohort.

When comparing effects of provider exit on those with and without a chronic condition, the number of beneficiaries who ever have an EM visit in a given quarter decreases by approximately 5.9% among beneficiaries with a chronic condition, and 3.9% for those without (Panel B). Similarly, there is a 5.1% and 4.5% decrease in the number of beneficiaries who have an encounter in the post-exit period among beneficiaries with and without chronic conditions, respectively. I find a post-exit increase of approximately 28% (significant at the 5% level) in the number of beneficiaries with a chronic condition

Table 4: Pre-Post, 12-Month Key Provider Cohort

	(1) Ever EM Visit	(2) Ever Encounter	(3) Ever ED Visit	(4) Ever Hospitalization
Panel A. Study Sample				
Post Exit	-0.030***	-0.031***	-0.003	0.002
	(0.007)	(0.007)	(0.006)	(0.003)
Observations	645338	645338	645338	645338
Adjusted R^2	0.270	0.283	0.222	0.069
Pre-Exit Mean	.657	.659	.194	.03
Panel B. Chronic Condition Subsample				
No Chronic Condition	-0.024**	-0.028**	-0.001	-0.002
	(0.009)	(0.009)	(0.007)	(0.003)
Chronic Condition	-0.045***	-0.039**	-0.010	0.011*
	(0.012)	(0.013)	(0.010)	(0.005)
Observations	645338	645338	645338	645338
Adjusted R^2	0.270	0.283	0.222	0.069
Pre-Exit Mean, No Chronic	.612	.616	.183	.027
Pre-Exit Mean, Chronic	.767	.768	.223	.038

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

who ever have a hospitalization in a post-exit quarter, from 3.8% of beneficiaries in the pre-exit period to approximately 4.9% of beneficiaries in the post-exit period. Notably, this effect is estimated less precisely than in the main cohort. Given the similarity in sample characteristics and poi9nt estimates of the effects between the two cohorts, I attribute this decreased precision as a lack of power due to smaller sample size.

I plot the θ_q's estimated in the event study specifications for the study sample and chronic condition subsample associated with this cohort in Figures A.3 and A.4. As with the pre-post estimations, these figures indicate that the effect of a key provider's exit among enrollees in the 12-month cohort is very similar to that in the 6-month cohort, though less precisely estimated.

6.6. Additional Robustness Checks

I perform several checks for the robustness of my results. The first responds to the observation in Section 3 that nearly one fifth of beneficiaries left their plan during an enrollment episode (and were subsequently dropped from my sample). During my study period, some states (such as Indiana) had policies that allowed beneficiaries to follow their PCP across plans outside of open enrollment period. I can't rule out the possibility that the beneficiaries who switched plans within an enrollment episode were following a preferred PCP, and that beneficiaries who remained were neutral or negative about their exiting provider. I estimate my model on the cohort of beneficiaries whose key provider exits entirely from Medicaid (and not just their managed care plan) in order to assess the robustness of my results to provider neutrality or antipathy. Approximately half (52%) of the exits I observe occur because a key provider exits Medicaid entirely. I find similar results to my main specification; I observe a decrease of 6% and 8% of the share of beneficiaries in a given quarter who ever have an EM visit or encounter, respectively, following a key provider's exit from Medicaid (Appendix Table B.15), and no effect on ED visits or hospitalizations. For beneficiaries with a chronic condition, I observe a decrease in EM visits and encounters that is similar in magnitude to the main results (6.8% and 5.3%, respectively), and no effect on emergency department visits. Finally, the effect of an exit on hospitalizations is smaller (a 1.4 percentage point increase) and insignificant at the 5% level for beneficiaries with a chronic condition.

I also estimate Equations 2 and 3 with standard errors clustered at the key provider level instead of the beneficiary level, to determine if my results are robust to serial correlation across key providers. While the standard errors are slightly larger, as expected, there is no change in the magnitude, nor the overall significance, of the coefficients, with the exception that the effect of a provider's exit on the share of chronic condition enrollees who ever have an encounter is now significant at the 10% level, instead of the 1% level. (Appendix Tables B.16).[9]

I next estimate Equations 1 and 2 using only the treated cohort. This is possible in a pre-post evaluation where the shock (a key provider's exit) occurs at different calendar times. In this scenario, beneficiaries act as both treated and control (to other beneficiaries), dependent on the timing relative to their own exit. Appendix Figure J.15 plots provider exits per calendar quarter to show the relatively flat distribution of exits across time. While including a separate (never exit) control cohort increases precision of the estimated calendar time effects, there is a risk that, due to my potential under-identification of provider exits, I incorrectly include beneficiaries whose key provider does exit their plan in the control cohort. Thus, the treated-only specification provides an additional check on the effects I estimate in my main specification, i.e. that they are not significant underestimates of the effect of an exit due to mis-identifying a control group. Additionally, if lower-quality providers are more likely to exit, comparing treated beneficiaries to other treated beneficiaries will make the control group more similar in terms of the quality of the key provider.

Estimating the event study and pre-post models on a treated-only subset of the sample returns results that are very similar to my treated and control specification. Plots of the θ_qs from the event study describe a similar effect of a key provider's exit on outcomes of interest, except that pre-exit utilization trends in the direction of the post-exit decrease in a somewhat concerning pre-trend (Appendix Figure B.5). However, a significant drop in visits occurring around the exit is still visible. There are no clear effects on hospitalizations or ED visits. In the pre-post specification, I observe a decrease of slightly more than 5 percentage points in the number of beneficiaries per quarter who ever have an EM visit or encounter in the post-exit period (Appendix Table B.17); the percentage-point decrease for EM visits is larger for beneficiaries with a chronic condition (6 percentage points), though slightly smaller for encounters (5 percentage points). As in the effects estimated on the main sample, hospitalizations among beneficiaries with a chronic condition increase by 2.1 percentage points. These robustness checks suggest that my main results are largely stable to the inclusion of a control cohort that may include individuals who have also experienced an exit.

Finally, I estimate Equations 2 and 3 using controls for beneficiary and key provider characteristics instead of beneficiary-episode fixed effects. This specification allows me to relax the more restrictive beneficiary-episode controls that may absorb relevant variation in outcomes based on observable characteristics of the sample. Beneficiary controls include an indicator for whether the beneficiary is treated, a set‘ of patient characteristics comprised of indicators for gender, race (white or not white), age in decile group, and ever diagnosed with any of the four chronic conditions of interest. Due to the missingness of certain demographic variables—namely, sex and race—the model with controls is estimated on a slightly smaller sample than the primary specifications (there are 3,166 treated and 88,653 control enrollee-episodes in this sample). Key provider controls include indicators for gender, age in decile group, an indicator for whether the key provider is a PCP or not, and an indicator for whether the provider is an MDDO or not. I include calendar fixed effects and fixed effects for relative time, as above. Finally, I include fixed effects for the CMC plan that the beneficiary is enrolled in throughout the episode, as well as fixed effects for the HRR of residence (i.e. where all treatment takes place, by design of my sample construction). Estimates of the effect of a key provider's exit on the outcomes of interest are very similar to my preferred estimation with beneficiary-episode fixed effects (Appendix Table B.18).

7. Discussion

In this study, I find that the truncation of a relationship between a CMC enrollee and the provider responsible for the plurality of their care results in a significant, short-term reduction in primary care visits in the year following the exit, and that this effect is larger for standard measures of primary care utilization in terms of percentage points among

[9]There are 481 unique exiting key providers who treat the subsample of treated patients with a chronic condition, and each key provider treats 2.5 (SD: 3.1) of these patients on average.

beneficiaries with chronic conditions. Furthermore, I observe a nearly 50% increase in the number of beneficiaries with chronic conditions that ever have a hospitalization in the year following their key provider's exit. Finally, I observe that these effects increase with the share of encounters a key provider was associated with, which is consistent with other studies that have found an association between increasing continuity of relationships and reduced risk of hospitalization and other adverse events among medically complex or chronic disease patients (Hussey et al., 2014; Bayliss et al., 2015).

One way to interpret these results is to consider a back-of-the-envelope cost-benefit analysis of the tradeoff between reduced primary care visits and increased hospitalizations. My estimates imply that for one hundred beneficiaries with chronic conditions, a key provider's exit will lead to a reduction in four beneficiaries with an EM visit and two additional beneficiaries with a hospitalization. In terms of costs, the reduction in EM visits represents savings of approximately \$460, offset by an increase in hospitalization spending of approximately \$12,394.[10] Thus, the costs of disruption via hospitalizations outweigh any savings by nearly 30 times the magnitude of the reduced EM visits.

This study has several limitations. First, I infer a key provider's exit using a conservative approach based on observations of their treatment of beneficiaries in the claims data. While the conservative nature of this estimate most likely limits measurement error in the form of incorrectly identifying an exit, this approach almost certainly undercounts the number of providers that exit a plan. Compared to prior estimates of an annual PCP turnover rate of 12% in Medicaid managed care plans in a select group of states, I find an annual turnover rate (from exits) of approximately 1%. However, given my robustness check using the treated-only cohort, it's not immediately clear whether this likely underestimation of exits will translate to an underestimation of the effect of an exit on outcomes of interest. A somewhat related concern is my conservative structuring of the study sample, which requires that enrollees must be continuously enrolled for at least one quarter following their key provider's exit. My effect sizes thus do not reflect the impact of a provider's exit on beneficiaries who drop out of Medicaid following this disruption. To explore the robustness of my estimates to the inclusion of "dropouts," I perform an imputation exercise, details of which are given in Appendix F, and find that estimating my model on a sample that includes beneficiaries who exit Medicaid or their plan after their key provider exits returns estimated effect sizes that are slightly larger than the sizes I estimate in my study sample, but generally within the 95% confidence interval of the estimates I report in my main analysis.[11]

A second potential limitation of this study arises from the quality concerns of the MAX data: if the encounters in managed care are mis-represented in any systematic way (i.e., under or over counted), my estimated effect of a disruption to care will be subject to measurement error and potentially biased. For example, if there is systematic under-reporting of EM visits among beneficiaries after their key provider exits, then I will have over-estimated the impact of a disruption on utilization of primary care. The likelihood of these results being biased from measurement error is mitigated by two factors. First, reimbursement to the plan is not tied to the reporting of this data (payment for enrollee care is determined through a separate process and set of forms), and thus there does not appear to be any incentive for plans to systematically mis-report claims in either direction. Second, I use guidance provided by validated reports on the data's integrity to select a subsample of states that are deemed to have encounter data of acceptably high quality. In general, ensuring the availability of up-to-date, high-quality, validated claims data for Medicaid managed care enrollees should continue to be a priority for policymakers.

A third potential limitation is the small size of the treated cohort relate to the full sample (0.22% of the full sample). This fact may raise concerns that the beneficiaries experiencing a key provider's exit are not representative of the general Medicaid population in ways that challenge the external validity of the estimated effect of an exit. For example, if continuous enrollment requirements are responsible for the precipitous drop in beneficiaries, then we might be concerned that the study sample does not represent the general Medicaid population in terms of average enrollment tenure (which

[10]Alexander and Schnell (2019) find an average price associated with a relatively common EM visit code (99203) of approximately \$115 in 2013/2014. Using a combination of charged amounts and estimated discount rates on charges in the MAX data, I estimate that the median cost of a hospitalization for a patient with one of the chronic conditions of interest is \$6,197 (25^{th} percentile: \$3,087; 75^{th} percentile: \$16,352)

[11]I thank an anonymous reviewer for this recommendation.

would be substantiated given evidence of churn into and out of the program, as discussed in Section 1). Alternatively, we might be concerned that the three-encounter minimum requirement for identifying a key provider within the six months post-enrollment would include only beneficiaries with a relatively engaged approach to outpatient care, such as beneficiaries with a chronic condition. Indeed, Table 2 indicates that beneficiaries in the study sample are twice as likely to have one of the four chronic conditions of interest (though, as noted in Section 3, prior studies have observed rates of chronic conditions that are more in line with this study sample than with the full sample). While these are reasonable concerns that merit consideration in the interpretation of the results, Table A.13, which reports beneficiary and beneficiary-episode counts by each restriction imposed, suggests that that most limiting step in terms of dropping beneficiaries occurs in the identification of a key provider's exit. Indeed, as noted above, my intentionally conservative approach to identifying provider exits likely significantly undercounts the true prevalence of provider exits. However, for the reasons discussed above, it is not clear that this undercount would meaningfully bias the results.

At the time of analysis, I used the most up-to-date Medicaid administrative claims data available, which only includes the new Medicaid expansion population in the final year of data (2014). Recent studies have demonstrated that adults newly eligible via Medicaid expansion were both more likely to have previously undiagnosed chronic conditions detected through their new coverage, and (conditional on having a chronic condition) more likely to directly benefit from increased access to primary/preventive care provided by the expansion (Rosland et al., 2019; Saloner, 2017; Winkelman and Chang, 2017). With rising chronic condition prevalence among new enrollees, my findings suggest that we should be increasingly concerned about the effects of disrupted care among Medicaid enrollees, especially with respect to the impact on primary care utilization and hospitalizations. Further research using more updated data is necessary to fully capture the effect of disruptions in this population.

Medicaid enrollees represent our nation's most economically and socially vulnerable citizens, with high rates of chronic conditions and medically complex needs. My findings suggest that loss of access to a continuous or trusted source of care may prove especially challenging and costly for this patient population. Additionally, these differences highlight the need for additional research on the Medicaid population, and suggest caution in extrapolating insights from other settings with relatively more resources, such as Medicare. These findings also have important implications for Medicaid policies that seek to expand access to care. For example, policies aimed at re-allocating patients more equitably across plans and providers (for example, to relieve physician burden, as in a policy enacted by a southeastern state in 2014 (Piwnica-Worms et al., 2021)) may introduce disruptions to relationships that could result in reduced access and increased adverse health events for certain cohorts of patients, such as those with chronic conditions. Indeed, managed care plan exits from the Medicaid market are another potential source of disrupted care to Medicaid enrollees, and their impact on healthcare utilization and outcomes merits further exploration.[12]

My results, which isolate the effect of provider exit while holding constant everything else about a healthcare delivery setting (including plan), suggest that relational continuity between patients and providers is particularly important for medically complex patients, such as those with chronic conditions, and that policymakers should consider costs of disruptions to this population in crafting policies surrounding care delivery and organization. While these findings are informative, there is substantial need for further investigation into other dimensions of continuity in access and care in this population. Additional exploration can build on the results presented in this paper to develop our understanding of the

[12]In the states in my study sample, I observe 8 managed care organizations (20% of all managed care organizations) and 45 plan products (approximately 40% of all plan products) exiting at some point during the study period. "Plan products" refers to the one of multiple products that a larger managed care organization may offer to Medicaid enrollees that can differ along several dimensions, such as cost sharing, regional coverage, and plan design elements. Recent literature suggests that exploring the effects of these organization and/or plan product exits on patient utilization and outcomes deserves additional attention. Ndumele et al. (2017) documented a high (nearly 30%) rate of comprehensive managed care plan exit from the Medicaid market between 2006 and 2014, though found very little effect on the quality of care received or patient experience of providers. Piwnica-Worms et al. (2021) exploited a policy in one southeastern state that redistributed patients to new plans to estimate the effect of a disruption in plan continuity (through forced redistribution) on care utilization and health outcomes among a highly vulnerable population of children in Medicaid who had been diagnosed with pediatric asthma, finding small but significant decreases in general office visits after the reallocation relative to a non-switching control group.

institutional and clinical features (or lack thereof) that reconnect enrollees to care following their provider's exit, and how the availability of other providers impacts the probability of reconnecting to care. Additionally, a deeper understanding of the characteristics of patient-provider relationships that most effectively engage enrollees in care and help them avoid adverse health outcomes would generate important insights that could be used to aid enrollees in strategically selecting providers at the time of enrollment (or after a provider's exit). Finally, continuity in care has multiple dimensions in addition to the inter-personal component between patient and provider. To more comprehensively understand the implications for disrupted care in the health of vulnerable populations, and in particular how this might exacerbate existing disparities in healthcare access and health outcomes, future research should consider loss of access to regular sites of care, which could have many sources (such as clinic closures or forced migration due to neighborhood gentrification), in addition to plan exits from Medicaid.

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Appendix A Additional Tables and Figures

A.1 Tables

Table A.1: Frequency of Women with Restricted-Benefits Pregnancy Flag

	Pregnancy Flag	Percent	Total
Treated	93	2.29%	4,061
Control	4,346	4.35%	99,962
Total	4,439	4.27%	103,969

Table A.2: Share(Encounters) Associated with Key Provider, Treated Beneficiaries

Population	Mean	SD	10th Pctile	Median	75th Pctile	90th Pctile	Beneficiary-Quarters
Pooled	.75	.22	.44	.75	1	1	4061
Chronic Conditions	.74	.22	.43	.75	1	1	1209

This table shows the distribution of the share of encounters associated with a treated beneficiary's key provider in the 6-month key provider identification period.

Table A.3: Outcomes in the Pre-Exit Period, Study Sample, Treated Beneficiaries

Outcome	Mean	SD	Ever (Mean)	SD	Median	75th Pctile	90th Pctile	Beneficiary-Quarters
EM Visit	1.88	2.1	.68	.47	1	3	5	11446
Encounter	2.21	2.7	.68	.47	1	3	6	11446
ED Visit	.35	.88	.21	.41	0	0	1	11446
Hospitalization	.04	.22	.036	.19	0	0	0	11446
Tests	.97	1.5	.48	.5	0	1	3	11446
FQHC	.004	.1	.003	.05	0	0	0	11446
Urgent Care	.006	.09	.005	.07	0	0	0	11446
Ever Fill Date	.62	.48	.62	.48	1	1	1	9257
Num. Drugs	2.46	3.2	.62	.48	1	4	7	9257
Num. Fill Dates	4.3	6.2	.62	.48	2	6	13	9257
Num. Fill Dates per Drug	1.04	.97	.62	.48	1	1.75	2.33	9257

This table shows the distribution of the outcomes of interest for the Treated sample in the pre-exit period (t'<0), calculated at the beneficiary-episode-quarter level. Outcomes are calculated as a binary variable, with 0 indicating that the event never occurred, and 1 indicating that it occurred at least once. Prescription drug outcomes represent records from AZ, KY, NM, WA, and any beneficiary in NJ with an enrollment episode start date after 2009. Statistics that would represent cells with less than 11 observations have been suppressed, indicated by <.001 for all prescription-related measures, and <.001 for all other measures.

Table A.4: Timeline of Key Events, In Months

Measure	Mean	SD	25th Pctile	50th Pctile	75th Pctile	90th Pctile	Num. Benes
Enrollment Length in CMCO	30	12	21	27	36	48	105548
Enrollment to First Encounter	3	4	0	1	3	6	105548
Enrollment to First Visit with KP	3	4	1	2	4	7	105548
Pre-Exit Period	15	9	8	12	20	28	4061
Enrollment to KP Exit	23	10	16	20	29	36	4061
KP Exit to End of Enrollment	14	10	7	12	20	27	4061
First Visit with KP to Exit	20	9	13	18	25	33	4061
Last Visit with KP to Exit	9	8	2	6	12	20	4061
First Visit with KP to Last Visit with KP	14	12	5	11	20	29	105548

Table A.5: Time Between Events for Treated Beneficiaries

Measure	Mean	SD	25th %tile	Median	75th %tile	N
Days between pre-exit encounters	28	46	6	14	30	79536
Days between last visit with KP and next encounter	105	149	15	46	131	3819
Days between KP exit and next encounter	73	103	12	34	89	3439
Days between last visit with KP and exit	263	252	74	191	363	4061
Days between last encounter with KP and exit	270	252	80	202	376	4061
Months between last visit with KP and exit	8	8	2	6	11	4061
Months between last encounter with KP and exit	8	8	2	6	12	4061

Table A.6: Encounters Pre and Post Exit

Measure	Count	Total Beneficiaries	Share
Any visit with KP within 12 months of exit	3157	4061	0.77
Encounter with KP within 12 months of exit	3119	4061	0.77
No encounters post exit	706	4061	0.17

Table A.7: Evidence of Pre-Trends, Main Outcomes

Outcome	F Stat	P value
EM Visit	1.726	.077
Encounter	1.505	.139
ED Visit	1.228	.272
Hospitalization	.942	.487

Table A.8: Evidence of Pre-Trends, Main Outcomes, Chronic Condition Subsample

Outcome	F Stat	P value
EM Visit	.724	.687
Encounter	.554	.835
ED Visit	1.995	.036
Hospitalization	1.862	.053

Table A.13: Beneficiary and Beneficiary-Episode Count by Restriction

Step	N	Pct. of Total
All beneficiaries enrolled in a CMCO between 2009-2014	1,837,044	100%
All beneficiaries with a start date after Jan. 2009	1,444,216	79%
Beneficiaries with one plan per enrollment episode	1,385,430	75%
Beneficiaries with an identifiable key provider, 6-month identification period	270,498	15%
Treated		
Beneficiaries with a key provider who exits (outside their KP identification period)	13,577	1%
Beneficiaries who remain enrolled for at least one month following the exit	12,762	1%
Beneficiary-episodes where key provider exited, excluding those who exited in April or May of 2012 in AZ	6,209	0.34%
Beneficiary-episodes that meet the three-quarter enrollment requirements around the exit	4,061	0.22%
Control		
Beneficiaries with a key provider who doesn't exit	265,874	14%
Beneficiaries who remain enrolled for at least one month following the KP identification period	234,146	13%
Beneficiary-episodes that meet the three-quarter enrollment requirements following the key provider identification period	101,487	6%

Table A.9: Pre-Post by Share(Encounters), Study Sample

	EM Visit				Encounter					
Share	Mean	Coef.	SE	Pct. Change	Mean	Coef.	SE	Pct. Change	Bene. (N)	Share Range
1	.74	-.02	(.01)	-2.7%	.75	-.03	(.01)	-3.4%	994	(.12, .57)
2	.66	-.01	(.01)	-.9%	.67	-.01	(.01)	-.9%	1149	(.58, .75)
3	.7	-.05**	(.02)	-7.6%	.69	-.06**	(.02)	-8.4%	546	(.76, .98)
4	.64	-.07***	(.01)	-10%	.64	-.07***	(.01)	-10%	1372	(1, 1)

Means are calculated as the average per-quarter share of beneficiaries who ever have a record of the outcome of interest in the 4 quarters prior to the exit. Coefficients are from the estimates of the pre-post model described in the paper. Standard errors are in parentheses. Percent change is calculated as the percent change from the pre-exit mean represented by the coefficient. 'Bene. (N)' represents the number of beneficiaries whose key provider was associated with the corresponding share of encounters. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.10: Pre-Post by Share(Encounters), Study Sample

	Hospitalization				ED Visit					
Share	Mean	Coef.	SE	Pct. Change	Mean	Coef.	SE	Pct. Change	Bene. (N)	Share Range
1	.05	-.01	(.01)	-10%	.27	-.03*	(.01)	-11%	994	(.12, .57)
2	.03	0	(.01)	13%	.2	.01	(.01)	5.1%	1149	(.58, .75)
3	.03	.01	(.01)	18%	.21	.02	(.01)	7.8%	546	(.76, .98)
4	.03	.01*	(0)	34%	.18	-.01	(.01)	-2.9%	1372	(1, 1)

Means are calculated as the average per-quarter share of beneficiaries who ever have a record of the outcome of interest in the 4 quarters prior to the exit. Coefficients are from the estimates of the pre-post model described in the paper. Standard errors are in parentheses. Percent change is calculated as the percent change from the pre-exit mean represented by the coefficient. 'Bene. (N)' represents the number of beneficiaries whose key provider was associated with the corresponding share of encounters. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.11: Pre-Post by Share(Encounters), Chronic Condition Subsample

		EM Visit				Encounter				
Share	Mean	Coef.	SE	Pct. Change	Mean	Coef.	SE	Pct. Change	Bene. (N)	Share Range
1	.84	-.01	(.02)	-1.5%	.84	.01	(.02)	.9%	303	(.12, .56)
2	.81	-.03	(.02)	-4.1%	.81	-.02	(.02)	-2.8%	248	(.56, .73)
3	.78	-.03	(.02)	-3.5%	.77	0	(.02)	-.4%	284	(.73, .97)
4	.75	-.08***	(.02)	-11%	.76	-.09***	(.02)	-12%	374	(.98, 1)

Means are calculated as the average per-quarter share of beneficiaries who ever have a record of the outcome of interest in the 4 quarters prior to the exit. Coefficients are from the estimates of the pre-post model described in the paper. Standard errors are in parentheses. Percent change is calculated as the percent change from the pre-exit mean represented by the coefficient. 'Bene. (N)' represents the number of beneficiaries whose key provider was associated with the corresponding share of encounters. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

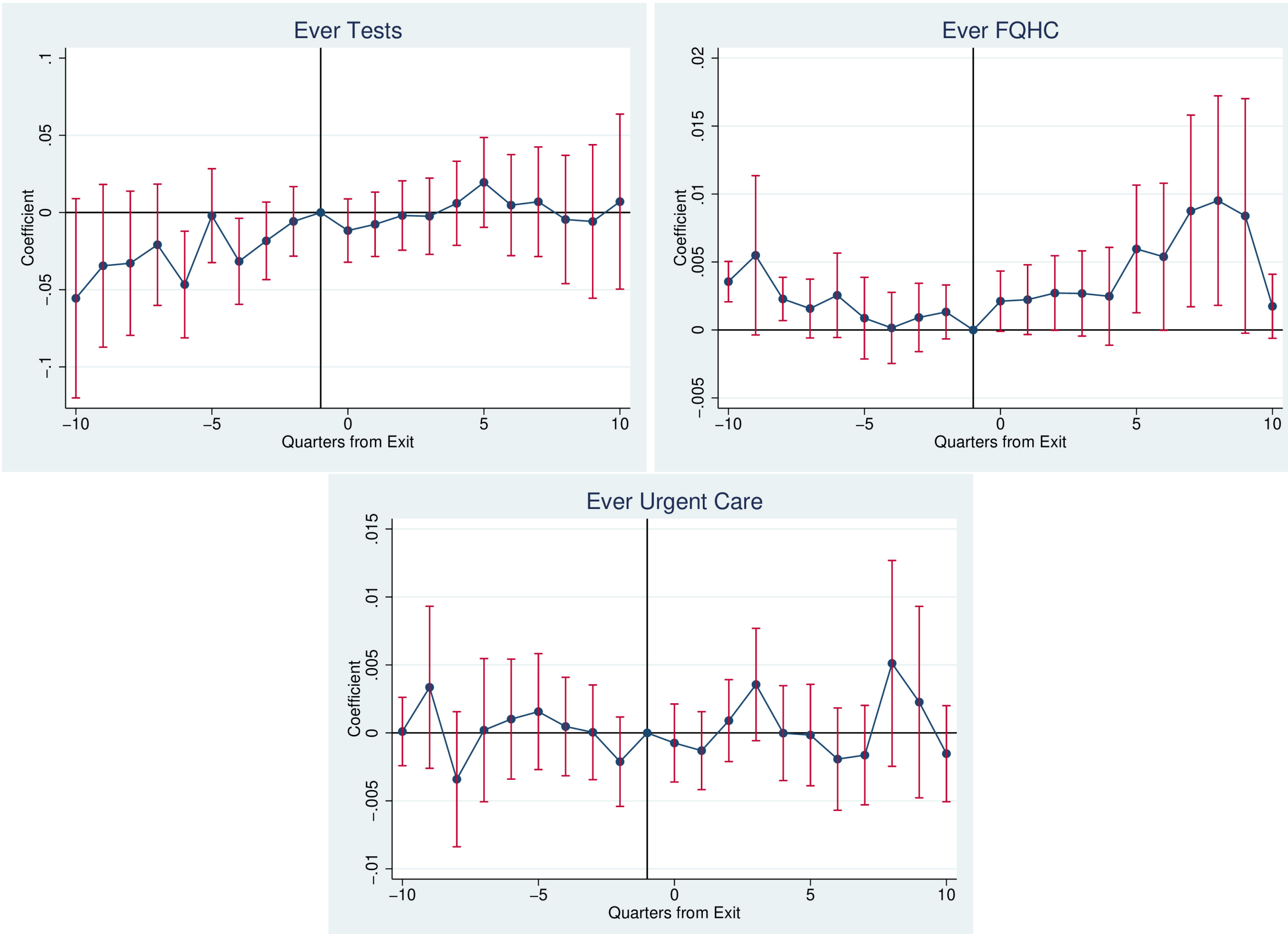
Table A.12: Pre-Post by Share(Encounters), Chronic Condition Subsample

		Hospitalization				ED Visit				
Share	Mean	Coef.	SE	Pct. Change	Mean	Coef.	SE	Pct. Change	Bene. (N)	Share Range
1	.06	.02	(.01)	26%	.31	-.01	(.02)	-3%	303	(.12, .56)
2	.05	.02	(.01)	35%	.24	0	(.02)	1.7%	248	(.56, .73)
3	.04	.02	(.01)	40%	.19	.04	(.02)	18%	284	(.73, .97)
4	.03	.03***	(.01)	110%	.2	-.01	(.02)	-3.1%	374	(.98, 1)

Means are calculated as the average per-quarter share of beneficiaries who ever have a record of the outcome of interest in the 4 quarters prior to the exit. Coefficients are from the estimates of the pre-post model described in the paper. Standard errors are in parentheses. Percent change is calculated as the percent change from the pre-exit mean represented by the coefficient. 'Bene. (N)' represents the number of beneficiaries whose key provider was associated with the corresponding share of encounters. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

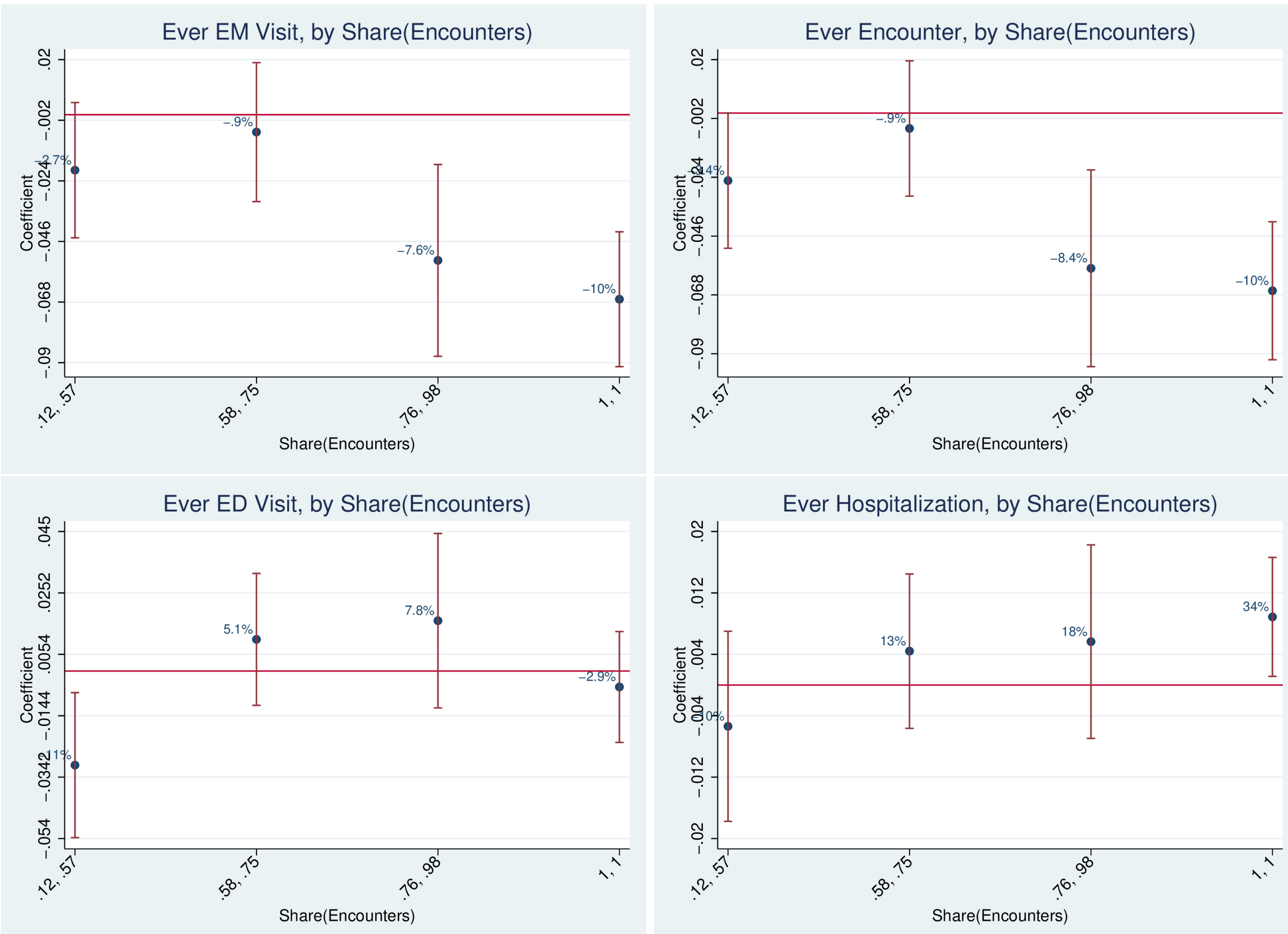
A.2 Figures

Figure A.1: Event Study, Study Sample, Additional Outcomes of Interest



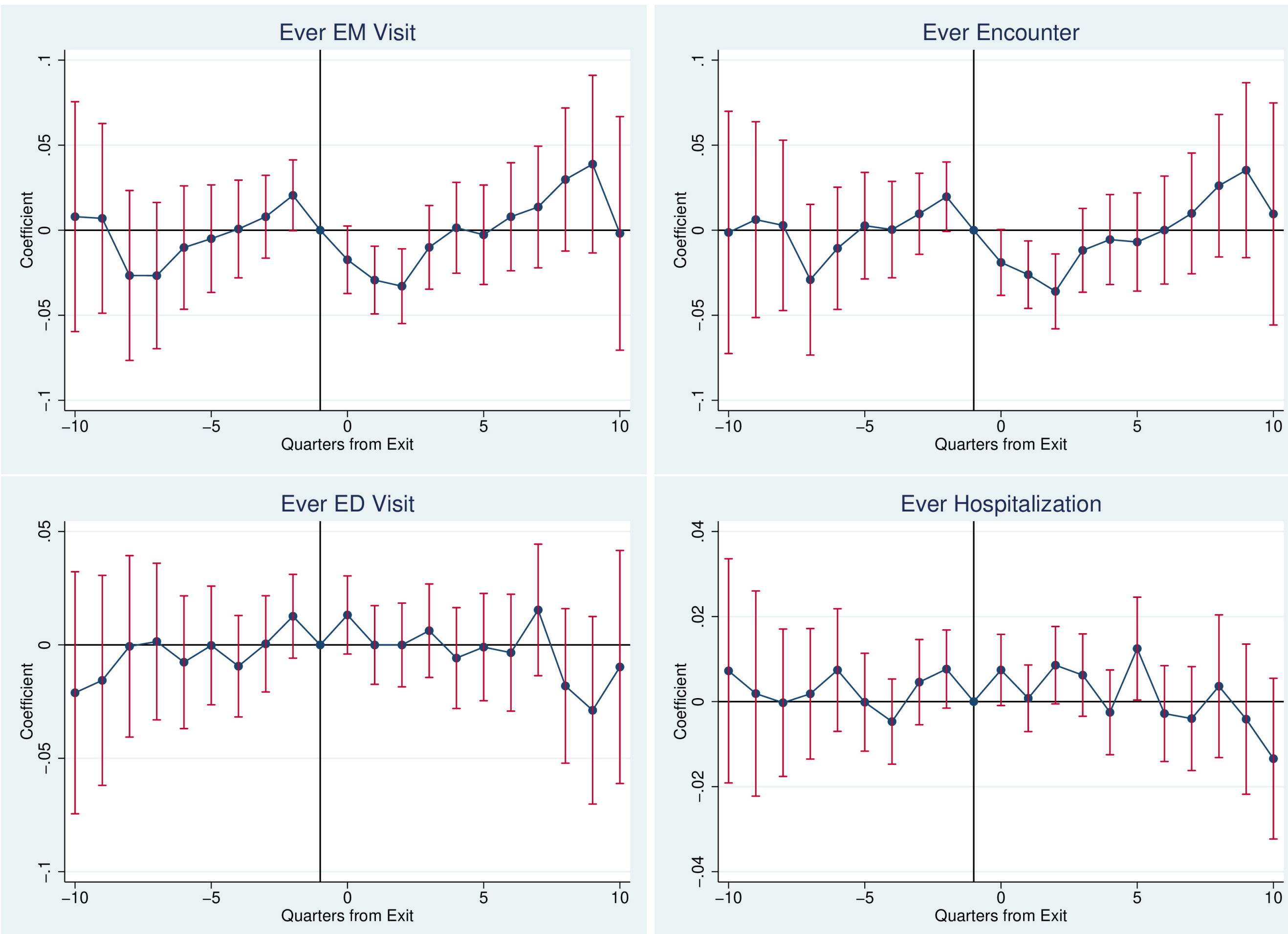
This figure plots the θ_q's from Equation 1. Clockwise, the plots show the difference in the share of beneficiaries with at least one test, at least one FQHC visit, or at least one urgent care visit between beneficiaries whose key provider exited in $t' = 0$ and those whose key provider doesn't exit, in a given quarter relative to the quarter prior to the exit ($t' = -1$).

Figure A.2: Coefficients from Pre-Post by Share of Encounter Quartiles, Study Sample



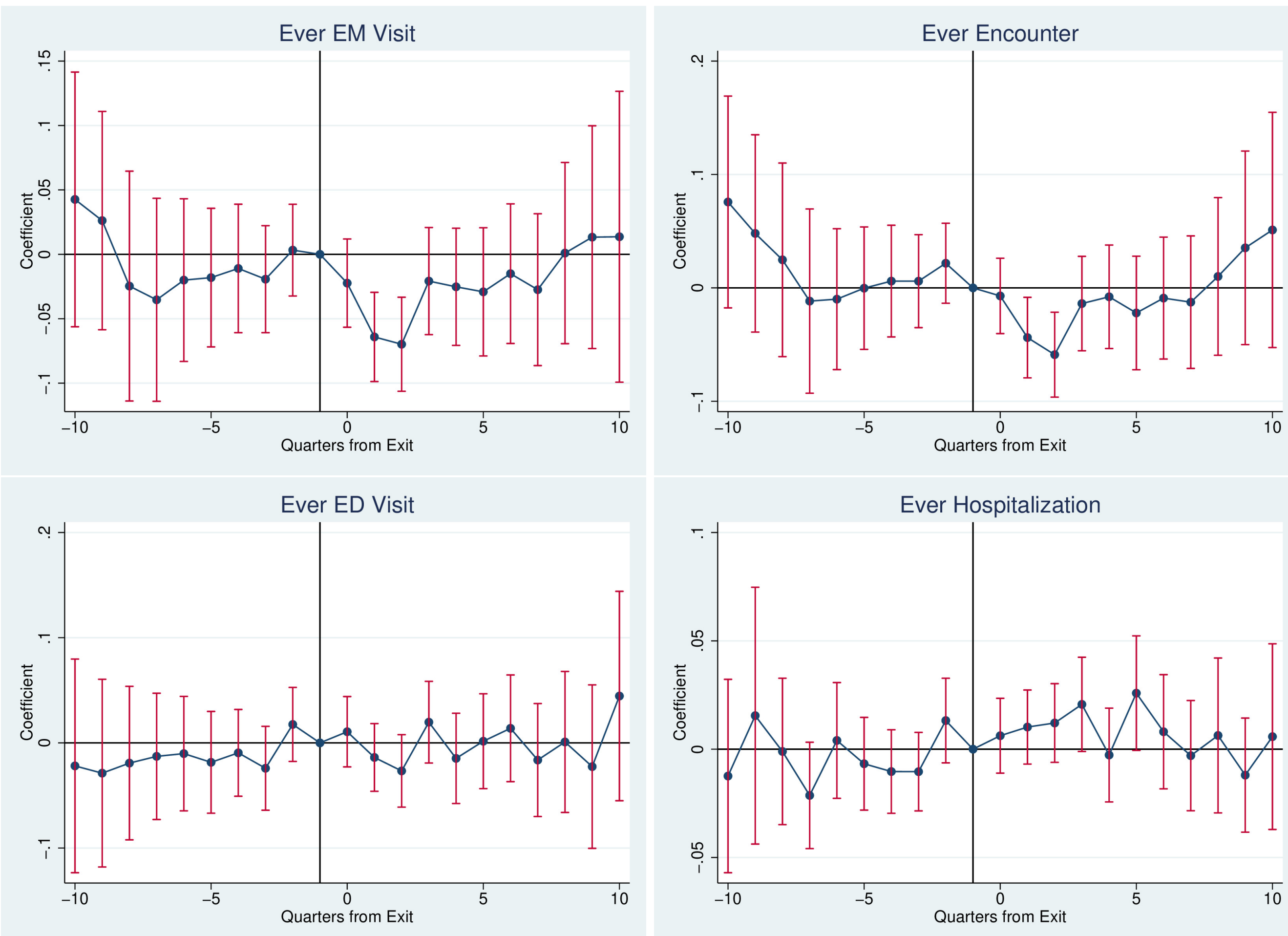
This figure plots the δ_2's obtained from estimating Equation 3 by quartiles of the share of encounters associated with the key provider during the 6-month key provider identification period. Clockwise, the plots show the difference, by quartile of share of encounters, in the share of beneficiaries with at least one EM visit, an encounter, an ED visit, or a hospitalization (per quarter) between beneficiaries whose key provider exited in $t' = 0$ and those whose key provider doesn't exit, in any of the four quarters following the exit (the post-period) relative to the four quarters prior to the exit (the pre-period), for all beneficiaries in the study sample.

Figure A.3: Event Study, Study Sample, 12-Month Cohort



This figure plots the θ_q's from Equation 1 for the 12-month cohort. Clockwise, the plots show the difference in the share of beneficiaries with at least one EM visit, an encounter, an ED visit, or a hospitalization between beneficiaries whose key provider exited in $t' = 0$ and those whose key provider doesn't exit, in a given quarter relative to the quarter prior to the exit ($t' = -1$).

Figure A.4: Event Study, Chronic Condition subsample, 12-Month Cohort



This figure plots the θ_q's estimated on the chronic condition subsample from Equation 1 for the 12-month cohort. Clockwise, the plots show the difference in the share of beneficiaries with at least one EM visit, an encounter, an ED visit, or a hospitalization between beneficiaries whose key provider exited in $t' = 0$ and those whose key provider doesn't exit, in a given quarter relative to the quarter prior to the exit ($t' = -1$).

Appendix B Robustness Checks

Table B.14: Pre-Post, Study Sample, Alternative Pre-Period

	(1) Ever EM Visit	(2) Ever Encounter	(3) Ever ED Visit	(4) Ever Hospitalization
Post Exit	-0.033***	-0.035***	-0.005	0.004
	(0.007)	(0.007)	(0.005)	(0.003)
Observations	706491	706491	706491	706491
Adjusted R^2	0.278	0.291	0.227	0.066
Pre-Exit Mean	.679	.686	.208	.036

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table B.15: Pre-Post, Medicaid Exit

	(1) Ever EM Visit	(2) Ever Encounter	(3) Ever ED Visit	(4) Ever Hospitalization
Panel A. Study Sample				
Post Exit	-0.039***	-0.052***	-0.015	0.000
	(0.010)	(0.010)	(0.008)	(0.004)
Observations	690894	690894	690894	690894
Adjusted R^2	0.278	0.291	0.227	0.066
Pre-Exit Mean	.648	.652	.229	.036
Panel B. Chronic Condition Subsample				
No Chronic Condition	-0.035**	-0.056***	-0.017	-0.005
	(0.011)	(0.012)	(0.009)	(0.004)
Chronic Condition	-0.053**	-0.041*	-0.008	0.014
	(0.018)	(0.019)	(0.017)	(0.009)
Observations	690894	690894	690894	690894
Adjusted R^2	0.278	0.291	0.227	0.066
Pre-Exit Mean, No Chronic	.604	.612	.219	.032
Pre-Exit Mean, Chronic	.778	.769	.259	.047

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table B.16: Pre-Post, Cluster at KP Level

	(1) Ever EM Visit	(2) Ever Encounter	(3) Ever ED Visit	(4) Ever Hospitalization
Panel A. Study Sample				
Post Exit	-0.037***	-0.038***	-0.004	0.004
	(0.008)	(0.010)	(0.006)	(0.003)
Observations	702113	702113	702113	702113
Adjusted R^2	0.279	0.291	0.228	0.066
Pre-Exit Mean	.679	.683	.212	.036
Panel B. Chronic Condition Subsample				
No Chronic Condition	-0.034***	-0.041***	-0.008	-0.004
	(0.009)	(0.010)	(0.007)	(0.004)
Chronic Condition	-0.042**	-0.032	0.005	0.021***
	(0.015)	(0.017)	(0.010)	(0.006)
Observations	702113	702113	702113	702113
Adjusted R^2	0.279	0.291	0.228	0.066
Pre-Exit Mean, No Chronic	.63	.636	.202	.032
Pre-Exit Mean, Chronic	.794	.793	.235	.045

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table B.17: Pre-Post, Treated Only

	(1) Ever EM Visit	(2) Ever Encounter	(3) Ever ED Visit	(4) Ever Hospitalization
Panel A. Study Sample				
Post Exit	-0.053***	-0.056***	0.002	0.003
	(0.014)	(0.014)	(0.012)	(0.006)
Observations	23275	23275	23275	23275
Adjusted R^2	0.297	0.306	0.237	0.079
Pre-Exit Mean	.679	.683	.212	.036
Panel B. Chronic Condition Subsample				
No Chronic Condition	-0.049***	-0.059***	-0.002	-0.004
	(0.014)	(0.014)	(0.012)	(0.006)
Chronic Condition	-0.060***	-0.050**	0.011	0.021**
	(0.017)	(0.017)	(0.015)	(0.008)
Observations	23275	23275	23275	23275
Adjusted R^2	0.297	0.306	0.237	0.080
Pre-Exit Mean, No Chronic	.63	.636	.202	.032
Pre-Exit Mean, Chronic	.794	.793	.235	.045

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

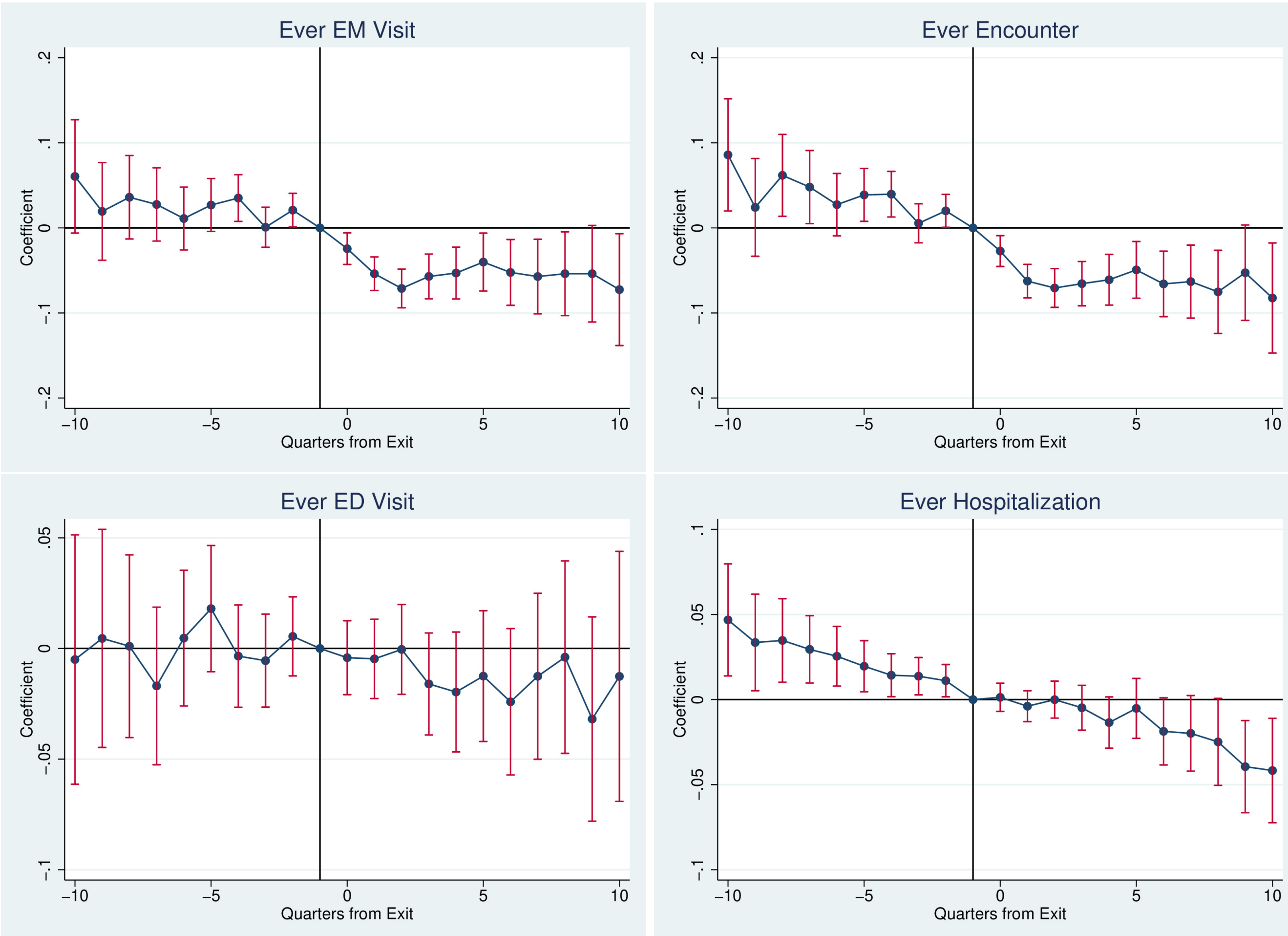
Table B.18: Pre-Post, With Controls

	(1) Ever EM Visit	(2) Ever Encounter	(3) Ever ED Visit	(4) Ever Hospitalization
Panel A. Study Sample				
Post Exit	-0.050***	-0.051***	-0.001	0.002
	(0.010)	(0.011)	(0.005)	(0.003)
Exit	0.024**	0.015	0.013*	0.000
	(0.008)	(0.009)	(0.006)	(0.002)
Observations	606551	606551	606551	606551
Adjusted R^2	0.043	0.046	0.033	0.012
Pre-Exit Mean	.693	.697	.212	.04
Panel B. Chronic Condition Subsample				
No Chronic Condition	-0.054***	-0.056***	-0.007	-0.004
	(0.011)	(0.013)	(0.005)	(0.003)
Chronic Condition	-0.043***	-0.038**	0.011	0.014**
	(0.012)	(0.013)	(0.010)	(0.004)
Exit	0.024**	0.015	0.013*	0.000
	(0.008)	(0.009)	(0.006)	(0.002)
Observations	606551	606551	606551	606551
Adjusted R^2	0.043	0.046	0.033	0.012
Pre-Exit Mean, No Chronic	.642	.649	.198	.035
Pre-Exit Mean, Chronic	.803	.801	.241	.05

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Figure B.5: Event Study, Treated Beneficiaries Only, Study Sample



This figure plots the θ_q's obtained from estimating Equation 1 on only the treated beneficiaries. Clockwise, the plots show the difference in the share of beneficiaries with at least one EM visit, an encounter, an ED visit, or a hospitalization between beneficiaries whose key provider exited in $t' = 0$ and those whose key provider doesn't exit, in a given quarter relative to the quarter prior to the exit ($t' = -1$).

Appendix C Mandatory Enrollment into Managed Care

In the majority of states in my study sample, managed care for adults was mandated throughout the study period. Thus, regardless of any plan switching that might occur (either associated or unassociated with a key provider's exit), most beneficiaries would remain in managed care. Table C.19 provides details of state-specific programs and mandatory status over time. End dates are provided if they occur before the end of 2014.

Table C.19: Mandatory Managed Care for Adults by State

State	Program	Adult Populations Enrolled	Mandatory?	Start Date	End Date
AZ	Arizona health Care Cost Containment System	Low-income adults	Mandatory	Oct. 1982	
IN	Hoosier Healthwise	Poverty-level pregnant women, section 1931 adults only	Mandatory	Jan. 2008	
	Hoosier Healthwise	Presumptively eligible pregnant women	Mandatory	Mar. 2011	
	Healthy Indiana Plan	Low-income adults	Mandatory	Jan. 2008	
KY	KY Health Care Partnership Program	Low-Income Adults	Mandatory	Nov. 1997	
	Medicaid Managed Care Program	Low-income adults	Mandatory	2011	
NJ	NJ Family Care	Low-income adults	Mandatory	<2010	
NM	Salud!	Low-income adults	Mandatory	Jul. 1997	Dec. 2013
	State Coverage Initiative (Section 1115 Demonstration)	Childless low-income adults	Voluntary	Jan. 2010	Dec. 2013
	Centennial Care	Low-income adults	Mandatory	Jan. 2014	
WA	Healthy Options	Low-income adults	Mandatory	Jul. 1994	
	Washington Basic Health (Transitional Bridge Demonstration)	Low-income adults (<133% FPL and not Medicaid-eligible)	Mandatory	Jan. 2011	Dec. 2013
	Medical Care Services (Transitional Bridge Demonstration)	Low-income adults (<133% FPL and not Medicaid-eligible)	Mandatory	Jan. 2011	Dec. 2013

Notes: Hoosier Healthwise changed which waivers it was operating under in 2008 and 2011. By 2013, enrollment into Washington Basic Health (Transitional Bridge Demonstration) and Medical Care Services (Transitional Bridge Demonstration) appeared to be voluntary. Information in this table was collected from publicly available Medicaid managed care enrollment reports (where the earliest year available is 2010) and state-specific managed care summaries as of 2014 available from Medicaid.gov.

Appendix D 12-Month Key Provider Cohort

While my main results focus on the 6-month key provider cohort, I explore the effect of a provider's exit among the 12-month key provider cohort as an important sensitivity check. First, the 12-month cohort allows me to test the sensitivity of my results to the time period in which the exiting key provider is identified. Markedly different results across the 6-month and 12-month cohorts might suggest that the key provider time period captures different populations; in other words, enrollees that have three encounters in a 6-month time period versus a 12-month time period may be sufficiently different from each other, which may also raise concerns about the external validity of my results. For example, enrollees with more chronic conditions might also be more likely to have three encounters in the first six months of their enrollment, and may also be more sensitive to disrupted care than other non-chronic condition enrollees. I test for this explicitly by comparing beneficiary and key provider characteristics between these two cohorts, as shown in Tables D.20 and D.21, respectively. I find no meaningful differences in beneficiary or key provider characteristics between these two cohorts, except that, as expected, enrollees in the 12-month cohort have enrollment episodes that are significantly longer (by about 5 months) than the 6-month cohort. Additionally, the share of encounters associated with a key provider is approximately 6 percentage points greater for the 6-month cohort than the 12-month cohort, most likely because enrollees on average have fewer encounters in a 6-month period (mean 7.23 encounters, SD: 3.91) than in a 12-month period (mean 9.48 encounters, SD 6.06). Additionally, as reported in Tables D.22 and D.23, there is significant overlap in enrollees between the two cohorts:

60% of treated enrollees in the 12-month cohort are also treated in the 6-month cohort, and 54% of treated enrollees in the 6-month cohort are also treated in the 12-month cohort.[13]

Second, there are potential tradeoffs in sample size associated with the time periods used to identify the key provider.[14] On the one hand, there are likely to be more enrollees with at least three encounters in a 12-month period than in a 6-month period. Therefore, we might expect the 12-month cohort to capture more enrollees, thus increasing the statistical power of my study. On the other hand, given the relatively high churn rates of individuals into and out of Medicaid, requiring individuals to be enrolled for at least 12 months following their initial encounter (in addition to the requisite pre- and post-exit periods for treated enrollees) may exclude a large number of enrollees, thus limiting statistical power through a decreased sample size. Ultimately, the 6-month cohort includes an additional 352 beneficiary-episodes in which a key provider exits (and 6,385 additional control beneficiary-episodes). Based on the increased precision with which I can estimate my results, these observations do provide additional power. Though the magnitudes of the estimated effects of provider exit on outcomes of interest are reassuringly close across both cohorts, the significance for key adverse outcomes (namely, hospitalizations) differ.

Table D.20: Compare Beneficiary Characteristics Between KP Cohorts

	Treated			Control		
Beneficiary Characteristic	6-Month	12-Month	T-stat	6-Month	12-Month	T-stat
Share Male	.24	.24	-.3	.21	.22	3.32
Share Missing Sex	0	0		0	0	.05
Share White	.56	.54	-1.74	.56	.54	-9.61
Share Missing Race	.03	.03	.29	.04	.04	-.34
Mean Age at Enrollment	34.85	34.64	-.87	33.99	34.35	7.7
Share Chronic Condition	.3	.29	-1.13	.25	.26	6.37
Share CHF	.02	.02	-.53	.01	.01	1.42
Share COPD	.05	.04	-.99	.03	.04	1.82
Share Diabetes	.12	.11	-.62	.09	.1	3.21
Share Hypertension	.22	.21	-.54	.18	.19	6.33
Mean Months per Episode	37.44	42.33	17.43	29.52	35.2	110.01
Median Num. of Episodes	1.52	1.47	-3.44	1.61	1.56	-15.03
Share Metropolitan	.87	.86	-.89	.89	.9	6.68
Share Micropolitan	.08	.09	1.86	.07	.06	-4.75
Share Small/Rural	.06	.05	-.91	.04	.04	-4.5
Share Male KP	.67	.71	3.44	.68	.68	.65
Share OP Encounter	.75	.69	-12.09	.76	.69	-65.72
Share MD/DO KP	.92	.93	.82	.93	.93	3.24
Share PCP	.9	.91	1.29	.89	.9	2.46
Share NP/PA KP	.08	.07	-.82	.07	.07	-3.24
Share PCP	.91	.94	1.53	.9	.91	2.54
Mean KP Age at Enrollment	47.87	48.25	1.39	48.77	48.7	-1.63
Total Beneficiary-Episodes	4061	3709		101487	95102	

Characteristics are calculated at the unique beneficiary-episode of enrollment level. 'Share Chronic Condition' represents the share of beneficiaries that had evidence of at least one of the four chronic conditions of interest. Measures and t-statistics are calculated by regressing the characteristic on an indicator for the 12-month key provider cohort. T-statistics specifically represent the significance of the difference between the different treated key provider cohorts and different control key provider cohorts.

[13] We wouldn't necessarily expect that all enrollees in the 12-month cohort with a key provider that exits would also have an exiting key provider in the 6-month cohort, since enrollees may have different key providers identified based on the associated time period of the cohort. Thus, an enrollee in the 12-month cohort whose key provider did not exit may be found to have an exiting key provider in the 6-month cohort.

[14] I thank an anonymous reviewer for pointing out this tradeoff, and for suggesting that the additional observations associated with the 6-month cohort may provide additional statistical power, compared to a 12-month cohort.

Table D.21: Compare Key Providers Characteristics Between KP Cohorts

KP Characteristic	Exiters 6-Month	Exiters 12-Month	Exiters T-stat	Non-Exiters 6-Month	Non-Exiters 12-Month	Non-Exiters T-stat
Share Male KP	.66	.69	3.08	.65	.66	1.61
Share MD/DO KP	.91	.92	1.23	.92	.93	3.84
Share PCP	.88	.9	1.82	.87	.88	6.89
Share NP/PA KP	.09	.08	-1.23	.08	.07	-3.84
Share PCP	.91	.94	1.18	.9	.91	1.62
Mean KP Age at Enrollment	47.66	48	1.1	48.24	48.32	1.53
Share OP Encounter	.7	.65	-8.61	.7	.65	-43.63
Total	875	734		15107	14279	

Characteristics are calculated at the unique NPI level. Totals from the Exiters and Non-Exiters column will not add to the total in the Study Sample due to providers being counted as exiters and non-exiters, dependent on beneficiary and episode. Measures and t-statistics are calculated by regressing the characteristic on an indicator for the 12-month key provider cohort. T-statistics specifically represent the significance of the difference between the different Exiters key provider cohorts and different Non-Exiters key provider cohorts.

Table D.22: Compare Beneficiary-Episode Overlaps Between KP Cohorts, Treated

6-Month Cohort	12-Month Cohort No	12-Month Cohort Yes	Total
No	0	1500	1500
Yes	1852	2209	4061
Total	1852	3709	5561

Table D.23: Compare Beneficiary-Episode Overlaps Between KP Cohorts, Control

6-Month Cohort	12-Month Cohort No	12-Month Cohort Yes	Total
No	0	32933	32933
Yes	39318	62169	101487
Total	39318	95102	134420

Appendix E Balanced Panel Robustness Check

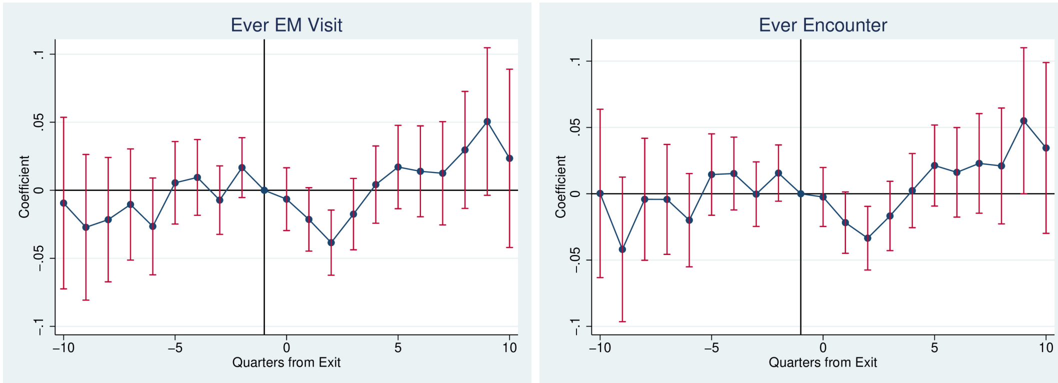
To test the robustness of my results to different degrees of balance across quarters relative to the key provider's exit, I estimate my main event study model on several subsamples of the study sample, representing increasingly restrictive balance requirements with respect to the number of quarters before and after a key provider's exit that a beneficiary must remain enrolled in their plan. Specifically, I construct treated cohorts by restricting the treated sample to those beneficiaries who remain enrolled continuously in their plan in the two, three, and four quarters before and after a key provider's exit. I mimic this restriction among controls by requiring non-switchers to remain continuously enrolled in their plan for at least five, seven, and nine quarters following the key provider definition period, corresponding to the two, three, and four balanced panel treated cohorts, respectively. Table E.24 presents counts of treated and control beneficiaries in each of the balanced panel samples resulting from these restrictions. key provider

Table E.24: Beneficiary-Episodes by Balanced Panel Cohort

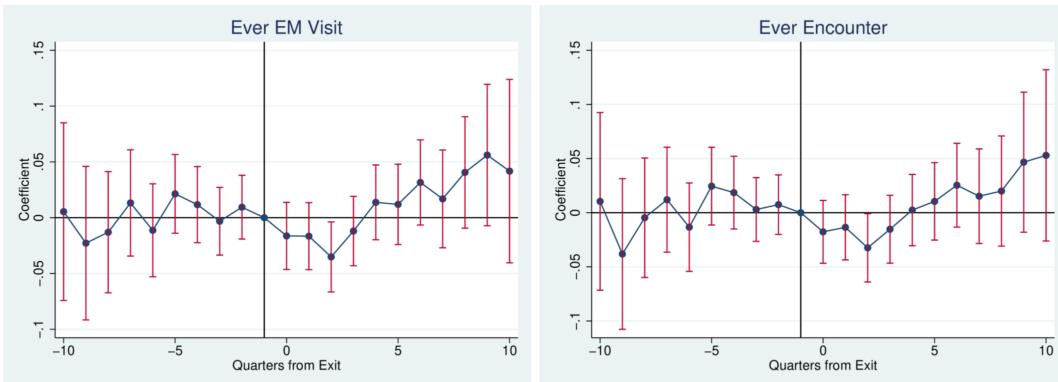
Balanced Quarters (Pre and Post)	Treated	Control
2	2550	65320
3	1470	40933
4	912	24859

Figure E.6 plots the θ_qs from estimating Equation 1 on each of the balanced panel subsamples.

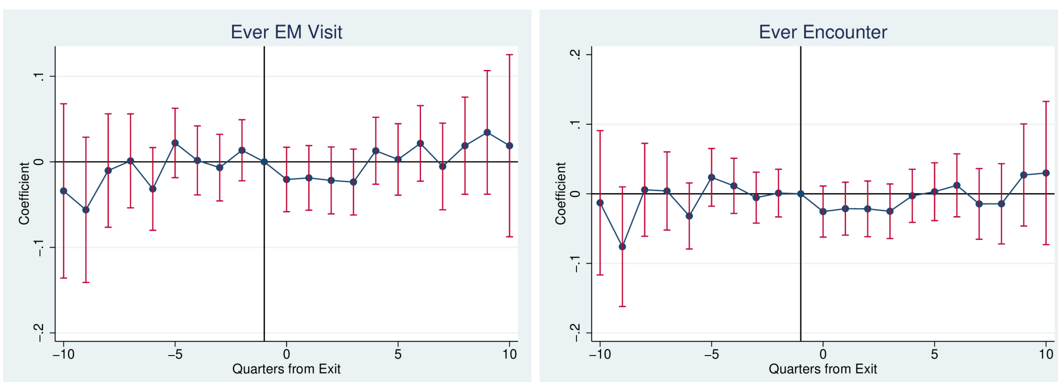
Figure E.6: Event Study by Balanced Quarter Restrictions



(a) 2 Quarters



(b) 3 Quarters



(c) 4 Quarters

This figure plots the θ_qs from Equation 1 for the study sample. Panels a, b, and c plot the coefficients obtained from estimating the model on the subset of treated beneficiaries (and relevant non-switching controls) who are enrolled in the same plan for at least two, three, and four quarters before and after the key provider's exit, respectively. The non-switching control group was constructed to mimic the balance of the treated cohorts, such that controls were included if they remained enrolled for at least five, seven, and nine consecutive quarters following the key provider identification period. The plots show the difference in the share of beneficiaries with at least one EM visit or an encounter between beneficiaries whose key provider exited in $t' = 0$ and those whose key provider doesn't exit, in a given quarter relative to the quarter prior to the exit ($t' = -1$).

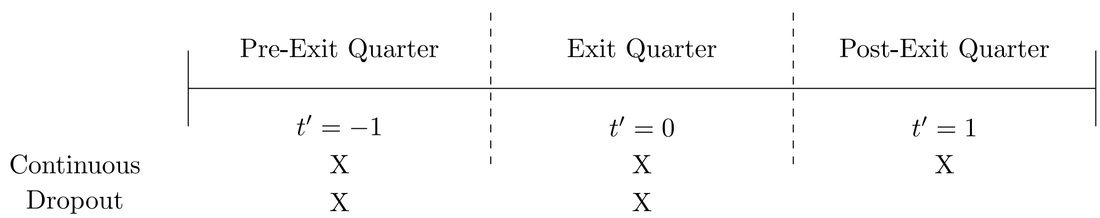
Effects (when significant) are similar to the effects I estimate in my (main) one-quarter balanced panel, such that I observe a decrease in the probability of primary care utilization following a key provider's exit from a beneficiary's managed care network.

Appendix F Exploring Bias in Excluding Beneficiary Exiters

The final sample is constructed in such a way that it excludes treated beneficiaries who do not remain in their plan for at least a quarter following their key provider's exit, and control beneficiaries who are not enrolled consistently for at least three quarters following the key provider definition period. This necessarily excludes treated beneficiaries who switch plans or who exit Medicaid entirely in the quarter following their key provider's exit. As pointed out by an anonymous reviewer, excluding these populations may introduce bias to the effects I estimate in the following way: On the one hand, my results may understate the effect of a key provider's exit on primary care utilization if beneficiaries who exit Medicaid contemporaneously with the key provider's exit have no additional primary care visits. On the other hand, my results may overstate the exit's effect on primary care utilization if beneficiaries re-enroll after exiting and regain access to primary care (not that regaining access would necessarily lead to use of primary care). To explore the extent of these biases, I perform the following exercise.

Of the beneficiaries whose key provider exits, I define a "continuous" treated cohort as those who remain in the same plan for at least a quarter after the exit; this cohort is equivalent to the treated group in my main analysis. I define a "dropout" cohort as those beneficiaries who exit Medicaid or the plan at some point during the post-exit quarter. Within the dropout cohort, I further differentiate between "full" and "churn" dropouts, where full refers to beneficiaries who exit from Medicaid entirely (i.e. who are not observed in the year following the key provider's exit), and churn refers to beneficiaries who switch plans at some point in the year following the key provider's exit (i.e. who churn out and back into Medicaid or a plan). For the former, I impute 0 primary care utilization in the quarters following an exit from Medicaid as a representation of the "worst-case" scenario in which full dropouts no longer have access to care following their exit from Medicaid. For the latter, I use existing records of utilization associated with a beneficiary's next enrollment episode in the year following the key provider's exit. Note that my main analysis excludes both of these dropout cohorts for sake of holding all else besides the key provider's exit constant, including plan and Medicaid enrollment. Figure F.7 below illustrates these cohorts.

Figure F.7: Treated Cohorts



I similarly define a set of control cohorts, relative to the key provider identification period, as illustrated in Figure F.8. The "continuous" control cohort remains in the same plan for at least three quarters following the key provider identification period, and is the (non-switching) control cohort I use in my main analysis. The "dropout" cohort exits their plan or Medicaid within this three quarter period (at some point after the second post-KP quarter). Full and churn dropout cohorts are defined analogously to their treated counterparts above, except that for controls, the one-year lookout period is relative to the final date of their index enrollment episode (instead of the key provider's exit date, as above).

Figure F.8: Control Cohorts

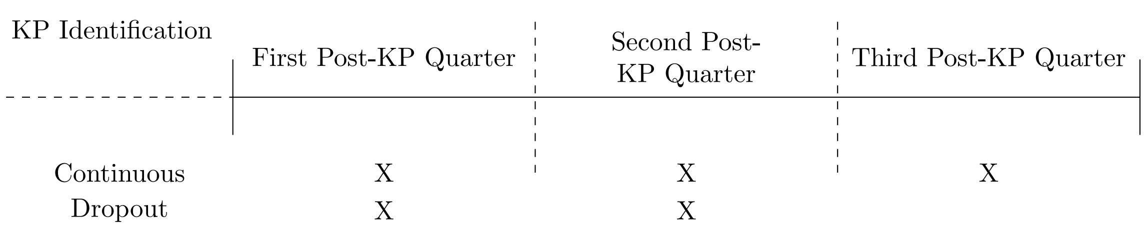


Table F.25 reports counts of the beneficiary-episodes in each cohort.

Table F.25: Beneficiary-Episodes by Cohort

Cohort	N	Pct. Total	Pct. Sub-Total
Treated	4551	100	
Continuous	4061	89.2	
Dropout	490	10.8	100
Churn	160	3.5	32.7
Full	330	7.3	67.3
Control	128511	100	
Continuous	101487	79	
Dropout	27024	21	100
Churn	6494	5.1	24
Full	20530	16	76

To perform the exercise, I generate four post-exit dummy quarters for the treated dropouts. This mimics the setting of my pre-post specification, in which I evaluate changes in utilization by collapsing the four quarters prior to the exit to a single pre-period, and the four quarters after the exit to a single post-period.[15] I drop any beneficiary whose dummy calendar quarters span into 2015 (since this is outside my study period). Of the resulting 490 treated dropouts, 160 (approximately 33%) re-enroll within a year of their key provider's exit. As noted above, instead of imputing 0's for these beneficiaries, I use their recorded primary care visits for these quarters.[16]

Similarly, and to be consistent with the treated dropouts, I generate four post-exit dummy quarters for control dropouts, dropping any beneficiary whose dummy quarter spans into 2015. I identify 27,024 dropout controls. Of these, 6,595 (24%) re-enroll within a year of the final enrollment month associated with their index enrollment episode.

I estimate the pre-post model given in Equation 2 on two subsamples of the data. The first ("Full Dropout") subsample includes the continuous and full dropout treated and control cohorts, in which I assume that beneficiaries who leave Medicaid entirely after their key provider's exit do not have any additional utilization. The second ("Churn Dropout") subsample includes the continuous and churn dropout treated and control cohorts, in which I allow beneficiaries to switch plans following their key provider's exit, and include their subsequent utilization to inform the differences estimated between the treated and control cohorts in the pre- and post-periods. Table F.26 reports the coefficients from estimating this model. The point estimates of the effect of a provider's exit on primary care utilization are slightly larger in both the Full Dropout and Churn Dropout subsamples relative to the effects reported in Table 3, as are the changes as a percent of the pre-exit mean. While this larger effect was expected (mechanically) for the full dropout cohort for whom I impute zero primary care utilization, it suggests that individuals who exit Medicaid or their plan following a key provider's exit may fail to re-engage in primary care even after re-enrolling, for at least the year following the exit. Thus, if bias enters my model through the exclusion of these dropout populations, my results would understate the effect of a key provider's exit on primary care utilization to a small degree.

[15]While my panel is only balanced for the quarters immediately before and after the exit, my concern with only imputing dummy quarters for $q' = 1$ is that this approach would overstate the impact of an exit on the quarter immediately following the exit, instead of "overstating" in a more uniform manner for the four post-exit quarters.

[16]A further consideration in constructing these imputed primary care records is that I drop partial quarters in my analysis, for simplicity's sake. Thus, for an enrollee who is enrolled for two months following their key provider's exit, they are classified as a dropout, since those two months comprise only a partial quarter. While I continue to characterize these enrollees as dropouts, I add back in their primary care utilization in the partial post-exit quarter in order to most accurately represent their primary care use.

Table F.26: Pre-Post, Imputation

	Full Dropout		Churn Dropout	
	(1)	(2)	(3)	(4)
	Ever EM Visit	Ever Encounter	Ever EM Visit	Ever Encounter
Post Exit	-0.049***	-0.050***	-0.043***	-0.045***
	(0.007)	(0.007)	(0.007)	(0.007)
Observations	827482	827482	742163	742163
Adjusted R^2	0.314	0.324	0.278	0.289
Pre-Exit Mean	.677	.682	.679	.683

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Appendix G Effect on Prescription Drug Fills

If a key provider was the primary prescriber for a beneficiary's prescription drugs, then their exit may cause a lapse in drug coverage that could generate adverse health consequences, particularly for beneficiaries with a chronic condition. A careful consideration of this potential consequence would involve the identification of prescription drugs that, when filled, indicated that the beneficiary was receiving necessary care, and when missed, would indicate a lapse in coverage that might be expected to have deleterious health effects. However, such an analysis is beyond the scope of this paper.

Instead, I explore the effect of a key provider's exit on four measures that seek to capture elements of prescription drug coverage and access. The first is a binary indicator that the enrollee has at least one prescription drug fill in a given quarter (equal to one if there is at least one fill, and zero otherwise) ("Ever Fill Date"). The second is the number of distinct drugs (measured by the National Drug Code, NDC) prescribed to an enrollee in a given quarter ("Num. Drugs"). The third is the number of drug fill dates overall in a given quarter ("Num. Fill Dates"), and the fourth is the number of drug fill dates per drug in a given quarter ("Num. Fill Dates per Drug"). I limit this analysis to drugs with a recorded days supplied of at least 30 days, as a crude approach to capturing prescriptions treating chronic conditions (as opposed to a one-time antibiotic prescription meant to only cover a short period of time) for which a lapse in coverage might indicate harm to a patient's health.

To do this, I use the prescription drug ("RX") MAX files. Table G.27 reports the usability of encounter prescription drug records for the sample states in the years for which this analysis is available:

Table G.27: Usability of RX Encounter Records by State and Year

State	2009	2010	2011	Notes
AZ	Y	Y	NR	NR=file not available at time of evaluation
IN	Y	0	0	0=IN submitted 200 or fewer RX encounter records
KY	Y	Y	Y	
NJ		Y	Y	NJ did not include prescription drugs in their HMO benefit package during 2009
NM	Y	Y	Y	
WA	Y	Y	Y	

For this analysis, I include beneficiary-episodes from AZ, KY, NM, and WA; and all beneficiary-episodes whose enrollment episode start date was 2010 or later in NJ. I exclude IN due to data limitations, as noted above. The resulting study sample contains 3,300 treated and 78,584 control beneficiary-episodes; the chronic condition subsample contains 1,010 treated and 21,070 control beneficiary-episodes.[17]

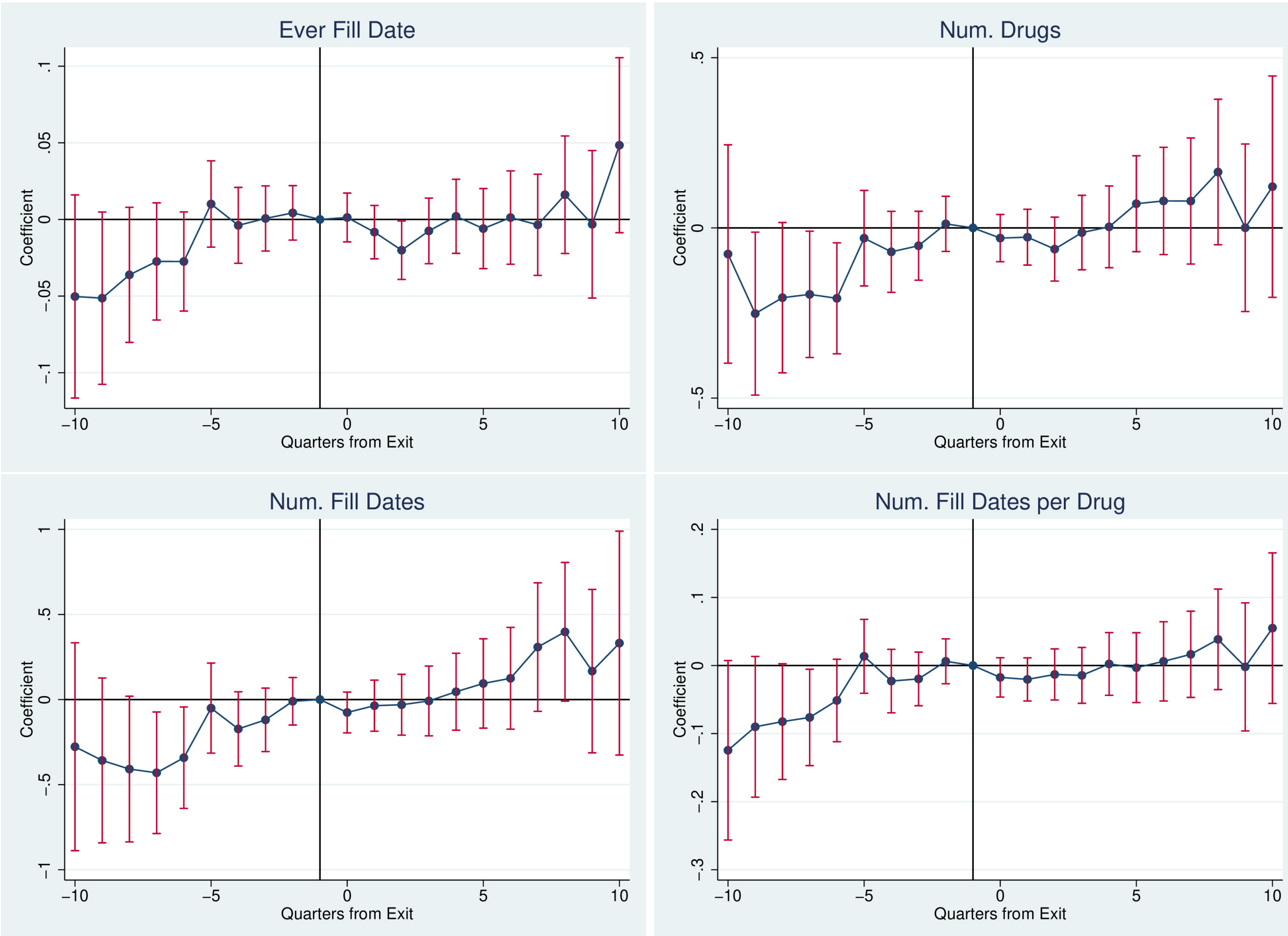
Figures G.9 and G.10 plot the estimated difference in measures of prescription drug coverage between the treated and control groups, relative to the quarter before the key provider's exit, for the study sample and the chronic condition subsample, respectively. In the study sample, there is little to no meaningful effect of a key provider's exit on prescription drug coverage. There is a slight, statistically significant decrease in the probability that a beneficiary has at least one drug fill date in the second quarter following the key provider's exit, which notably coincides with the largest decrease in the probability of having a primary care visit, as seen in Figure 2. Notably, there is a suggestion of pre-trends in the tenth through sixth quarters leading to the key provider's exit for all measures.

In the chronic condition subsample, there is no observable effect of a key provider's exit on the probability that a beneficiary fills at least one prescription in a given quarter, or the number of fills per prescribed drug. There is an increasing trend in the difference between the number of drugs and the number of fill dates between treated and control beneficiaries, relative to the pre-exit quarter; however, similarly increasing trends in the pre-period challenge the interpretation of this statistically significant positive difference as causal.

Collectively, these results suggest that the effect of a key provider's exit on crude measures of prescription drug coverage is minimal, if it exists. However, additional exploration with more carefully defined outcomes is merited in future research.

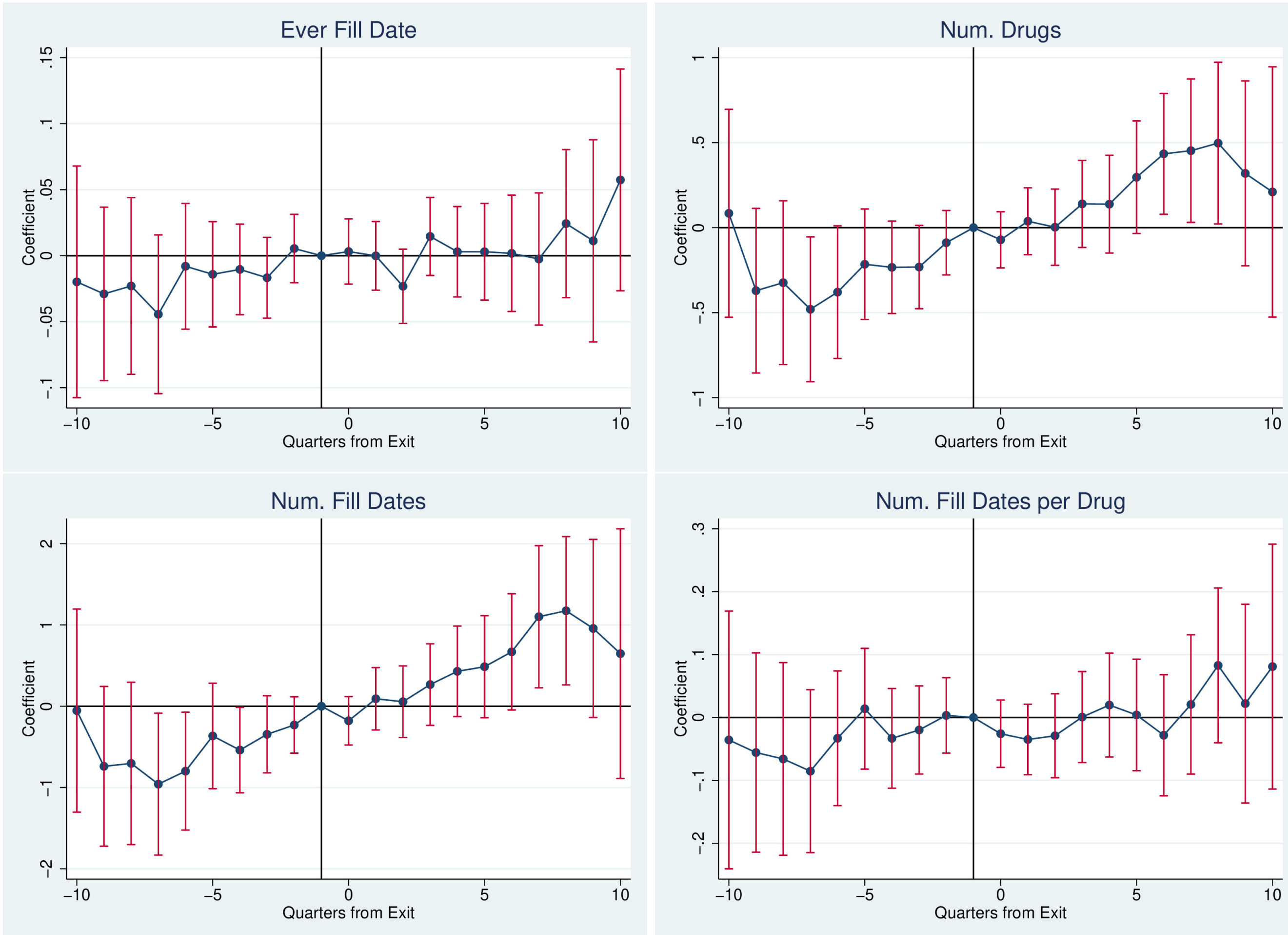
[17] I re-run my main analyses on this subsample as a check on the robustness of my results to this slightly different sample, and find similar effects of a key provider's exit on the main outcomes of interest.

Figure G.9: Event Study, Study Sample, Prescription Drug Coverage



This figure plots the θ_qs from Equation 1 for the study sample. Clockwise, the plots show the difference in the share of beneficiaries with at least one prescription drug fill, the number of distinct drugs prescribed to a beneficiary, the number of drug fill dates overall, and the number of drug fill dates per drug between beneficiaries whose key provider exited in $t' = 0$ and those whose key provider doesn't exit, in a given quarter relative to the quarter prior to the exit ($t' = -1$).

Figure G.10: Event Study, Chronic Condition subsample, Prescription Drug Coverage



This figure plots the θ_qs from Equation 1 for the chronic condition subsample.

Appendix H MAX Data

H.1 Data Files

The MAX data is organized into five files:

1. Person Summary ("PS"): this file is an overview of the beneficiary's enrollment in Medicaid in a given year. It includes time-invariant information (such as gender, race, and date of birth) as well as time-varying characteristics of the enrollment episode (such as state in which the beneficiary was enrolled in Medicaid, and zip code of residence).

2. Inpatient ("IP"): this file includes all inpatient records, such as hospitalizations. Most records are associated with one admission date, and subsequent claim-specific service dates.

3. Prescription Drug ("RX"): this file contains records for prescription drug fills, including the identity of the prescriber, when the script was prescribed, when the script was filled, and number of days covered.

4. Long-Term Care ("LTC"): this file contains records for long-term care

5. Other Services ("OT"): this file includes claims for all "other" services, including outpatient encounters and ED visits.

H.2 Data Quality and Sample States

A common concern among researchers with regards to the MAX database is the integrity of the managed care "encounter" data submitted by states. Because reimbursement from the federal government is *not* linked to the claims that states submit to CMS for inclusion in the MAX database, there are no financial incentives to ensure data is accurately reported. This is particularly relevant given the administrative burden of using the MSIS standardized format to submit claims. In order to create a subsample of the data that has the highest possible data integrity, I consider the data quality of the following two key identifying characteristics of a claim that I rely on in my analysis:

1. The quality of the encounter claims themselves
2. The quality of the reported NPI on the claim

I assess the first concern (encounter claim quality) using guidance published by Mathematica Policy Research (MPR) (Byrd and Dodd, 2012, 2015) to flag a subset of states for whom they key files (IP, OT, RX) are of acceptable quality among adult eligibles.[18] I use documentation by Bencio (2013) (detailed in Section J.1) to identify states with sufficient servicing provider identifier information. I further drop 6 states.

My final sample includes the following states and years:

Table H.28: Availability of Data by Year and State

	Years of Available Data					
State	2009	2010	2011	2012	2013	2014
AZ	X	X	X	X	X	
IN	X	X	X	X	X	
KY	X	X	X	X		
NJ	X	X	X	X	X	X
NM	X	X	X	X		
WA	X	X	X	X	X	

Appendix I Sample Construction

I.1 Cleaned Beneficiary Sample

I construct my sample using a series of restrictions. I begin by including an individual who was eligible for Medicaid as an "adult" in one of the sample states above, and who have a non-missing beneficiary identifier ("bene_id"). These bene_id's are unique to individuals across states and time. Note that to comply with data use requirements, I suppress any cell with size less than 11; I will indicate this using a "-99."

I use the variable `MCCOMBO_STRG` in the person summary ("PS") files to identify the months in a given year-state that a beneficiary is enrolled in a managed care organization. This is a 24-digit code in which each two-digit pair indicates the type of managed care the beneficiary was enrolled in in that particular month:

Figure I.11: Sample of MCCOMBO_STRG

$$\underbrace{00}_{\text{Jan.: not enrolled}}\ \overbrace{0101010101}^{\text{Feb.–Jun.: comprehensive plan}}\ \underbrace{080808080808}_{\text{Jul.–Dec.: comprehensive plan + other managed care plan}}$$

Note that in the example given in Figure I.11, I would consider the period from February to December to be one episode, since the beneficiary was in a broad "comprehensive managed care plan" category.

Some beneficiaries have duplicates of this variable in a given year. There are three reasons this can happen:

[18] Note: for some states and years, the RX data is of questionable quality; I flag this in my data

1. The beneficiary moves during a year, and remains in Medicaid (e.g. Table I.29). In this case, the zip code on file for the beneficiary's residence will change as well. I keep these beneficiaries, as long as there is exactly one record per year-month.

Table I.29: Example of Multi-State Records for a Mover

Year	State	MCCOMBO_STRG	Enrollment Episode
2009	KY	000000161616161616161616	1
2010	KY	161616000000000000000000	1
2010	IN	000000000101010101010101	2
2010	IN	010101010101010101010101	2

2. The beneficiary has multiple states or `MCCOMBO_STRG`s in a given year-month. This appears to often be accompanied by a different eligibility code as well. Because it's not obvious how to attribute these beneficiaries (or, how being enrolled in multiple states and plans might make them different from others), I drop these. Between 1 to 2% of beneficiaries in a given year fall into this category. I drop *all* records of them (across all years), if they are flagged as having multiple observations for a given year-month.

Table I.30: Example of Multi-State Records for a Non-Mover

Year	State	MCCOMBO_STRG	Eligibility Code	Enrollment Episode
2009	KY	15 (adult)	080808080808080808080808	?
2009	IN	34 (child, poverty)	000000000000000007070707	?

3. The beneficiary has one record per year-month, but the state submitting the record is not the same state as suggested by the zip code on the record (e.g. Table I.31). It's not clear what is going on here, and for that reason, I drop any episode of enrollment where the zip code does not map to the state submitting the claim.

Table I.31: Example of Multi-State Records for Out-of-State Care

Year	State	MCCOMBO_STRG	Zip Code
2009	CA	161616000000000000000000	[CA zip]
2009	AZ	000000161616161616161616	[CA zip]

I use the PS files to create a "long" dataset with one observation per year-month per beneficiary. In addition to time-invariant information such as gender and birth date, this long dataset tells us important time-variant information, such as MCCOMBO code for a particular month, plans attributed to that beneficiary in a given month (up to 4), state that is filing the claim, and zip code of residence.

I.1.1 Enrollment Episodes

I define an episode of enrollment in three broad categories:

1. Medicaid enrollment: the beneficiary was continuously enrolled in Medicaid; include any months where `MCCOMBO_STRG` is not "00" (not eligible for Medicaid in that month)

2. Managed care enrollment: the beneficiary was enrolled in a managed care plan; include any months where `MCCOMBO_STRG` is not "00", "16" (in traditional fee-for-service Medicaid), or "99" (unknown status in that month)

3. Comprehensive managed care enrollment: the beneficiary was enrolled in a comprehensive managed care plan; include any months where `MCCOMBO_STRG` is "01" (comprehensive plan only), "06" (comprehensive plan and dental plan), "07" (comprehensive plan and behavioral plan), "08" (comprehensive plan and other managed care plan), or "09" (comprehensive plan, dental plan and behavioral plan)

The focus of the exiter analysis will be on comprehensive managed care enrollment, given the emphasis in these plans on in-network care (thus potentially making a provider's exit from that plan very salient).

I.1.2 Restrictions

I impose the following restrictions on the sample (counts by restriction step are given in Table I.32).

1. **Beneficiary is never a dual enrollee**: I drop any beneficiary who is a dual eligible, since we have incomplete information on their claims (given that Medicare is the primary payer for their services).
2. **Beneficiary never has multiple records per year-month**: I drop these beneficiaries for reasons outlined above and illustrated in the examples provided by Table I.30.
3. **Beneficiary-episode has the correct eligibility category**: My question is most appropriate to ask among adult, non-disabled, and non-elderly enrollees who have relatively more agency over their healthcare utilization.
4. **Beneficiary-episode state on record matches the state of residence**: I drop these beneficiaries for reasons outlined above and illustrated in the examples provided by Table I.31.
5. **Beneficiary-episode is associated with a state that is in the selected sample**: These are the states that have reliable encounter and NPI information.

Table I.32: Count of Distinct Beneficiaries, by Restriction

Stage	Medicaid	MCO	CMCO
Baseline (unrestricted) count	2,720,964	2,720,964	2,720,964
Beneficiary Level Restriction			
Drop ever dual	2,648,679	2,648,679	2,648,679
Drop ever multiple records	2,532,322	2,532,322	2,532,322
Beneficiary-Episode Level Restriction			
Drop if episode is associated with non-adult eligibility	2,409,929	2,419,335	2,432,399
Drop if episode is “out-of-state”	2,399,455	2,410,390	2,423,226
Drop if state associated with episode is out-of-sample	1,811,401	1,821,664	1,837,044
Final (cleaned) beneficiaries	1,811,401	1,821,664	1,837,044

Following the application of these restrictions, the final counts of distinct beneficiaries by state and year are given in the final row of Table I.32. There are two final datasets produced by these programs. The first has one observation per beneficiary, and contains all time-invariant information (such as gender, DOB, race, etc.). The second has one observation per bene-year-month, and contains time-varying information such as the episode of enrollment, the zip code of residence, the plan(s), etc.

I.2 Data Prep

On the base of the beneficiaries identified in Section I.1, I merge in the claims information from the IP, OT, and RX files.[19] I fill in missing NPIs with the `SRVC_PRVDR_ID_NMBR`-NPI crosswalk described in Section J.1, and merge in additional provider information.[20]

I.2.1 Outcomes of Interest

I flag the following outcomes of interest, identified in the claims data. Coding note: there are several possible coding systems associated with procedures. In addition to the more traditional (ICD-9/10, CPT, HCPCS) and more common systems used, there are also CRVS systems and unnamed “other systems.” I have limited information as to what the “other systems” are, and since my prior is that a disruption to care will lead to a decrease in the frequency of events (such as preventive care, office visits, etc.) that are defined using procedure codes,[21] I do not exclude these unknown systems from the code that flags the outcomes of interest. Since 99% of all procedure codes are accounted for (i.e. either in a known system, or not applicable to the claim), it is reasonable to expect that any measurement error in including systems that may overlap in code with the commonly used systems will be relatively trivial.

[19] Note that for Indiana in 2009 and 2010, the RX files are denoted as of questionable accuracy by the MPR publications

[20] I use the crosswalk to fill in any missing NPI, not just for OT files. Older state-specific provider identifiers known as “Legacy Provider Identifiers” (LPIs) are defined consistently across files. However, my analysis does not rely on knowing the NPI associated with an IP or RX claim.

[21] The only exception to this is for some ACSC categories, namely avoidable amputations for diabetic patients and dialysis (which are to be flagged and excluded from flags for hypertension ACSCs)

I.2.2 Inpatient Hospitalizations and Emergency Department Visits

I flag inpatient hospitalizations ("hospitalizations") when the type of service code on the claim equals 01 ("INPATIENT HOSPITAL"), the claim is from the IP file, and the admission date is non-missing, and 0 otherwise. I flag emergency department visits ("ED visits") when the place of service code on the claim equals 23 ("EMERGENCY ROOM - HOSPITAL") and the claim is from the OT file.

I.2.3 Urgent Care Facility and Federally Qualified Health Centers Visits

I use the place of service code to flag the use of urgent care facilities and federally qualified health centers (FQHC). Specifically in the MAX claims, urgent care facilities are associated with place of service code 20 ("URGENT CARE FACILITY") and FQHCs with code 50 ("FEDERALLY QUALIFIED HEALTH CENTER").

I.2.4 Office Visits

I include two measures of office visits, intended to capture outpatient use. The first is intended to capture any general visit that takes place in an office, and is defined as any claim where place of service code is 11 ("OFFICE"). The second is intended to capture an office visit that was conducted specifically by a clinician that could be the beneficiary's primary provider, and is defined as any claim where place of service code is 11, and where type of service is 8 ("PHYSICIANS"), 10 ("OTHER PRACTITIONERS"), 36 ("NURSE MIDWIFE SERVICES"), or 37 ("NURSE PRACTITIONER SERVICES"). I also include a measure of "outpatient encounters", which I use in Section I.2.11 to identify a beneficiary's "key provider." These encounters, intended to capture claims that represent primary/preventive care visits, are defined as a claim where type of service is as above (8, 10, 36, or 37) and place of service is 11 ("OFFICE"), 17 ("WALK-IN RETAIL HEALTH CLINIC"), 22 ("OUTPATIENT HOSPITAL"), 49 ("INDEPENDENT CLINIC"), 50 ("FEDERALLY QUALIFIED HEALTH CENTER"), or 72 ("RURAL HEALTH CLINIC").

I.2.5 Evaluation and Management Visits

I use Berenson-Eggers Type of Service (BETOS) codes to identify evaluation and management ("EM") visits. These types of visits are commonly used as a measure of a patient's access to preventive care, and vary across intensity of visit (by time and effort contributed by the physician) as well as reimbursement level. I include two versions of EM visits in my analysis: The first version, any EM visit, is broadly defined as any BETOS code that starts with "M", i.e. all BETOS codes that are categorized as EM visits. The second version, EM office visits, is limited to claims categorized as "M1A" ("OFFICE VISITS - NEW") or "M1B" ("OFFICE VISITS - ESTABLISHED") BETOS codes, and which is intended to capture EM visits in an outpatient, office setting (as opposed to e.g. "M2A", "HOSPITAL VISIT - INITIAL").

I.2.6 Tests

Using the BETOS codes as described above, I flag the presence of any type of test (e.g. lab tests, urinalysis, etc.) by identifying any claim with a BETOS code starting with "T."

I.2.7 Hospitalizations from Ambulatory Care Sensitive Conditions

Hospitalizations due to ambulatory care sensitive conditions (ACSCs) are considered preventable with adequate preventive care, and thus represent a measure of the quality of the preventive care a patient has access to. To flag such hospitalizations, I modify a program created by AHRQ to flag hospital admissions with a diagnosis or procedure code that is characterized by a particular prevention quality indicator (PQI) (details on each indicator category is available online). I define several broad and condition-specific categories of ACSC hospitalizations:

1. Any ACSC (Any of the following PQIs are flagged as present: 01, 02, 03, 05, 07, 08, 09, 10, 11, 12, 13, 14, 15, 16)
2. Overall ACSC, as defined by PQI 90 (includes all of the PQIs except 02 and 09)
3. Acute ACSC, as defined by PQI 91 (includes PQI 10, 11, and 12)
4. Chronic ACSC, as defined by PQI 92 (includes PQI 01, 03, 05, 07, 08, 13, 14, 15, and 16)
5. CHF ACSCs, as defined by PQI 08
6. COPD ACSCs (only applicable to adults over age 40), as defined by PQI 05
7. Diabetes ACSCs, including PQIs 01, 03, 14, and 16
8. Hypertension ACSCs, as defined by PQI 07

These outcomes are limited to hospitalizations (i.e. the place of service must be an inpatient hospital with a non-missing admission date) for beneficiaries whose age is 18 or older at time of admission.

Due to data limitations, I am unable to impose the following restrictions that are in place in the source code:

1. I am unable to exclude hospitalizations that are transfers from another hospital, which is defined in the code using variables that aren't available in the MAX data; this should be applied to all measures

2. I am unable to exclude claims where the Major Diagnostic Category ("MDC") is 14 (Pregnancy, Childbirth, and Puerperium) since the MAX data does not have DRG or MDC codes; these should be applied to PQI02 (Perforated Appendix) and PI16 (Lower Extremity Amputation)

Tables I.33-I.36 report the initial frequency of outcomes of interest.

Table I.33: Initial Frequency of Hospitalizations and ED Visits

Year	IP Hosp	Pct.	IP Hosp (40)	Pct.	OT Hosp	Pct.	ED	Pct.
2009	146153	12	18546	2	−99	.	391369	32
2010	132513	11	17846	1	−99	.	409574	33
2011	146504	11	21438	2	−99	.	437591	34
2012	147071	12	19588	2	−99	.	417156	33
2013	100414	11	15345	2	−99	.	315101	34
2014	24154	8	7980	3	−99	.	121064	40

Table I.34: Initial Frequency of ACSC Hospitalizations

Year	Any	Pct.	Overall	Pct.	Acute	Pct.	Chronic	Pct.	Diab.	Pct.
2009	3197	2	2932	2	1423	1	1529	1	1529	1
2010	2867	2	2655	2	1198	1	1481	1	1481	1
2011	3311	2	3078	2	1383	1	1734	1	1734	1
2012	3012	2	2798	2	1198	1	1637	1	1637	1
2013	2251	2	2125	2	849	1	1303	1	1303	1
2014	1038	4	991	4	404	2	601	2	601	2

Table I.35: Initial Frequency of Office Event Outcomes

Year	Office Visit	Pct.	EM Visit	Pct.
2009	865468	71	769824	63
2010	873358	70	801425	65
2011	906135	71	827790	65
2012	869973	70	796778	64
2013	659523	70	603084	64
2014	242477	80	215207	71

Table I.36: Initial Frequency of Place of Service Outcomes

Year	Urgent Care	Pct.	FQHC	Pct.
2009	22569	2	15186	1
2010	25814	2	12938	1
2011	28534	2	18517	1
2012	29747	2	18977	2
2013	16220	2	15218	2
2014	939	0	3600	1

At this point, claims can be from any state (though the beneficiaries are residents of a state in the sample, as detailed above). The initial number of beneficiaries per year are given in Table I.37.

Table I.37: Number of Distinct Beneficiaries per Year

Year	Num. Benes
2009	1263971
2010	1266885
2011	1293219
2012	1255147
2013	939333
2014	305007

I.2.8 Episode Identification

I use the beneficiary-year-month dataset created in Section I.1 to identify episodes of CMCO enrollment ("enrollment episode"). I drop any enrollment episode whose start date is January 2009. Since my data starts in January 2009, it's not clear if this is an actual start date of an episode, or simply an artifact of the data (i.e. a beneficiary may have started an episode in November 2008). For this reason, I drop these episodes entirely. Table I.38 shows the number of distinct beneficiaries per year after dropping this cohort.

Table I.38: Number of Distinct Beneficiaries per Year, After Dropping Jan 2009 Start Date

Year	Num. Benes
2009	434339
2010	667331
2011	933163
2012	984016
2013	741200
2014	253877
Total	1444216

After this restriction, beneficiaries can have up to 10 episodes.

I.2.9 Out-of-State Care

There is a concern that patients who get care out-of-state may have incomplete encounter records, particularly if that state is not in the sample of states that have known adequate encounter quality. I flag any beneficiary who has a record of out-of-state care, in three categories: out-of-state encounter from the OT file, out-of-state encounter from the IP file, and out-of-state general record in the IP file.

Table I.39: Beneficiary-Episodes that Have an Out-of-State Record

Type	Num. OOS	Share	Total Bene-Episodes
OOS OT Encounter	170	0	2190464
OOS IP Encounter	13	0	2190464
OOS IP, General	26	0	2190464

I drop any beneficiary who has a record of an out-of-state encounter of any kind during the enrollment episode. Additionally, I drop any beneficiary-episode that has multiple enrollment states ($< 1\%$), i.e. a beneficiary for whom there are two or more state associated with their enrollment (`STATE_PS`) in a particular episode (this might happen if a beneficiary moves mid-episode).

I.2.10 Plan Attribution

The PS files contain a record of a plan ID at the monthly level. A beneficiary can have up to four plans in a given year-month. Table I.40 shows the number of non-missing PS plans (1-4) that a beneficiary in my sample has recorded on a claim in a given year-month.

Table I.40: Distribution of Number of Non-Missing Plans per Month

Number of Non-Missing Plans in a Year-Month	Year-Months	Share of All Year-Months
1	16972737	0.35238
2	30528440	0.63381
3	662137	0.01375
4	2917	0.00006

Table I.41 reports the share of OT claims in which the `PHP_ID` on file for the OT claim matches the PHP[1-4] from the PS files, conditional on PHP[1-4] not being missing. This is included to identify whether there is one PHP in particular that corresponds to the most "common" plan ID observed, given that I don't know the meaning/implied ranking of PHP1-4

Table I.41: Share(Claims) Where PHP_ID in OT Matches PHP[1-4]

PHP Number	Mean	SD	P25	Median	P75	Total
1	0.64156	0.33539	0.33	0.67	1.00	47525303
2	0.47985	0.26891	0.25	0.50	0.67	47525303
3	0.51560	0.26029	0.29	0.67	0.67	47525303
4	0.29809	0.25468	0.00	0.29	0.50	47525303

It's not clear whether the numbers assigned to these plans correspond to e.g. primary payer. In one random example, PHP1 was a non-HMO/HIO, and PHP2 was an HMO/HIO. Both were filled in all the months the beneficiary was enrolled. In this example, the HMO/HIO most closely matched the `PHP_ID` in the encounter claim.

Using a crosswalk prepared by MPR that links plan ID to plan type ("MCXW"), I flag which of the four potential plans corresponds to an HMO/HIO. I drop any beneficiary who has a year-month in which two HMO/HIOs are recorded (over 98% have only one HMO per year-month). I keep all beneficiaries who have the same HMO/HIO for the entire episode of enrollment, indicating that they did not switch out of their plan at any time during the enrollment episode. Table I.42 documents the number of remaining beneficiaries per year that have one plan per episode.

Table I.42: Number of Distinct Beneficiaries per Year, One Plan per Enrollment Episode

Year	Num. Benes
2009	403151
2010	623281
2011	886548
2012	935696
2013	702259
2014	253847
Total	1385430

Of note, there are no records of a claim where PHP1 is missing, but PHP2-4 are non-missing. Additionally, there is never a claim where the `PHP_ID` associated with an OT record is not missing, but the corresponding PHP1-4 is. Conversely, there are claims where `PHP_ID` in the OT file is missing (or 8-filled), with a non-missing corresponding PHP1-4. Thus, we can reasonably conclude that the PHP variables are a complete record of the beneficiary's enrollment during a given period in time, and should be considered more reliable than the `PHP_ID` on a particular claim as representative of the plan the beneficiary was enrolled in.

I.2.11 Key Provider Identification

The goal of this study is to understand the effect on a patient's utilization and health when the provider who is most important to them exits from their plan. I refer to this provider as the "key provider," and define them flexibly as the provider (physician, MD/DO; or advanced practice nurse, NP/PA) of any specialty who is responsible for the majority of a beneficiary's outpatient encounters. Encounters are defined as any record in the OT file where the type of claim is "encounter." I define outpatient encounters as an encounter where the type of service is in one of the following categories: physicians, other practitioners, nurse midwife services, nurse practitioner services (see: Table I.49); and the place of service is one of the following categories: office, walk-in retail clinic, outpatient hospital, independent clinic, federally qualified health center (FQHC), rural health clinic (see: Table I.50).[22] These categories are intended to reasonably capture an outpatient setting/service in which we would expect to observe that a beneficiary is being serviced by an individual provider,[23] without being too prescriptive on what that might be. As I discuss in Section J.1, there is a non-negligible number of NPIs associated with the servicing provider on the claim that are missing or inaccurate. In most cases, these claims also (or instead) contain legacy provider identifiers ("LPIs"), an older state-specific identifier that I describe in more detail below. After applying a NPI-LPI crosswalk I develop in Section J.1, the number of distinct LPIs on an outpatient encounter claim with a missing NPI is given in Table I.43.

Table I.43: Percent of Missing NPIs per Distinct LPI in Outpatient Encounter Data

State	2009	Share	2010	Share	2011	Share	2012	Share	2013	Share	2014	Share
AZ	−99	.	−99	.	−99	.	−99	.	−99	.	−99	.
IN	12	0.00	35	0.01	41	0.01	236	0.03	249	0.03	−99	.
KY	−99	.	−99	.	−99	.	−99	.	−99	.	−99	.
NJ	−99	.	−99	.	−99	.	−99	.	−99	.	−99	.
NM	−99	.	−99	.	−99	.	−99	.	−99	.	−99	.
WA	174	0.04	−99	.	−99	.	−99	.	−99	.	−99	.

My primary concern in not having sufficient non-missing NPI is that I won't be able to accurately identify the key provider, as I define it above. For example, in WA in 2009, it is likely that I will identify fewer key providers that have non-missing NPIs (and more that have missing NPIs, though non-missing LPIs). As long as the LPI variable is sufficiently non-missing (which it is for all state-years in my sample), I will include all state-years to identify key providers, including WA-2009. My definition of key provider will allow for a missing NPI (non-missing LPI) to be flagged as a key provider if that LPI meets the criteria below; because I ultimately limit my analysis of exiting key providers to those that have non-missing NPIs, I will simply end up dropping the observations for whom the key provider is identified from the LPI. Table I.43 shows the number of missing NPIs at the distinct LPI level, in the outpatient encounter data.

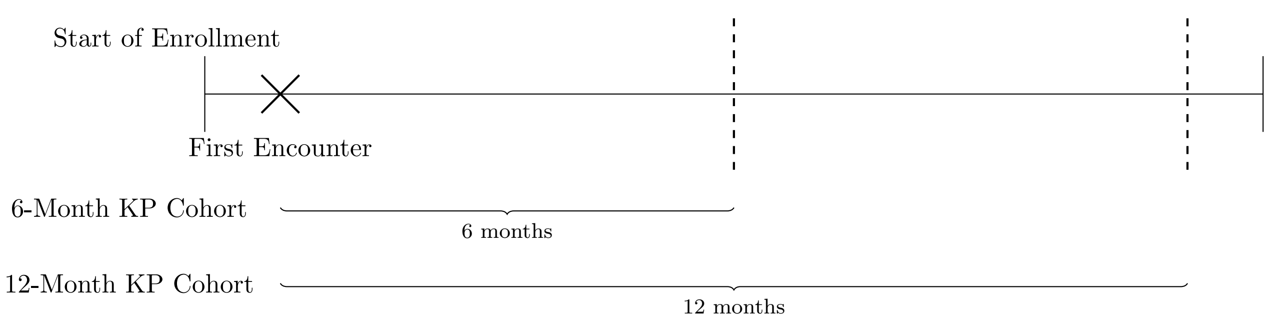
I define the key provider as the provider that has the following characteristics:

1. Has a taxonomy type of "Allopathic/Osteopathic Physicians" ("MD/DO") or "Physician Assistants & Advanced Practice Nursing Providers" ("NP/PA")[24]

2. Is responsible for the plurality of the patient's encounters for a particular type of service (given in Table I.49) during the definition period

3. Is an individual (not an organization)

Figure I.12: Key Provider Definition Period



In the case of a tie for share of services, I drop these beneficiaries. This occurs for approximately half of beneficiaries. The final number of beneficiaries with an identifiable key provider, per state, is given in Table I.44.

[22]These place of service codes largely overlap with those used by the Dartmouth Atlas in identifying primary care visits for post-acute care. `https://www.dartmouthatlas.org/interactive-apps/post-acute-care/`

[23]In contrast to a record in which we might expect to observe an NPI associated with an institution, e.g. for a type of service like "lab and x-ray"

[24]This can include other nursing degrees in addition to Nurse Practitioners and Physician Assistants, though I simplify the abbreviation by just referencing these two degrees.

Table I.44: Counts of Key Providers

	6-Month KP Cohort			12-Month KP Cohort		
State	Total Bene	Bene W/ KP	Pct	Total Bene	Bene W/ KP	Pct
AZ	216859	71310	33%	157419	71812	46%
IN	95909	52449	55%	50444	30772	61%
KY	49612	26358	53%	22982	14468	63%
NJ	190161	81087	43%	137114	75835	55%
NM	29229	7351	25%	22664	9229	41%
WA	92988	31968	34%	44654	22384	50%
Total	674577	270498	40%	435252	224496	52%

Table I.45 provides additional details on the share of service dates each key provider is associated with.

Table I.45: Share of Services Attributable to the Identified Key Provider

Measure	Mean	STD	P25	Median	P75	Tot. Benes
Panel A. 6-Month KP Cohort						
Total Encounters	7.23	3.91	4	6	9	291484
Encounters with KP	5.12	2.75	3	4	6	291484
Share(Encounters)	0.76	0.22	0.6	0.75	1	291484
Panel B. 12-Month KP Cohort						
Total Encounters	9.48	6.06	5	8	12	232674
Encounters with KP	6.02	3.88	3	5	7	232674
Share(Encounters)	0.69	0.23	0.5	0.67	0.89	232674

I.2.12 Flag Exiters

I merge the provider exiter data onto the key provider data to flag exit dates for a specific NPI from a particular plan. I define a beneficiary as "treated" (having experienced an exit) if their key provider exits the plan they're enrolled in during their episode of enrollment (outside of the key provider definition period). Table I.46 shows the number of beneficiaries per year who experience a key provider exit. An additional restriction is that the beneficiary must be enrolled for at least one month pre and post exit.

Table I.46: Frequency of Treated, by Year

	6-Month KP Cohort			12-Month KP Cohort		
Enrollment State	Experience Exit	All Benes	Share	Experience Exit	All Benes	Share
AZ	9711	63303	0.15	7363	58875	0.13
IN	929	48183	0.02	672	26748	0.03
KY	289	24255	0.01	154	11272	0.01
NJ	1373	75822	0.02	1398	66873	0.02
NM	209	6916	0.03	249	8497	0.03
WA	251	28452	0.01	169	19517	0.01
All	12762	246908	0.05	10005	191779	0.05

I define months from exit as the months relative to the key provider's exit, where $t' = 0$ is the month of exit, i.e. the final month that a provider is observed treating patients in a particular plan.

I.2.13 Rural-Urban Commuting Area Codes

I use the beneficiary's zip code to classify the area they live in as a metropolitan, micropolitan, or small town/rural area.

Table I.47: Classification of Broad Geographic Areas

Classification	Codes
Metropolitan	1, 2, 3
Micropolitan	4, 5, 6
Small Town/Rural	7, 8, 9, 10

Additional information on these definitions can be found online: `https://www.ers.usda.gov/data-products/rural-urban-co` `documentation/`.

I.3 Overview of Beneficiary Counts by Restriction

Table I.48 provides counts of unique enrollees at each restriction step. Details on each of these steps are provided above.

Table I.48: Restrictions and Beneficiary Counts in the Sample Construction

Restriction	Number of Unique Beneficiaries	
	6-Month KP Cohort	12-Month KP Cohort
Eligible for Medicaid as an "Adult," non-missing beneficiary ID, enrolled in an MCO for at least one month in a sample state	2,720,964	2,720,964
Can be attributed to exactly one CMCO that is an HMO/HIO during an episode of enrollment whose start date is after January 1, 2009 (the start of my observable data)	1,385,430	1,385,430
Has one identifiable key provider	270,498	224,496
Has a Key Provider that exits, and remains enrolled for at least one month after exit	12,762	10,005

Because my final analysis is at the quarter level, and I require treated beneficiaries to be enrolled for at least one quarter before and after the exit, the final treated count will drop further to 4,061 and 3,709 for the 6-month and 12-month KP cohorts, respectively.

I.4 Additional Tables and Figures

Table I.49: Relevant Types of Service

TOS Code	Description
8	Physicians
10	Other practitioners
36	Nurse midwife services
37	Nurse practitioner services

Table I.50: Relevant Places of Service

POS Code	Description
11	Office
17	Walk-in retail clinic
22	Outpatient hospital
49	Independent clinic
50	Federally qualified health center
72	Rural health clinic

Appendix J Identifying Provider Networks and Exiters

J.1 NPI-Provider ID Crosswalk

The OT files in the MAX data identify providers with three variables.

1. `SRVC_PRVDR_ID_NMBR`: the state-assigned unique identifier of the provider that treated the beneficiary, though not necessarily the same provider that billed for the service
2. `PRVDR_ID_NMBR`: the state-assigned unique identifier of the billing provider
3. `NPI`: the National Provider Identifier (NPI) of the provider who treated the patient.

The first two variables (`SRVC_PRVDR_ID_NMBR` and `PRVDR_ID_NMBR`) are referred to as "legacy" provider identifiers ("LPIs"), since they are the state-unique IDs that states originally reported in MSIS files to identify providers.[25] With just these LPIs and no associated provider details, provider-based research was limited by insufficient information to follow providers within and across states, and across time. Starting in 2009, CMS began requiring that states report the NPI associated with the provider treating the patient (i.e., associated with the `SRVC_PRVDR_ID_NMBR`, which I refer to as the LPI from now on). The NPI is a universally used unique provider identifier that is distinct to individuals across space and time, and can be linked to external databases with a wealth of personal and professional information. However, particularly in the first few years after the reporting requirement was instated, the reported NPIs on the MAX claims were sometimes inaccurate and/or missing. Thus, researchers at Mathematica Policy Research (MPR) were contracted by CMS to create a crosswalk between the LPI and the NPI, to be made available to researchers who needed to fill in missing NPIs on claims. The MAX Provider Characteristics ("MAXPC") crosswalks for 2009 to 2011 include links from LPIs to NPIs for states whose claims were available at the time. Bencio (2012) and Bencio (2013) document the creation of the MAXPC files, as well as guidance on how to use them and which states are unusable in terms of the quality of the reported servicing and billing providers, for 2009 and 2010, respectively. Unfortunately, no explicit guidance was published for the 2011 crosswalks, nor are there any crosswalks after 2011. I discuss how I address these gaps below.

The MAXPC reports note that while states were required to report the NPIs associated with the LPI, sometimes the `PRVDR_ID_NMBR` (billing provider) was reported instead. Table J.51 summarizes the believed accuracy of the LPI, where "Y" indicates that the state-year is believed to be of good accuracy in terms of reporting, "C" indicates that researchers should use with caution, "N" indicates highly recommended to not use (e.g. CA classified less than 30% of their OT servicing providers are "individuals" in 2009), and "[NA]" indicates that no data was available at the time of the report.

Table J.51: NPI Quality per State, OT Files

State	2009	2010
AZ	Y	Y
IN	Y	Y
KY	C	Y
NJ	Y	[NA]
NM	C	C
WA	C	Y

The states in my sample have MAXPC crosswalks available for the following years:

Table J.52: Counts of Missing NPIs Filled in by the MAXPC Crosswalk in The OT Files

State	2009	2010	2011
AZ	X	X	
IN	X	X	X
KY	X	X	X
NJ	X		
NM	X	X	
WA	X	X	X

Table J.53 documents the initial count of missing NPIs associated with each unique (non-missing) LPI on encounter claims. Note that NPI is classified as missing when the NPI cell is truly empty, as well as when the entry in the NPI cell is not an identifiable NPI (i.e. when the first digit is neither a "1" or a "2", or when the NPI is not 10 digits long). This table is at the unique non-missing LPI level (not the claims level).

[25]Notably, a particular provider can have multiple provider IDs per state.

Table J.53: Initial Counts of Missing NPIs in Encounter Data

State	2009	Pct	2010	Pct	2011	Pct	2012	Pct	2013	Pct	2014	Pct
AZ	3328	13	3175	11	3001	11	2751	10	2561	9	.	.
IN	18432	100	19161	100	19682	100	20360	95	20782	94	.	.
KY	588	9	544	7	1091	4	2877	7	.	.	.	.
NJ	−99	.	−99	.	−99	.	−99	.	−99	.	−99	.
NM	121	0	163	1	177	1	738	2	.	.	.	.
WA	31610	85	27	0	−99	.	−99	.	−99	.	.	.

To fill in remaining missing NPIs when possible, I create an updated NPI-LPI crosswalk in three steps:

Step 1: MAXPC, 2009–2011. Fill in any NPI that maps to a LPI pair identified in the MAXPC crosswalk.

Table J.54: Counts of NPIs Filled in by the MAXPC Crosswalk in The OT Files

State	2009	Share	2010	Share	2011	Share
AZ	19365	0.71	20165	0.68	−99	.
IN	20990	0.66	21314	0.79	21664	0.78
KY	31262	0.76	33262	0.77	34889	0.76
NJ	19128	0.70	−99	.	−99	.
NM	6555	0.17	6105	0.15	−99	.
WA	24652	0.40	28395	0.82	29113	0.86

Note: these counts indicate the NPIs matched to LPIs that match to the crosswalk, and can ultimately be used to fill in missing NPIs in the claims

Step 2: NPPES, 2009–2014. The National Plan & Provider Enumeration System ("NPPES") has a downloadable, publicly available file that contains all NPIs and, in addition to other relevant information I discuss below, includes state-specific records of Medicaid provider IDs (LPIs). I use these "other provider identifiers" to create a crosswalk where a LPI is linked to exactly one NPI.[26] Bencio and colleagues use the NPPES crosswalk in the creation of the MAXPC files, though they note that a main limitation of this approach is that non-medical providers[27] typically don't have NPIs, and thus won't be identifiable in the NPPES database. Since my primary analysis focuses on identifying medical providers, this is not a significant limitation to this project, nor the goal of the crosswalk in identifying individual servicing providers. Fill in any NPI record with the "NPPES NPI" associated with the LPI-NPI pairing in the NPPES crosswalk.

Table J.55: Counts of NPIs Filled in by the NPPES Crosswalk in The OT Files

State	2009	Share	2010	Share	2011	Share	2012	Share	2013	Share	2014	Share
AZ	1119	0.04	1404	0.05	9596	0.32	9633	0.32	9624	0.31	.	.
IN	637	0.02	669	0.02	659	0.02	11314	0.38	11765	0.33	.	.
KY	1937	0.05	1870	0.04	2117	0.05	12511	0.28	.	.	.	.
NJ	3529	0.13	13872	0.47	14139	0.45	14772	0.42	14486	0.39	14176	0.37
NM	1899	0.05	1875	0.05	2278	0.05	2251	0.04	.	.	.	.
WA	−99	.	−99	.	−99	.	−99	.	−99	.	.	.

Step 3: All-Year Crosswalk, 2009–2014. Append the (partially) cleaned datasets created by Steps 1 and 2 to create an all-year dataset, and identify any LPI-NPI pairings where:

- The LPI is only ever associated with one (non-missing) NPI for all years that it appears in the OT files

[26] I use the downloadable version labeled with the time stamps 5/23/2005 and 3/12/2017; the "other provider ID type" with a value of "05" indicates a Medicaid provider ID. I also validate this crosswalk against the MAXPC pairs, and find that approximately 17% of pairs in the NPPES crosswalk are also in the MAXPC crosswalk. I take these pairs to be time invariant, and thus applicable across multiple years.

[27] Such as adult day care, case management, and non-emergency transportation

- The LPI is only ever associated with one (non-missing) NPI where ENTITY=1 (i.e. the NPI is linked to an individual) for all years in which it appears in the OT files as an individual. This is meant to reflect the fact that we would expect LPI to be largely linked to individuals (as in Bencio (2012), Bencio (2013)), but that sometimes, the LPI is on a claim where the associated NPI is a billing provider or institution, as in a claim for a lab test, for example.

Table J.56: Counts of NPIs Filled in by the All-NPI Crosswalk in The OT Files

State	2009	Share	2010	Share	2011	Share	2012	Share	2013	Share	2014	Share
AZ	2621	0.11	3822	0.15	14921	0.61	15310	0.61	15666	0.62	.	.
IN	8036	0.27	3232	0.13	3542	0.14	15941	0.58	16954	0.59	.	.
KY	3252	0.09	3470	0.09	3939	0.10	29184	0.70	.	.	.	.
NJ	3761	0.14	14527	0.51	15902	0.53	19773	0.57	21444	0.60	23423	0.62
NM	25401	0.75	27989	0.78	38876	0.94	43487	0.95	.	.	.	.
WA	21056	0.46	5805	0.17	4694	0.14	34250	1.00	35919	1.00	.	.

The final crosswalk is unique at the LPI-state-NPI level. Table J.57 shows the final percentage of missing NPIs after merging the crosswalk onto the OT encounters dataset and filling in when NPI is missing.

Table J.57: Final Counts of Missing NPIs in Encounter Data

State	2009	Pct	2010	Pct	2011	Pct	2012	Pct	2013	Pct	2014	Pct
AZ	3149	12	2975	11	2834	10	2641	9	2463	9	.	.
IN	282	2	413	2	469	2	1078	5	1097	5	.	.
KY	499	7	469	6	972	3	2596	6	.	.	.	.
NJ	−99	.	−99	.	−99	.	−99	.	−99	.	−99	.
NM	93	0	130	0	121	0	212	1	.	.	.	.
WA	7473	20	25	0	−99	.	−99	.	−99	.	.	.

For additional provider information, I bring in gender and professional information from the NPPES files; gender and year of graduation from medical school from the Physician Compare database (also publicly available); and physician age in 2018 (the year I was granted access to the data), year of graduation from medical school, and gender from Doximity, a social network for healthcare professionals. I use the NPPES professional information to identify provider type, taxonomy, and sub-specialty. I use the NPPES gender to identify whether a provider is male or female, and fill in any missing gender cells with Physician Compare and Doximity data, in that order. I use the year of graduation from medical school from Physician Compare, filled in when missing by Doximity year of graduation. Finally, I calculate predicted age in 2018 as a function of year of graduation, and use this as my preferred measure of age. The relationship between age and year of graduation is given in Table J.58, which is a simple OLS regression of non-missing age in 2018 on year of graduation.

Table J.58: Relationship Between Age and Year of Graduation

Analysis of Variance					
Source	DF	Sum of Squares	Mean Square	F Value	Pr>F
Model	1	3438411	3438411	142913	<.0001
Error	33705	810925	24.05949		
Corrected Total	33706	4249336			

Root MSE	4.90505	R-Square	0.8092
Dependent Mean	55.88970	Adj R-Sq	0.8092
Coeff Var	8.77630		

Parameter Estimates					
Variable	DF	Parameter Estimate	Standard Error	t Value	Pr>—t—
Intercept	1	1848.37432	4.74162	389.82	<.0001
DOX_MED_YOG	1	−0.90083	0.00238	−378.04	<.0001

J.2 Provider Networks

In this project, I evaluate the impact of a provider's exit from a Medicaid managed care organization on utilization and quality of care outcomes among the beneficiaries for whom that provider was particularly important (i.e. their "key provider"). To do this, I first construct provider networks based on which plans providers were seeing patients in, and when. I use the universe of all Medicaid beneficiary claims from the OT files (which contain all outpatient records I'm interested in) within the subset of states in my sample (Table H.28). I use encounter records, which denote that a patient received care while covered by a managed care plan, to construct observed networks. Encounter records have three relevant pieces of information:

1. The date of the claim
2. The servicing provider LPI on the claim, and associated NPI
3. The plan (`PHP_ID`) that paid the provider for the claim.

The plan variable (`PHP_ID`) is 8-filled when the claim is not an encounter. It's reasonable to expect that in aggregate across multiple patient claims (and with the restrictions below), I will be able to accurately identify a provider's last date in a plan, regardless of whether there is some error in `PHP_ID` reporting for a specific beneficiary. I clean the service provider variable and link it to an NPI as in Section J.1. Table J.57 shows the frequency with which an NPI is missing in an encounter claim. Despite the relatively high frequency of missing NPIs in WA 2009, I include this state-year in my analysis to identify exiters, with the following reasoning: if this high prevalence of missing NPIs occurred in a later year, I might be worried about mis-attributing a provider to an exit date from a plan, when in reality, I simply can't observe their NPI. However, because 2009 is the first year I start searching for exiters, there's no down-stream risk of flagging individuals inaccurately as exiters.

I create a "network" dataset that has one observation per NPI for each year-month they were actively treating patients in a particular Medicaid plan, as well as the number of patients and number of encounter-days (i.e., up to 31 days per month) in that year-month (per plan). The final dataset has 226,504 distinct NPIs that matched to an individual provider entity in the NPPES database.

Next, I turn to identifying provider exits from plans. There are several ways that I might incorrectly flag a provider exit by just noting the final date in the data in which the provider treats a Medicaid enrollee:

1. The provider is still available to patients in the plan, but hasn't seen any patients in recent months
2. There is simply no more data for that plan/state after a particular year-month

To avoid falsely flagging exits, I impose the following restrictions:

1. The provider must see a minimum of a rolling average of X patients per year-month for all time periods prior to an exit,[28] and then exactly no patients thereafter. I calculate the "rolling average" as the average number of patients a provider saw over the past three months. Thus, the rolling average for a provider's patient count in July is the mean of the number of patients they treated in May, June, and July. This restriction is imposed to avoid incorrectly characterizing providers who only see patients irregularly (i.e. a few months out of the year) as having exited, while also allowing for the fact that providers actively involved in Medicaid may not be treating patients every month (for example, if they go on vacation, or if their patients don't schedule a visit).

Table J.59: NPI Counts with Different Min. Patient Thresholds

Min. Pts Cutoff	Num. NPIs
1	181684
2	66267
3	47726
6	29873
9	21977

[28] I create dummy year-months to fill in any gaps in treating patients between the first date of treating patient in a plan, to the last date. Thus, this restriction is imposed over all year-months a provider is observed to be involved in a plan, regardless of whether they see a patient in a particular year-month.

2. The potential exit date must be at least 3 months before the end of the year (December) of the final year that a state's data is available (e.g. to be flagged as an exiting provider in Indiana, the provider cannot have exited in November, 2013)

3. The provider must be in the plan for at least Z months.

Table J.60: Restriction 3: Provider Counts with Different Month Thresholds

Min. Months Cutoff	Num. NPIs
4	21972
5	21388
6	20729
7	19334

4. The potential exit date cannot be in the same year as a plan's exit

5. The potential exit data does not occur in the same year as when a particular plan is flagged as having insufficient counter data (e.g. if MCO A has notably insufficient encounter data in 2011, I will not flag any providers as exiting from MCO A in 2011) (this step is likely unnecessary given prior restrictions)

Table J.61 documents the number of NPIs at each step of the restriction criteria process enumerated above.

Table J.61: NPI Count by Criteria

Criteria	NPI Count	NPI-Plan Count
0	226504	916863
1	47726	80247
2	25181	41073
3	21388	35499
4	12687	20371
5	12687	20371

After imposing all restrictions, I identify a provider-plan exit rate of approximately 2% ($\frac{20,371}{916,863}$), which is a lower-bound estimate of all exits. Prior estimates have found exit rates of PCPs from Medicaid managed care plans at an annual rate of 12%; given the conservative nature of the restrictions imposed, it is not unreasonable to observe a much lower exit rate in this particular setting.

The final sample includes 12,687 NPI-plan exiters (20,371 distinct NPIs). Of these plan exits, 6,946 (34%) represent an exit from Medicaid entirely, given by Table J.62.

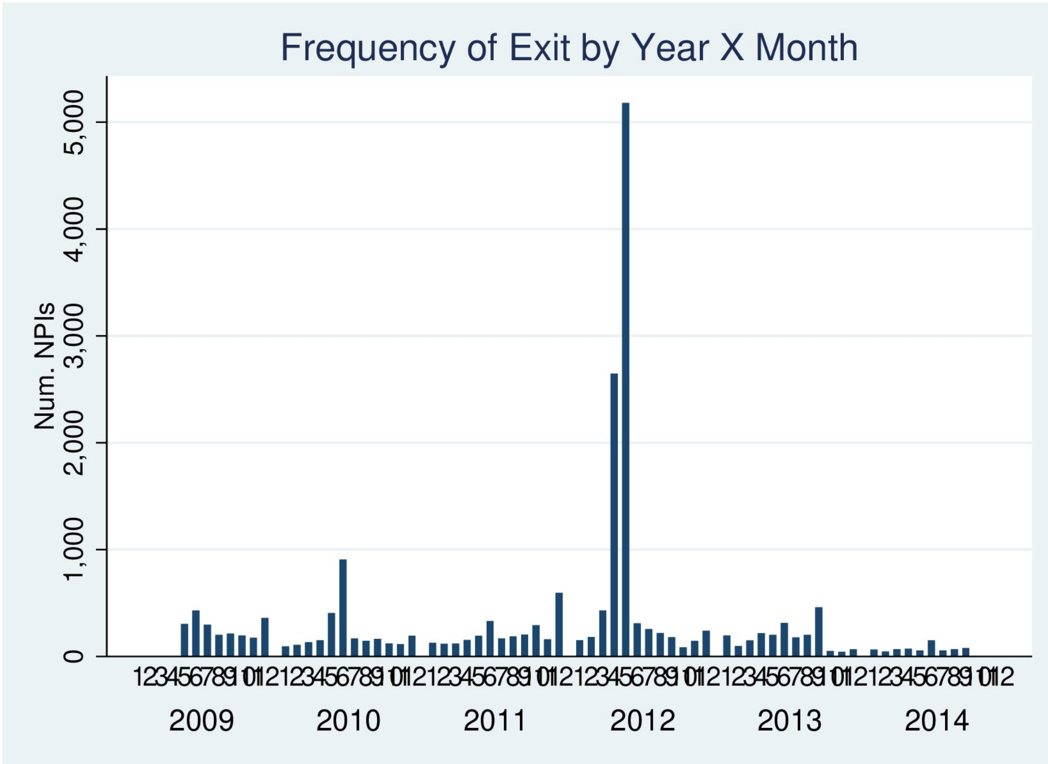
Table J.62: Percent of Plan Exits that Are Also Medicaid Exits

Exit from Medicaid?	Num. NPI-Plans	Total	Share
0	13425	20371	.66
1	6946	20371	.34

I define exits at the monthly level (as well as relative time from exit); for standardization, each provider's exit date from a particlar plan is given as month of exit, a day value of 1, and the year of exit. Thus, for a provider whose actual last date in a plan was June 15, 2012 (which I can't necessarily observe), their dummy exit date is June 1, 2012. I refer to this exit month in relative time as $m = 0$.

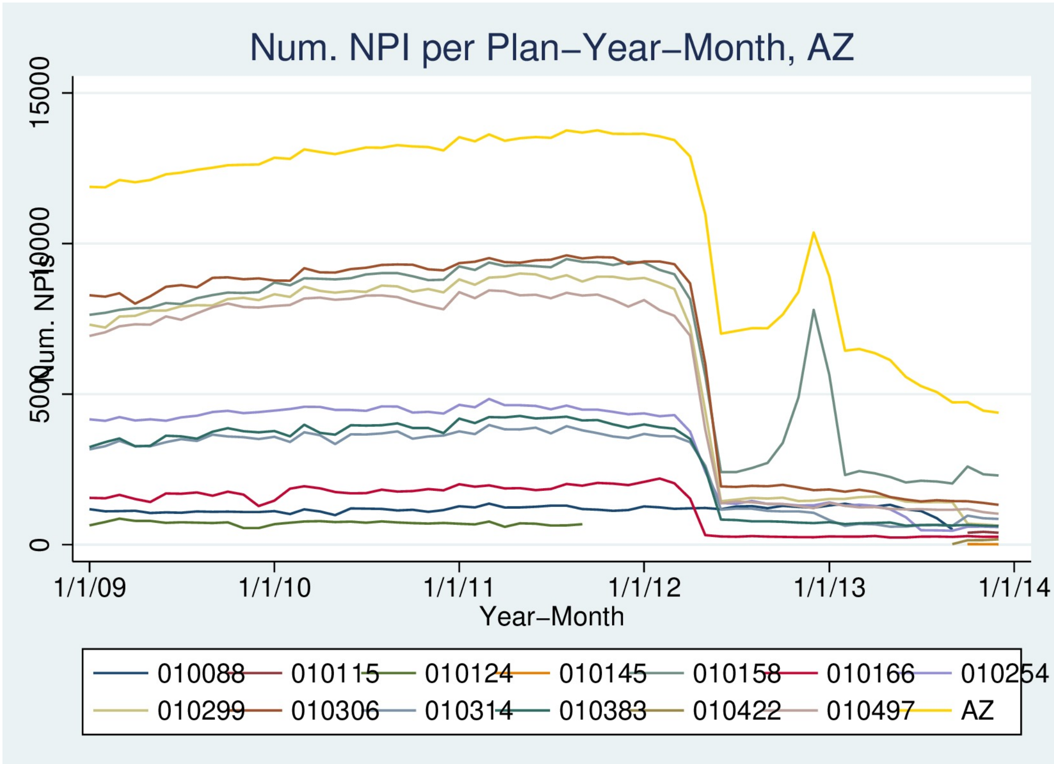
In Figure J.13, I plot the frequency of the month, year, and year-month of exit to determine if there are any patterns that might indicate that exits have been flagged with some systematic error.

Figure J.13: Exit Time Trends



I find some evidence of a calendar-month trend in exits (mid-year and end of year), which is not surprising. I also find that a large share of total exits occur in April and May of 2012. After confirming that this is not mechanical (i.e., these aren't full plans exiting the data in May 2012), I find that these are largely explained by what appears to be a state-wide policy change in physician membership in plans in April and May of 2012. Figure J.14 shows the number of NPIs associated with each plan in AZ per year-month. There is a notable fall-off in NPIs in April/May of 2012, both within plans and within the state. There is also a significant increase in NPIs in December 2012, in the plan Arizona Physicians IPA Public Sector Health Plan (APIPA).

Figure J.14: Number of NPIs in AZ Plans, per Year and Month



While this change in NPIs is significant and would seem to flag concerns that there is a mechanical error in identifying NPIs, it is notably a mid-year change. Since the LPI-NPI crosswalk is at the year level, a failure of the crosswalk would occur in the beginning of each year, unless AZ abruptly changed a provider's LPI mid-year. Even then, the rate of missing NPIs (per LPI) is relatively low in AZ, decreasing from 13% in 2009 to 9% in 2013 (see: Table J.53) before the crosswalk is applied. This makes it likely that the decrease in NPIs per plan (and subsequent large number of exits) reflects a policy change in Arizona. In my main analysis, I drop providers that have been flagged as exiters in April and May 2012 in Arizona.

After dropping Arizona exiters in April and May 2012, Figure J.15 and Table J.63 show the new exit time trends and percent of NPIs who exit Medicaid entirely, respectively.

Figure J.15: Exit Time Trends, After Dropping AZ (April and May, 2012)

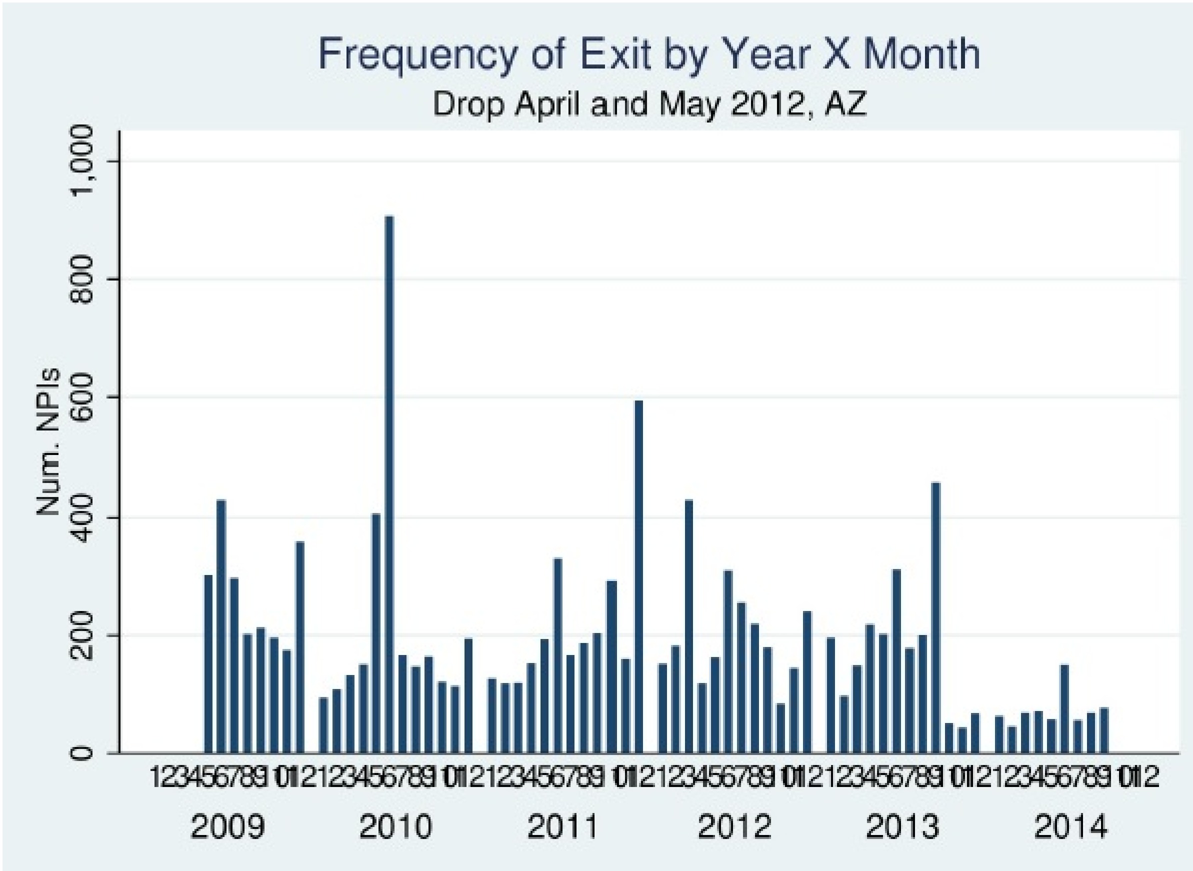
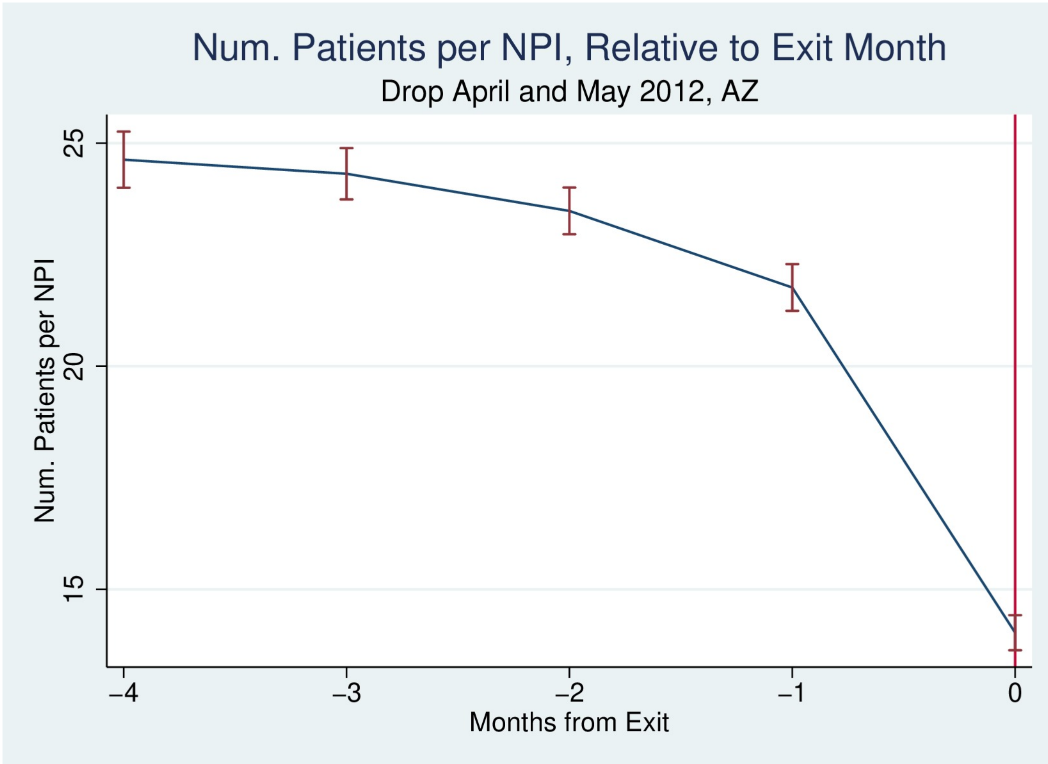


Table J.63: Plan v. Medicaid Exits, Dropped April and May 2012, AZ

Exit from Medicaid?	Num. NPI-Plans	Total	Share
0	6180	12825	.48
1	6645	12825	.52

Figure J.16 plots the average number of patients per provider relative to exit month, with confidence intervals, after dropping NPIs that were flagged as exiters in April and May 2012 in Arizona.

Figure J.16: Num. Patients Pre-Exit



There is a decrease in the number of patients in the actual month of exit where month=0, which is somewhat mechanical, since this is the last month a provider treats patients in a plan, but the exit can occur at any point during that month. Thus, for a provider who exits mid-way through the month, they will most likely have seen fewer patients, thereby bringing down the mean. This plot was generated using a regression with provider fixed effects. The overall trend suggests that the number of patients a provider sees in the months leading up to the exit are not statistically different, until the month of the exit itself (0).