

# The Intracorrelation of Family Health Insurance and Job Lock

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## Abstract

This paper tests for the presence of job lock and “health insurance plan lock” stemming from the health shock of a child family member. Using the onset of an acute, unanticipated health shock, I estimate a 7 – 14 percent decreased likelihood of all family members leaving their current health insurance network and health plan within one year of the emergency. This corresponds to a reduced one-year job mobility rate of approximately 13 percent for the health plan’s primary policyholder. Furthermore, the non-portability of health insurance products may contribute to the observed job and health plan lock.

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# 1 Introduction

Unexpected, individual-level health shocks can have far-reaching household implications (e.g. Charles (1999), Dobkin et al. (2018), Fadlon and Nielsen (2021)), including effects on employment decisions. Understanding the circumstances under which this occurs is particularly relevant in the US since household-level linkages are defining features of the health care system. This is because health insurance is often tied to the employment of an individual and *bundled* at the family level.<sup>1</sup> As such, a health shock could result in the job lock of another family member, where the family member is non-optimally locked to a job in order to ensure continuous health insurance coverage for themselves and other family members. (Madrian (1994)).<sup>2</sup>

In general, testing for and identifying job lock is challenging because it requires data that links both health and labor market outcomes. Furthermore, it is difficult to identify groups with a heightened need for health insurance access, such that their slowed rate of job change can be attributed to job lock versus another phenomenon. This paper revisits the question of job lock in a new context (transitory, acute health shocks) using a *big* US medical claims administrative dataset, consisting of nearly 15 million individuals who hold private insurance through a large commercial insurer.<sup>3</sup> In particular, job changes are proxied for using health insurance network changes in response to an as-good-as-random appendicitis health shock. This is because access to a specific health plan and health insurance network is employer-specific, captured by an employer-specific insurance group number. As such, health insurance network mobility can plausibly serve as a proxy for job mobility under certain circumstances.

More specifically, I test for the presence of job lock by examining whether the acute appendicitis emergency of a child family member induces lower rates of job exit for the family health plan's primary policy holder. I also explore the consequences of job lock on the health insurance outcomes of non-ill family members whose health insurance is tied to the health insurance plan and job of the primary policy holder. This approach bypasses some of the above-mentioned challenges in measuring and capturing job lock by leveraging the as-good-as-random and isolated onset of appendicitis as a clean source of variation that alters the need for continued access to health insurance. Further, while appendicitis may seem like a rare occurrence, it is the most common surgical emergency experienced

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<sup>1</sup>Illustrating the share of the population who are under the above described structure, the Current Population Survey (2014 - 2018) indicates that approximately 58 percent of people 64 years of age or younger have employer-sponsored health insurance, with approximately 76 percent belonging to a family health plan.

<sup>2</sup>For example, a family health plan could increase the cost of job switching for a health plan's primary beneficiary beyond what it would be if every family member had their own health plans.

<sup>3</sup>This estimate is as of 2018.

by children (Narsule et al. (2011)). Lastly, by examining the health shock of a child, who is neither a policy holder or financial contributor to the family, this study captures job lock tied to the need for health insurance. This allows for a more precise capture of job lock that is tied to the need for health care rather than the need to smooth income (e.g. due to a reduction in labor supply) around the time of a health shock (Fadlon and Nielsen (2021)).<sup>4</sup>

To measure the rate of insurance network switching after the health shock, I exploit features of the data structure; namely that, dropping out of the data is due either to switching to another insurance network (i.e. due to changing jobs or changing insurance networks conditional on remaining at the same job) or due to the complete loss of health insurance.<sup>5</sup> The latter is unlikely given that the health shock does not directly affect the primary beneficiary of the health insurance plan (i.e. an adult family member). Thus, dropout from the data is tantamount to switching insurance networks/dropping the current network.

Further, I exploit the fact that insurance network switching, outside of a Qualifying Life Event, is only traditionally allowed in certain months.<sup>6</sup> This allows for the construction of a measure of within-job, health plan switching after the health shock. Taken together with the network switching estimates, this allows me to construct a job lock estimate stemming from the health shock.

To measure the impact of the emergency on network exit, and its subsequent effects on job lock, I estimate stacked Difference-in-Difference models and compare the responses of family members exposed to appendicitis to a control group who are never exposed to an appendicitis emergency. The control group is constructed using a *coarsened exact matching* approach, where individuals exposed to an appendicitis health shock are paired with control individuals who enroll in the insurance network in the same month and year and who have a similar tenure profile, prior to the health shock. The idea here is that those who do not experience the emergency can be used to control for the natural rate of exit from the network that would have occurred in the absence of this health shock, while also accounting for the non-linear rate of dropout that varies over time spent in the data (i.e. network tenure). This matching procedure allows for the establishment of a

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<sup>4</sup>For example, since a child is not the primary beneficiary of a health plan, observed responses cannot be attributed to the direct impacts of the health shock to the labor supply of the adult. As evidence, according to 2011-2013 American Community Survey estimates, approximately 53 percent of children, ages 6 through 17 had employer-sponsored health insurance (American-Fact-Finder (2019)). Given the low rate of employment for this age group, ESHI must come from a family member/affiliate.

<sup>5</sup>Individuals who switch plans within the network will still be observed in the data.

<sup>6</sup>A Qualifying Life Event (QLE) is an event that triggers a special enrollment period that allows individuals to change their health plans outside of an open enrollment period. Examples include the birth of a child, marriage, and divorce. More examples of qualifying life events can be found at [healthcare.gov](https://www.healthcare.gov) (2020).

“placebo” emergency date and in doing so, creates a point-in-time benchmark to examine the natural rate of “churn” out of the insurance network in the absence of a health shock.<sup>7</sup> This approach provides an estimate of how exposure to the family emergency affects the likelihood of dropout from the network, controlling for the natural rate of exit that would have happened in the absence of the health shock.

The results indicate that the emergency health shock of a single family member leads to *lower* rates of health insurance network and health plan changes for all other family members.<sup>8</sup> Specifically, within three months of an emergency, families exposed to appendicitis are approximately 14 percent less likely to leave their current health plan, relative to the control group. After six months, families are 12 percent less likely to leave their current plan and after one year, this number is approximately 7 percent. This translates to a reduction in the one-year rate of job change of approximately 13 percent. Alternatively stated, for families exposed to appendicitis, the ensuing job lock reduces the one-year job switching rate of the primary plan policy holder by approximately 13 percent, compared to policy holders in families not exposed to the health shock.

Investigating the underlying mechanisms, I find evidence that switching costs may be a source for reduced network switching and the subsequent job lock. In particular, switching frictions may arise from the bundling of health insurance products (Farrell and Klemperer (2007)), namely the bundling of *non-portable* health reimbursement accounts (HRA) with health insurance plans. This is because an HRA is tied to both a specific employer and typically to a specific health insurance plan. Thus, it may be costly to forfeit money in the HRA in times of high current, or anticipated, medical expenses. In support of this mechanism, families belonging to health plans that are paired with an HRA are nearly 6 percent more likely to stay in their current plan and network within one year of the sudden health shock as compared to families whose health insurance plan is paired with a portable Health Savings Account (HSA). This suggests that the non-portability of an HRA may make it costly to switch health plans *and* employers.

Furthermore, the effect of the emergency on dropout rates is nearly identical across all family members. This result suggests that if families’ health plan selection and employment decisions are influenced by the health status of the sickest family member, this may not only affect job lock of adults but can also “lock” other family members into the employer-provided health plan. Thus, while the joint nature of health plan decisions at the household-level is assumed in many settings (e.g. Bundorf et al. (2012)), the findings of this study suggest that when modeling health plan selection at the household level, the

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<sup>7</sup>This matching procedure is related to the matching techniques used by Miller (2017) and Fadlon and Nielsen (2019).

<sup>8</sup>The current health plan is defined as the health plan held at the time of the emergency.

aggregation of individual family member’s household health risk should also account for health risks stemming from transitory health shocks, in addition to chronic diseases.

This work contributes to two distinct strands of literature. Firstly, this work contributes to the extensive literature examining job lock (Madrian (1994), Currie and Madrian (1999), Gruber and Madrian (2002)). While there are numerous studies that find evidence in support of job lock (e.g. Bansak and Raphael (2008), Garthwaite et al. (2014), Chatterji et al. (2016)), consensus on its existence is not conclusive (e.g. Kapur (1998), Berger et al. (2004), Bailey and Chorniy (2016)). This is due in part to the disparate settings that test for job lock. For example, many studies test for job lock by examining whether workers with a higher presumed need for employer-sponsored health insurance are less mobile due to circumstances, such as having a chronic illness (Stroupe et al. (2001)). Alternatively, it is also tested for by examining whether job attachment is lower for individuals who have outside insurance options, such as through a spouse (Royalty and Abraham (2006)).

This paper contributes to the job lock literature by showing that unanticipated, transitory health shocks are another source of job lock. Traditionally, most studies that examine the impact of health shocks on job lock have focused on chronic or ongoing diseases, such as cancer (Bradley et al. (2013)). As such, this study provides a lower bound for the impacts of health shocks on job lock, which is important to understand given the prevalence of acute and transitory emergencies.<sup>9</sup> Additionally, this study shows that even when accounting for the non-linear rate of job exit due to job tenure, a potential form of bias for job lock estimates discussed by Berger et al. (2004), there is still evidence of job lock. Lastly, this work introduces the concept of testing for job lock using a new data source: *big* administrative medical claims data. This is beneficial for examining how other adverse health events can affect job lock and for creating larger samples that are able to more precisely estimate job lock. It also allows for the linking of family units so as to analyze subsequent insurance outcomes among non-ill family members, something that has not typically been examined in prior work.

Secondly, this work contributes to the health insurance plan choice literature (e.g. Handel (2013), Handel and Kolstad (2015), Polyakova (2016)). In particular, this work complements the existing literature by showing that health reimbursement arrangements (HRAs) can be a source of friction in switching health insurance plans, and subsequently jobs. Thus, HRAs may enable job lock, even after laws meant to address this issue, such as the Health Insurance Portability and Accountability Act (HIPAA), were passed

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<sup>9</sup>For example, according to the Centers for Disease Control and Prevention (CDC), in 2018, there were approximately 130 million emergency department visits, of which approximately 27 percent were due to injury (Centers for Disease Control and Prevention (2021)).

(Madrian (1994)) and even after the expansion of public health insurance options, such as the Affordable Care Act (Bailey and Dave (2019)).<sup>10</sup>

Additionally, by documenting the high degree of correlation and persistence in network choice after a health shock, the findings of this study are informative for our understanding of adverse selection in health care markets (Akerlof (1978), Rothschild and Stiglitz (1978), Cutler and Zeckhauser (1998)). In particular, the results show that the high degree of correlation in health plan choice persists among family members, even after a health shock, effectively leading to a more balanced health plan risk pool. This occurs because healthier family members, who tend to have lower health risk, can effectively balance/offset the health risk associated with the sicker family members. This is an important consideration when examining the market inefficiencies associated with adverse selection or when considering the costs and benefits of family health insurance plans (e.g. Sinaiko et al. (2017)).

The rest of this paper will proceed as follows. Section 2 discusses the Research Design while Section 3 discusses the Data. Section 4 discusses the Empirical Strategy. Section 5 presents the Results while Section 6 concludes.

## 2 Research Design

In this section, I discuss the quasi-experiment used to analyze the impact of an acute health shock. I also discuss features of the data and the additional considerations it necessitates, as well as the construction of the control group. A more detailed description of the data is found in Section 3.

### 2.1 Quasi-Experiment: Appendicitis

The central challenge in identifying the impact of health shocks on insurance coverage is in establishing a plausible counterfactual. In an ideal setting, one would compare families who are similar in their propensity to leave their health plan (i.e. due to job switching), but for the occurrence of the health shock. This could be achieved through a randomized control trial where individuals are “randomly assigned” a health shock or instead by focusing on a case setting in which the shocks are considered to be as-good-as random. In the latter scenario, the unobservable factors influencing dropout should be similar across the general population, facilitating the construction of a plausible control group.

Acute appendicitis meets the criteria of being an as-good-as-random health shock. This is because the causes and origins of this condition are not well understood (Baird

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<sup>10</sup>For a more detailed discussion on the provisions of HIPAA, please see Lewin (1999).

et al. (2017) and its onset seems to occur with few discernible predictable risk factors.<sup>11</sup> As such, the observable characteristics across the families exposed to appendicitis and those who are not, should not systematically differ. This implies that the onset of this disease can be considered as essentially random in its occurrence and timing.

Furthermore, acute appendicitis is a condition that requires immediate medical attention. As such, its onset allows for the identification of families' responses to unanticipated health shocks. Additionally, "acute appendicitis is one of the most common general surgical emergencies worldwide, with an estimated lifetime risk reported to be 7–8 percent" (Bhangu et al. (2015), p.1278). Thus, the results of this analysis are more likely to extrapolate to a broader group given its non-negligible rate of occurrence in the general population.

## 2.2 Assumptions and Background for Control Group

A key feature of the dataset used in this analysis is that the majority of individuals have commercial, employer-sponsored health insurance (ESHI). This means that the reason for exit from the insurance network will likely be driven by two factors: 1) job switching/job loss and 2) switching health insurance plans, specifically into the health plan of a different insurance network.<sup>12</sup> The assumption that insurance network exit and plan changes can be largely contributed to job changes is based on evidence that individuals tend to be inert in changing health plans (Handel (2013)). Additionally, Cunningham and Kohn (2000) show that a major reason for changes to health plans are job changes - among those who changed health plans over a one year period, nearly 70 percent did so either because they changed employers or their current employer changed the plan offerings.

A necessary assumption in constructing an appropriate counterfactual for the appendicitis group is that there exists an identifiable comparison group that is similar in its propensity to exit the insurance network due to job switching and health plan switching, in the absence of a health shock. Given the evidence of inertia in health plan choice, discussed above, it is plausible that the latter condition can be achieved when comparing the appendicitis group and a hypothetical control group, particularly in a setting where the health shock is considered to be exogenous.

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<sup>11</sup>There is a slightly higher rate of appendicitis in males versus females and the "peak incidence usually occurs in the second or third decade of life, and the disease is less common at both extremes of age" (Baird et al. (2017), p. 1278). Additionally, Golz et al. (2020) finds geo-spatial variation in the incidence of certain kinds of appendicitis (perforated appendicitis) in Washington state.

<sup>12</sup>Changes to insurance networks and plans can occur during open enrollment periods or due to *Qualifying life events*. The latter are events that if experienced by an individual, allow individuals to change their health insurance network/plan outside of an open enrollment period, are also a possible reason for insurance network and plan switching. This will be further discussed in the Section 5.

To achieve, ex-ante, comparability in the job switching rates, this analysis constructs a control group from individuals who have been in the insurance network for a similar amount of time before the appendicitis group’s emergency date (i.e. they have a similar tenure) and who join a network health plan in the same year and month. The assumption underlying this approach for selecting eligible controls is that: 1) since there are low rates of within-job health plan switching (Handel (2013)), and thus low rates of network switching, insurance network tenure is a good proxy for job tenure and 2) the rate of network exit due to job switching is likely a non-linear function of the time period in which a job is joined (Oreopoulos et al. (2012)) and the amount of time already spent at the job (Copeland (2019), US Department of Labor (2018)). Thus, individuals who are at similar points in their job cycle are more likely to have similar rates of exit from the insurance network.

In support of the non-linearity of exit over time spent in the network (i.e. tenure), Figure 2a depicts the Kaplan-Meier survival curve for a sample of individuals who are not exposed to appendicitis. Similarly, Figure 2b depicts the distinct Kaplan-Meier survival curves for a sample of individuals who are not exposed to appendicitis, stratified by the years in which they joined the data.<sup>13</sup> Both figures depict the share of the people who still belong to a health plan within the insurance network (i.e. have “survived” in the data) at any given point in time,  $t$ . Of interest is the slope of the graphs. The slope, at a given time  $t$ , indicates the rate of exit for those who have survived up until that point. As shown, there is a higher rate of exit for smaller values of  $t$ , whereas there is a lower rate of exit at higher values of  $t$ . These figures suggest that the rate of network exit is non-linear in time and that the rate of exit may vary by an individual’s start-month and start-year in the data.

In short, the analysis assumes that the exit rate of families exposed to appendicitis can be approximated by the exit rate of families who are defined as belonging to the same cohort, but who themselves were not exposed to a health shock. A cohort is defined as individuals who join the dataset in the same year and month and who have survived in the data at least as long as the families exposed to appendicitis at the time of the appendicitis health shock. To be discussed in the next section, the control group will be constructed from the above described eligible cohort. Further, the control group will ultimately consist of households with two adults and at least one child to ensure comparability in family structure across the appendicitis group and control group.

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<sup>13</sup>The join dates and days of total tenure are chosen for illustrative purposes; the general conclusions still hold if these are varied.

## 2.3 Construction of the Control Group

The construction of the control group is achieved through a coarsened exact matching approach. Specifically, this analysis matches both the *distribution* of pre-emergency tenure and the initial month-year (month  $\times$  year) of insurance network enrollment of the appendicitis group.

To construct the control group, the analysis begins by selecting individuals who have never been exposed, directly or indirectly, to appendicitis during their time in the insurance network/tenure with the insurer, and who have a similar family structure as treatment group individuals (to be defined in the Data Section). The candidate control sample is then limited to individuals who are similar to the reference individual in the treatment group, where this reference person is the individual who directly experiences the appendicitis emergency. Similarity is defined as: 1) joining the sample in the same year and month and 2) having at least as many days of total continuous tenure before the emergency date of the paired treatment individual.<sup>14</sup>

A cross-pairing for every possible appendicitis group and eligible control combination is then formed such that the two previously mentioned requirements are met. Up to fifty of the cross-pairing matches are then selected at random and the emergency date of the treated reference individual is assigned to the paired control individual.<sup>15</sup> This assigned date is the *placebo emergency* date for the control group individuals, but for the purposes of the analysis, will be referred to as the emergency date. Finally, control families are identified as the control individual matched in the cross-pairing along with any other individual who shares the same insurance policy on the placebo emergency date.

The emergency date assigned to the control group serves as an event-time benchmark from which to examine the outcome of interest. This approach is similar to that of Fadlon and Nielsen (2019), Miller (2017) and Jeon and Pohl (2017) who establish pseudo-event dates among eligible controls as a means for constructing counterfactuals in dynamic difference-in-difference and event-study frameworks. Further, in this analysis setting, the control group is constructed such that the pre-emergency tenure *distribution* and the initial enrollment month-year of the appendicitis group is matched, rather than just the mean of these characteristics. This is evident in the histogram displayed in Figure 1, which shows the very similar distributions of tenure held before the emergency date for the appendicitis and control groups, respectively.

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<sup>14</sup>In practice, the latter condition requires that  $\text{floor}(\frac{T^C}{365}) \geq \text{ceiling}(\frac{T^T}{365})$ , where  $T^C$  is the total days of tenure held by a control individual and  $T^T$  is the number of days of tenure held before the onset of the appendicitis emergency for the treated individual (i.e. the pre-emergency tenure).

<sup>15</sup>The number of cross-pairings matches used is varied and tested in the Robustness Checks Section. Additionally, a control individual/family can be used as a match more than once across different emergency dates and for different treatment families.

### 3 Data Description

Data consists of information on medical claims for commercially insured individuals who have both health and prescription insurance coverage administered by a single payer. Data were obtained from Optum’s Clinformatics Data Mart Database.<sup>16</sup> The dataset spans the period from January 2003 to June 2019 and includes certain demographic information typically captured by insurers, such as state of residence, age, and gender of individuals. The data also includes the medical claims records for individuals with Medicare Advantage plans but does not include individuals who have Medicaid or traditional Medicare. As a testament to the size of the data, there are approximately 16 million individuals observed in the data in 2018.

#### 3.1 Analysis Sample: Appendicitis and Control Groups

The focus of this analysis is on health shocks stemming from the onset of acute appendicitis. To construct the appendicitis treatment group, I make several restrictions. First, the appendicitis sample is limited to individuals who: 1) experience their first diagnosis of non-fatal acute appendicitis and 2) are admitted to an emergency room or hospital for emergency or urgent reasons.

Furthermore, the individual experiencing the health shock must be a child, where a child is defined as an individual younger than 16 years of age at the time of the emergency.<sup>17</sup> Family units are then identified for these individuals where a family is defined as a group of two or more individuals who are linked together by a shared health plan subscriber number on the date of the emergency (i.e. these individuals are covered by the same health plan policy). Of note, the age threshold is chosen so as to minimize the likelihood of dropout owing to life transitions that may occur around 18 years of age, such as college attendance. Also, since the primary policyholder of the health plan cannot be identified in the data, this approach avoids the challenges that occur when trying to identify the mechanisms underlying the response to adult emergencies.<sup>18</sup> Note, while the primary policy holder cannot be identified in the data, it must be one of the two adults in the family unit.

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<sup>16</sup>*Optum* refers to Optum<sup>©</sup>’s de-identified Clinformatics<sup>®</sup> Data Mart Database. Optum<sup>©</sup>’s Clinformatics<sup>®</sup> Data Mart (CDM) is a statistically de-identified database of administrative health claims for members of a large national managed care company affiliated with Optum.

<sup>17</sup>Acute appendicitis incidents are captured by identifying International Classification of Diseases (ICD) codes that begin with either 540 (ICD-9) or K35 (ICD-10). Further, Current Procedural Terminology (CPT) Codes of 44950, 44960, and 44970 are used to identify individuals who have had an appendectomy.

<sup>18</sup>These challenges are namely that, the effect of the health shock (e.g. the sign of the treatment effect) will likely vary depending on whether or not it is the primary policyholder who experiences the health shock, as discussed in Bradley et al. (2007).

Common restrictions are made for both the treatment and control groups. These restrictions include that families must consist of two adult heads and one at least one child and families must not experience an emergency admission or hospitalization within one year prior to the emergency date.<sup>19</sup> The former is done in order to narrow in on the possible mechanisms influencing the insurance decisions observed in the data.<sup>20</sup> For example, since marital status is unknown, in households observed to have one adult, it is not possible to determine if families drop out to join the health plan of an unobserved family member. Thus, focusing on two adult households allows for better homing in on mechanisms behind the switching/non-switching of health plans. The latter restriction is made to ensure that the responses captured after the appendicitis emergency truly stem from that emergency and not a different health shock. Additionally, families where there are pregnancy-related claims in the year prior to the emergency are also excluded to ensure that network exit around the time of the emergency is not due to the birth of a child.

Second, the age of all family members is restricted to being below 63 years. This restriction is made in order to minimize the probability of drop out due to non-emergency reasons such as retirement or Medicare eligibility (at age 65). Third, the sample is limited to families in which all individuals have at least one year of insurance coverage (through the insurance network) prior to the emergency. This allows for the examination of pre-trends along non-insurance outcomes. Thus, emergencies must occur between January 2004 and May 2018, ensuring that there is one year of pre- and post-emergency data available for observation.

Table 1 shows that the treatment and control groups have very similar demographic characteristics. The average family consists of slightly fewer than five individuals, where the average across individuals in both groups ranges between 25 - 26 years. The sample is roughly evenly split along gender, although there are more males in the treatment groups (approximately 53 percent), consistent with the fact that the disease tends to have a slightly higher incidence in males (Craig and Brenner (2020)). Also, the pre-emergency tenure is quite similar across both the treatment and controls groups at approximately 1279 days (3.5 years) and 1198 days (3.3 years), respectively.

Importantly, general health, as proxied by the one-year Charlson comorbidity score

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<sup>19</sup>For the treatment group, this restriction is relaxed to allow for the inclusion of individuals/families where there is an observed emergency admission up to one day prior to the date of the appendicitis emergency. This allows for the inclusion of individuals who present to health care providers with problematic health before actually being diagnosed with appendicitis.

<sup>20</sup>It is ideal to observe the behavior of all members in the household; as such, a family structure is picked that best allows for that identification, since one cannot observe marital status/relationship status in the data.

(Charlson et al. (1987)) is highly similar across both groups.<sup>21</sup> Specifically, the share of individuals in both groups having zero comorbid conditions prior to the emergency is approximately 92 percent. The comparability of this statistic across both groups is consistent with the nature of appendicitis, whose determinants are still not well understood. Further, this statistic substantiates the plausibility that when focusing on an appendicitis shock, the treatment and control groups are likely to be comparable on health status. This matters, if, for example, individuals vary in their rates of job exit or health insurance plan exit by health status or health risk.

The majority of individuals in the treatment group live in the South (40 percent), followed by the Midwest (25 percent), with the fewest individuals living in the East. These shares are roughly the same in the control group. Additionally, the most common health plan held by the treatment group is a Point-of-Service (POS) plan, which is a hybrid between a health maintenance organization (HMO) and a Preferred Provider Organization (PPO) plan. This plan is held by approximately 72 percent of individuals in the treatment group. The next most commonly held plan is the Exclusive Provider Organization (EPO) health plans, which are also similar to a hybrid of a PPO and HMO plan, then HMO plans, followed by PPO plans. The ordered prevalence of health plan type is similar to the control group where the POS plans are the most prevalent, followed by EPO, HMO and PPO plans, respectively.

Lastly, the appendicitis emergency tends to be expensive. The average medical costs incurred by families on the day of the emergency are approximately \$1600. This stands in contrast to the average family-level expenses of approximately \$116 spent in the year prior to the emergency.

## 4 Empirical Strategy

To examine the effects of an emergency on insurance coverage and other outcomes, the following stacked Difference-in-Difference model is used:

$$y_{it} = \alpha + \sum_{k=-12, k \neq -1}^{11} \rho_k D_{it}^k + \sum_{k=-12, k \neq -1}^{11} \beta_k D_{it}^k \times T_i + \gamma T_i + X_i' \delta + \phi_t + \epsilon_{it} \quad (1)$$

$y_{it}$  measures insurance network exit/dropout. It takes the value of one if an individual maintains continuous insurance coverage through the insurance network over the entire

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<sup>21</sup>The look back period used to compute the Charlson comorbidity score is one year before the emergency.

interval (i.e. there is no dropout), where each interval is approximately 30 days long; it is zero, otherwise.<sup>22</sup> Since, the primary policy holder cannot be identified, I focus on the responses of all individuals ( $i$ ) in the family with the understanding that one of the adults in the household will be the health plan’s primary beneficiary. Also, because individuals can, in principle, exit the network on their own, I examine the responses at the individual-level, rather than the family-level.<sup>23</sup>

Time is normalized to event-time, such that  $t$  represents intervals until/since an emergency occurs ( $t = 0$ ).  $D_{it}^k$  takes the value of one when an individual is observed  $k$  intervals since the emergency (placebo emergency); it is zero otherwise.<sup>24</sup>  $T_i$  represents treatment. It is a binary variable taking the value one if an individual belongs to a family that experiences an emergency; it takes the value of zero if an individual belongs to the control group. The stacked approach is consistent with that of Cengiz et al. (2019) who note that, “By aligning events by event-time (and not calendar time), it is equivalent to a setting where the events happen all at once and are not staggered; this prevents negative weighting of some events that may occur with a staggered design as discussed in Sun and Abraham (2020).”

The model focuses on a two year event window - one year before and one year after the emergency. Thus,  $t \in [-12, 11]$ . This is done because the effect of a health shock is likely largest closest to the emergency date (Dobkin et al. (2018)). As mentioned, the observations and outcomes of interest are also aggregated over roughly 30 day intervals and may be interpreted as *monthly* responses.

The parameters of interest in this model are the  $\beta_k$ . For each  $k$ ,  $\beta_k$  gives the effect of exposure to an emergency on the probability of exiting the network (as compared to the control group), in the  $k^{th}$  interval since the emergency, relative to the interval right before the emergency

In order to interpret the  $\beta$  coefficients as the causal effect of experiencing an emergency on the outcomes of interest, two key assumptions are necessary. The first is that the parallel trends assumption must be valid. In other words, in the absence of an emergency, the emergency sample would have trended similarly in the outcome to the control group. Given that the control group is also limited to having insurance for at least one

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<sup>22</sup>The focus is on rolling 30 day windows since an emergency, with the last interval being 35 days long. This is done because people are unlikely to make job/health plan decisions based on calendar months and instead make decisions based on time since an emergency. Thus, using this rolling window allows one to better capture the immediate effects of the emergency since emergencies can occur in the middle or end of the month.

<sup>23</sup>To account for the intracorrelation of family-level decisions, I cluster standard errors at the family level.

<sup>24</sup> $D_{it}^k = \mathbb{1}(t = e_i + k)$ , where  $\mathbb{1}(\cdot)$  is the indicator function and  $e_i$  is the date where an individual is exposed to an emergency.

year prior to the placebo emergency and that there is a general overlap in demographic characteristics as presented in Table 1, this is plausible. Furthermore, in the next section, I compare the stability of pre-emergency medical spending, the number of medical claims made and the number of medical visits to help substantiate the validity of the parallel trends assumption.

The second necessary assumption is that, conditional on observable characteristics, the timing of the emergency is as-good-as-random. This is a very plausible assumption given the nature of appendicitis. In support of this assumption, Figure 3 shows the average number of claims and spending made on each day leading up to the emergency for individuals experiencing appendicitis. It clearly demonstrates that there is little medical activity except on the day of the emergency (i.e.  $t = 0$ ) and on the day immediately preceding the emergency.

$X$  represents a vector of covariates. This vector includes gender; the month the emergency occurs; the year the emergency occurs; the type of health plan held at the time of the emergency (e.g. PPO; HMO; EPO); whether the plan has an “add-on” account such as a Health Reimbursement Account or a plan that comes with a Health Savings Account; the state of residence at the time of the emergency; the days of tenure held before the emergency (i.e. pre-emergency tenure) and the family size at the time of the emergency (i.e. the number of family members observed under the health plan). The covariates also include a categorical variable indicating the Charlson comorbidity index of an individual (Charlson et al. (1987)). This is included as a proxy for health as it measures the one year comorbidity risk of individuals. Additionally, calendar month and calendar year fixed effects are included.

## 5 Results

### 5.1 Parallel Trends

Before presenting the regression results, the validity of the parallel trends assumption is examined. This is done by estimating Equation 1 in the time periods leading up to the emergency (i.e.  $t \in [-12, -1]$ ) across several medical utilization outcomes: medical spending/costs, number of claims made, and number of medical visits made over roughly 30 day periods.<sup>25</sup> <sup>26</sup>

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<sup>25</sup>Patient spending is equal to the sum of the deductible, co-insurance, and co-pay amounts paid by the patient for any health care received. Prices for these amounts are deflated to 2015 prices.

<sup>26</sup>Note, it is common to test the parallel trends assumption by examining value of pre-treatment outcomes (i.e. insurance network attachment). However, in this setting, since insurance coverage must be maintained for at least one year prior to the emergency in both the treatment and control group, this

Given the increase in utilization in the day leading up to the emergency, as shown in Figure 3,  $t = -2$  is used to represent the reference time period. Since medical utilization will likely vary depending on if one experiences the appendicitis emergency directly or not, I estimate Equation 1 separately for those individuals who directly experience the emergency (i.e. the affected) and for those individuals that are indirectly exposed due to family affiliation (i.e. the unaffected), comparing each group to the control group.

Figures 4 and 5 presents these results. They show that across all outcomes, medical utilization trend quite similarly before the onset of an emergency across all groups. The notable exception to this are the number of claims and visits made by the affected emergency group in the 30 day period preceding the emergency (i.e. when  $k = -1$ ). However, as noted, this increase likely stems from medical utilization changes in the day leading up to the emergency.

## 5.2 Main Results - Insurance Network Changes

The main results are presented in Table 2, which reports the coefficient estimates of  $\rho$  and  $\beta$  from Equation 1. Estimates of  $\rho$  capture general trends in insurance coverage dropout (i.e. leaving the health insurance network) while  $\beta$  captures the added effect of an appendicitis emergency on the likelihood of leaving the insurance network. These results are also graphically presented in Figure 6a. By construction, in the intervals leading up to the emergency (i.e. when  $t \in [-12, -1]$ ), there is no difference in the dropout rates across the emergency sample and control group.

The coefficient estimates in Table 2 indicate that an appendicitis emergency results in an overall *reduced* likelihood of leaving the insurance network. Within three months of an emergency, treatment families have a 1.1 percentage point higher probability of remaining in the insurance network. This represents an approximately 14 percent lower rate of exit from the network.<sup>27</sup> Furthermore, this effect persists over time. Six months after an emergency, families exposed to the appendicitis emergency have a 1.7 percentage point higher probability of remaining in the insurance network; this represents a 12 percent lower likelihood of exiting the health insurance network. Further, within one year of the emergency, families exposed to appendicitis experience a network retention rate that is approximately 2 percentage points higher, corresponding to an approximately 7 percent lower likelihood of exiting the insurance network.

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does not allow for inspection of parallel trends along this dimension.

<sup>27</sup>The rate of exit for the control group in period  $k$  is  $\rho_k$  and the rate of exit for the treatment group is captured by  $\rho_k + \beta_k$ . The magnitude of interest is  $\frac{\beta_k}{\rho_k} \times 100\%$ , which represents the change in the rate of network exit due to the appendicitis emergency.

### 5.3 Health Plan Switching

I also examine the rate of within-insurance network health plan switching after the appendicitis health shock. Alongside the results from section 5.2 which show reduced network exit after the appendicitis emergency, if individuals remain in the same health insurance plan after the emergency (conditional on remaining in the network), then the appendicitis emergency results in both reduced network exit and health plan switching.<sup>28</sup>

To determine the rate of health plan switching after the emergency, I calculate the share of treatment and control group members who switch from one network plan to another, conditional on continuing to remain insured through the insurance network for at least one year after the emergency.<sup>29</sup> This time frame is chosen as it provides a sufficient window of observation for families experiencing emergencies across the different calendar months to have had at least one open enrollment window in which to make changes to their health plans.

The plan switching shares are presented in Figure 7. While descriptive, the figure shows that there is relatively little plan switching for those who remain insured through the insurance network, which is consistent with the prior literature (e.g. Handel (2013)). For instance, conditional on remaining insured through the network 12 months after an emergency, 95.6 and 95.5 percent of the treatment and control groups maintain the same health insurance plan, respectively.<sup>30</sup> Thus, these results imply that the health emergency results in reduced health plan switching. This is because of the reduced insurance network exit that occurs after the health shock coupled with the near identical rates of plan switching conditional on remaining in the network.

### 5.4 Correlation in Family Insurance Coverage

To better understand how individual health shocks may lock other family members onto the family plan as a result of health insurance being provided through the employment of a single individual, I examine the degree of correlation in insurance coverage across family

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<sup>28</sup>This insight can be shown through an application of Baye's rule:  $P(A) = \frac{P(A|B) \times P(B)}{P(B|A)}$ . Let A be the event that a family switches health plans within the network within one year and let B be the event that a family stays in the network for at least one year. Then, for the treatment (T) and control group (C), because  $P(B|A)^T = P(B|A)^C = 1$  and  $P(A|B)^T \approx P(A|B)^C$  (to be discussed), then  $P(A)^T < P(A)^C$  since  $P(B)^T < P(B)^C$ .

<sup>29</sup>The analysis proxies for health plan switching by examining changes in the family's insurance policy number. Also, identification of within-network health plan switching is possible even if there is a job change by the primary policyholder, so long as the family elects a plan in the same insurance network after the job change.

<sup>30</sup>If considering a window of three months, these numbers are approximately 99 percent for both the treatment and control groups, conditional on remaining insured for at least three months after the emergency. The corresponding number for six months after an emergency, is approximately 98 percent for both treatment and control groups.

members. To determine this, Equation 1 is re-estimated where the “treatment” group is now defined as the family members directly experiencing the emergency and the “control” group are the family members who are exposed to appendicitis through family affiliation. Table 3 presents the coefficient estimates of  $\beta$ , which are also graphically displayed in Figure 6b. As shown in the figure, the average rate of network exit is essentially identical across family members in the periods after an emergency.

This finding illustrates that there is a high degree of correlation and persistence in health insurance network choice within a family unit. This suggests that the health shock also serves to “lock” the non-ill family members into their health plans. A-priori, it is unclear whether this harms or benefits family members. There may be medium/longer-run household welfare losses from reduced health plan switching if there are welfare-improving plans that are being passed over (Handel (2013)) and families remain locked in to their plans. Alternatively, the high degree of correlation in insurance coverage could mitigate the effects of health insurance plan sorting on perceived health risk. This is because more healthy family members are also likely to stay in the health plan, which may help balance the risk pool within an insurance plan. In turn, this could affect health plan premiums or the availability of certain health insurance plan offerings through an employer.

## 5.5 Further Job Lock Evidence

In this section, I discuss how the observed results can be explained by job lock. In particular, I show that the timing of network exit, where the timing of exit is correlated with the reason for exit, is consistent with slowed rates of job switching after the appendicitis emergency. I also estimate the size of the job lock induced by the health shock.

### 5.5.1 Slowed Rates of Job Change outside of Open Enrollment

I first examine the occurrence of reduced job change by examining the dropout effects by calendar month of the emergency. This is achieved by leveraging the fact that during the calendar months falling into Quarter-1 through Quarter-3, individuals are typically barred from making changes to their health plans unless they experience a qualifying life event as deemed by the Internal Revenue Service code (e.g. job changes, marriage, divorce, birth of a child, move to a different county).<sup>31</sup> This occurs because months in Q1-Q3 generally fall outside of an *open enrollment* window, where open enrollment periods allow individuals to freely change their insurance network and health plan. Open enrollment periods typically occur between October and December of a given year and the network and plan selections will typically be realized in January of the subsequent calendar year.

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<sup>31</sup>More examples of qualifying life events can be found at [healthcare.gov](https://www.healthcare.gov) (2020).

Of importance to the above discussion, the sample construction is also set-up such that network exit due to qualifying life events outside of job changes are limited (in months falling between Q1-Q3). This is because the treatment and control group both consist of households with two adult heads where there have been no pregnancy-related medical claims in the year prior to the health shock. Thus, network and plan changes owing to marriage and child-birth should not occur given the sample construction. Additionally, a qualifying life-event, such as divorce, is not substantiated by the results since similar rates of network retention are found across individuals within a family (where a divorce would lead to differential rates of exit). Lastly, while county-level movement is not testable in the data, I find that the state of residence remains stable at relatively similar rates across both the treatment (95 percent) and control groups (94 percent), conditional on remaining in the network for at least one year after the emergency. This lends support to the assertion that most network exit, during non-open enrollment months, is likely due to [the lack of] job movement after the health shock.

To empirically test for the occurrence of slowed job change rates for the primary policy holder stemming from the health shock of a child, I re-estimate Equation 1 separately for each calendar month in which an emergency occurs. The results are presented in Figures 9 and 10 and trace out the effects of the health shock for emergencies occurring in calendar months January through November.<sup>32</sup> The findings indicate that in the months after an emergency, when the open enrollment option is not likely to be present, dropout rates from the insurance network are between 0 – 4 percentage points lower among the families exposed to the appendicitis health shock, depending on the month of the emergency. For example, among families experiencing an emergency in February, within three to ten months of the emergency, families exposed to the appendicitis health shock are 2 to 5 percent more likely to stay in the network. Of note, the standard errors for the  $\beta$  coefficient estimates are larger when examining the month-by-month response, which likely stems from the smaller within-month sample sizes. However, the generally positive coefficient point estimates are still illustrative and suggest that the person who is the primary policyholder of the health plan is less likely to switch jobs as a result of a family member’s health shock. As a result, the family unit remains insured through the same network and plan.

### 5.5.2 Job Lock Estimate

I next estimate the magnitude of job lock induced by the health shock. To do this, I estimate the share with same employer one year after the emergency for each calendar

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<sup>32</sup>December is not included since the  $t = 0$  time period includes dropout in January since the time interval is a rolling 30 day period from the initial emergency date.

month in which the emergency occurs<sup>33</sup>:

$$\text{Share with same employer 1-year after the emergency} = A + (1 - A) \times B$$

Here,  $A$  represents the share of families who stay in the network for at least one year after the emergency and  $B$  represents the share who exit the network for a new health plan conditional on remaining at the same job.  $A$  can be directly calculated in the data.  $B$  is estimated by calculating the share of families in the data who switch health insurance networks, but who remain at the same job. To determine this, I examine dates of network exit in relation to the traditional open enrollment period. Specifically, I assume that if the last date of an individual's network enrollment falls on the last date of the calendar year, December 31, these individuals exit the network and join a new health insurance network and health plan, *conditional* on remaining at the same job. This approach is taken because new health plan elections, made during the previous open enrollment period, tend to be realized on the first calendar day of the new year, January 1. As such, it is plausible that a network exit date on the last date of the calendar year is indicative of new network/plan switching alone, assuming that typical job quit dates do not tend to occur on this same day (i.e. job separation dates are continuous around December 31).

Under this scheme, approximately 85.5 percent of the treatment group remains at the employer for at least one additional year. In contrast, approximately 83.3 percent of the control group remains at their employer for at least one additional year after the emergency. This implies an estimated reduction in job mobility of approximately 13 percent.<sup>34</sup> This estimate is slightly smaller than what has been typically found in the job lock literature. Within this literature, upper end estimates of job lock vary between 25 percent and 40 percent (e.g. Madrian (1994), Stroupe et al. (2001), U.S. Government Accountability Office (2011)). However, these differences are likely due to the disparate settings in which job lock has been previously estimated alongside sample construction differences. For example, I examine an acute health shock experienced by a child versus other studies that examine the health shock of another adult (e.g. chronic disease) or those that examine how job change rates are affected by the presence of outside health insurance options. The former scenario might produce larger income shocks that result in the need to smooth income (Fadlon and Nielsen (2021)), which may have its own effect on the labor supply of adults and their propensity to switch jobs. Additionally, this

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<sup>33</sup>To get an aggregate, one-year estimate of job lock, I calculate the weighted sum of job lock across the emergency months.

<sup>34</sup>The calculation for this estimate is:  $\frac{(100-85.5)-(100-83.3)}{(100-83.3)}$ .

study matches treatment and control groups on their tenure in the network. Given the non-linear rate of exit over time, not accounting for this could lead to biased estimates of job lock if the control group consists of individuals who are in the earlier part of their job-tenure life cycle where job switching rates tend to be higher (US Department of Labor (2018)).

## 5.6 Mechanisms

To examine factors that could contribute to job lock, this analysis focuses on a well-defined and observable feature of health plans. Specifically, I focus on whether a health plan is associated with a portable “savings” account or not.

Health insurance plans can have additional add-on “savings” accounts, which can be used to pay for qualifying medical expenses. These accounts can take the form of either a Health Reimbursement Arrangement (HRA) or a Health Savings Account (HSA), and are usually paired with a high-deductible health insurance plan. Both employers and employees can contribute to an HSA account, while contributions to the HRA account are exclusively made by the employer (Tax Policy Center (2020)).<sup>35</sup>

A key feature of HRA accounts is that they are employer funded and are generally not portable across health plans nor employers. For example, if an employee switches from a high deductible health plan to a non-high deductible health plan, the money held in the HRA would typically be lost. Similarly, if an employee switches employers, the money held in the HRA would likely be lost as well. Thus, I examine whether the non-portability of health-associated savings accounts, disincentivize plan switching, and subsequently job switching. This could occur if high current medical expenses and/or anticipated medical expenses make it more costly to forfeit the money held in an HRA.

To address this question, I re-estimate Equation 1 separately across families who, prior to the appendicitis emergency, belong to a health plan paired with an HRA or a health plan paired with an HSA. These groups are likely to be similar, in the absence of the health shock since both are likely to belong to a high-deductible health plan. As a result, they will face similar financial expenses associated with the health shock, where the expense might influence the network exit decision (e.g. if families are liquidity constrained, the money held in the add-on accounts may be costly to forfeit).

The results are presented in Table 4 and Figure 8. Among families exposed to the appendicitis health shock, those who belong to the HRA plan at the time of the

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<sup>35</sup>The characteristics of enrollees across the three health plans are shown in Table 6. They are quite similar across age, family size and gender shares. However, HRA and HSA groups tend to be in the data longer than health plans with no paired account. Additionally, HSA enrollees tend to be slightly healthier, on average, as indicated by the higher share of individuals who have a zero comorbidity score.

emergency are much less likely to leave the insurance network after the emergency than those belonging to a plan with an HSA. For example, within three months of the health shock, families enrolled in an HRA have an approximately 3 percentage point higher probability of remaining in the insurance network compared to families with an HSA. This corresponds to an approximately 46 percent lower likelihood of exiting the network compared to treatment group families enrolled in an HSA. After 12 months, this number is approximately 3.3 percentage points (14 percent). These findings stand in contrast to those of control group families, where the difference in network exit rates across families holding an HRA vs. an HSA is essentially identical, over time, as observed in Figure 8. The differential insurance network retention rates observed across individuals with HRAs versus HSAs, among families exposed to appendicitis, is especially interesting given that both groups incur similar average patient expenses for the emergency (approximately \$2500-\$2800). This implies that differential expenses associated with the appendicitis emergency do not drive these results. Additionally, similar to the results presented in section 5.3, those who hold HRAs and HSAs at the time of the emergency exhibit little within-network plan switching conditional on remaining insured through the network for at least one year.

While these results are suggestive and apply to the share of the treatment group belonging to an HRA, they show that a distinct source for reduced health network and health plan switching may be the bundling of health insurance products (i.e. a health insurance plan and a non-portable paired “savings” account). Specifically, it may be more costly for families to forfeit money held in an HRA particularly when faced with high current medical expenses or if anticipating higher medical expenses in the future. This finding is consistent with Farrell and Klemperer (2007) who discuss product bundling, and the associated pecuniary costs, as a source for reduced switching across consumer products. It is also consistent with Lamiraud and Stadelmann (2020) who show that lower priced supplementary health care products, paired with basic health insurance, are associated with lower switching across Swiss health insurance plans.

Thus, while an appendicitis health shock may result in reduced health insurance network and health plan mobility, certain features of health insurance products, such as non-portable HRAs may amplify this response. As shown, this may result in a form of health plan lock, where people are more likely to stay in their health plan after a health shock, *and* job, since these health plans are tied to a specific employer.

## 5.7 Robustness Checks

To examine the robustness of the results, I re-estimate Equation 1 using a subset of the data that trims outlier families who have fewer than the 5th percentile (p5) and greater than the 95th percentile (p95) of matched control families. This is done because of heterogeneity in the number of corresponding matches for each treatment family, that tends to be correlated with the pre-emergency tenure of the treatment family. Thus, by examining the p5-p95 subset, this should limit the scope of potential biases caused by the overrepresentation of control families with lower pre-emergency tenure. This procedure results in 337 fewer treatment families and 8,954 fewer control families in the analysis.

The results of this analysis are presented in Table 5. These results are highly comparable to the main results in Table 2, suggesting that the main results appropriately capture the effects of the appendicitis health shock on insurance coverage outcomes.

## 6 Conclusion

This study examines how an individual-level adverse health shock affects the job lock and health plan lock of other family members. This is achieved by examining how the onset of acute appendicitis among a child family member affects health insurance plan decisions, and subsequent employment decisions, for families who belong to a large, national health insurer. Using a constructed control group and stacked difference-in-difference models, this study finds that the onset of acute appendicitis leads families to reduce their likelihood of switching health plans between 7 – 14 percent within one year of the emergency. Additionally, health plan switching rates are near identical across all family members exposed to the emergency. This translates to a reduction in the one-year job change rate of approximately 13 percent for the health plan’s primary policy holder.

Thus, the results of this study demonstrate that job lock is still present in labor markets, even after the passage of laws such as the Affordable Care Act. Furthermore, job lock can be triggered by acute, transitory health shocks, and not just chronic diseases. Additionally, the results demonstrate a specific form that family spillovers may take in response to the acute, transitory health shock of a family member. These findings have important policy implications. In particular, the high degree of correlation in plan choice among family members, in the periods after a health shock, is an important finding in light of long-standing evidence that individuals sort into health plans based on health risk type (i.e. adverse selection). These results show that, under certain scenarios, the high degree of correlation in plan choice among family members may partially alleviate concerns about the sorting of individuals into health plans based on their health risk.

This is because healthy family members continue to stay in the same health plan, which can offset the riskier health profile of the sicker family member.

Additionally, this study finds that one source for reduced job switching may be the non-portability of certain health plan products that are paired with health insurance plans. In particular, the non-portability of a health reimbursement arrangements (HRAs) may make health plan and job switching more costly at the time of an expensive health shock. This finding suggests that certain characteristics of the current health insurance market can contribute to “health plan lock,” which, in turn, affects job mobility. This is an important consideration given that policies meant to affect job lock, such as the Health Insurance Portability and Accountability Act (HIPAA), were passed in the mid-1990s.

Future work should explore alternate pathways by which transitory health shocks affect employment decisions. For example, there may also be behavioral explanations such as increased insurance product salience that makes individuals more highly value the features of their health plan (post-emergency). This examination is beyond the scope of this work but is a fruitful area for future research.

More generally, the findings of this study show that that adverse health events impact not only the individual who experiences the emergency, but also their affiliated family. As such, this study shows that it is important to examine the entire household in order to understand the full implications of adverse health events. Furthermore, the results can provide insight on how the current health insurance market structure, which bundles health insurance at the family level, may affect the efficient functioning of both health insurance and labor markets.

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Table 1: Summary Statistics

	<i>Treatment</i>	<i>Control</i>
	(1)	(2)
Average age	25.60	25.31
Average family size	4.80	4.75
Pre-emergency tenure	1279.03	1197.87
Share Male (%)	53.1	50.6
Share w/Charlson Comorbidity Score = 0	92.00	91.74
Share HMO (%)	11.44	13.06
Share PPO (%)	4.85	5.01
Share POS (%)	71.65	69.95
Share EPO (%)	11.90	11.77
Share East Coast (%)	11.56	10.93
Share Midwest (%)	25.46	25.28
Share South (%)	40.02	40.85
Share West Coast (%)	22.76	22.39
Share directly experiencing emergency (%)	21.81	-
Day-of-Emergency Spending	\$1613.42	-
Number of Individuals	21,246	605,517

Note: This table reports summary statistics for the individuals exposed to an appendicitis health shock (column 1) and the control group (column 2) at the time of the emergency (placebo emergency). Regions are defined by state groupings according to the US Census Bureau Census Regions (Census Bureau (2020)).

Table 2: Main Regression Estimates - Effect of Appendicitis Emergency on Insurance Coverage

	Full Interval Coverage		
	Main (1)	No Cov (2)	F.e. (3)
<hr/> <b>General effect (rel. to -1), <math>\rho_k</math></b> <hr/>			
<i>Intervals since emergency</i>			
0	-0.027*** (0.000)	-0.027*** (0.000)	-0.027*** (0.000)
1	-0.051*** (0.001)	-0.051*** (0.001)	-0.051*** (0.001)
2	-0.075*** (0.001)	-0.075*** (0.001)	-0.075*** (0.001)
3	-0.099*** (0.001)	-0.099*** (0.001)	-0.099*** (0.001)
4	-0.123*** (0.001)	-0.123*** (0.001)	-0.123*** (0.001)
5	-0.144*** (0.001)	-0.144*** (0.001)	-0.144*** (0.001)
6	-0.168*** (0.001)	-0.168*** (0.001)	-0.168*** (0.001)
7	-0.189*** (0.001)	-0.189*** (0.001)	-0.189*** (0.001)
8	-0.210*** (0.001)	-0.210*** (0.001)	-0.210*** (0.001)
9	-0.230*** (0.001)	-0.230*** (0.001)	-0.230*** (0.001)
10	-0.249*** (0.001)	-0.249*** (0.001)	-0.249*** (0.001)
11	-0.269*** (0.001)	-0.269*** (0.001)	-0.269*** (0.001)
<hr/> <b>Added effect of treatment (rel. to -1), <math>\beta_k</math></b> <hr/>			
<i>Intervals since emergency</i>			
0	0.002 (0.002)	0.002 (0.002)	0.002 (0.002)
1	0.006* (0.003)	0.006* (0.003)	0.006* (0.003)
2	0.011*** (0.004)	0.011*** (0.004)	0.011*** (0.004)
3	0.010** (0.004)	0.010** (0.004)	0.010** (0.004)
4	0.015*** (0.005)	0.015*** (0.005)	0.015*** (0.005)
5	0.017*** (0.005)	0.017*** (0.005)	0.017*** (0.005)
6	0.016*** (0.005)	0.016*** (0.005)	0.016*** (0.005)
7	0.017*** (0.006)	0.017*** (0.006)	0.017*** (0.006)
8	0.021*** (0.006)	0.021*** (0.006)	0.021*** (0.006)
9	0.019*** (0.006)	0.019*** (0.006)	0.019*** (0.006)
10	0.020*** (0.006)	0.020*** (0.006)	0.020*** (0.006)
11	0.020*** (0.007)	0.020*** (0.007)	0.020*** (0.007)
<hr/> <b>Number of individuals</b> <hr/>			
	590,613	590,613	590,613

1: Data consists of medical claims data pooled from 2003-2019.

2: General effect estimates represent coefficient estimates of  $\rho$ , while Added effect of treatment estimates represent coefficient estimates of  $\beta$  from Equation (1). By construction, the difference in coverage is constrained to be 0 between comparison groups when  $k \in [-12, -1]$ . *Treatment* takes the value of one if an individual belongs to families who are actually exposed to an emergency and is zero if they belong to the control group. Each interval represents an approximately 30 day rolling window since the emergency.

3: Column 1 include all demographic covariates while columns 2 excludes covariates. Column 3 presents results from a fixed-effect estimation estimated at the individual, (patient-id) level. All results presented are OLS estimates.

4: Standard errors are clustered at the family-id level– 139,029 clusters.

5: Level of statistical significance: \*\*\*1%; \*\*5%; \*10%.

Table 3: Main Regression Estimates - Subgroup Effect of an Emergency on Insurance Coverage

Full Interval Coverage	
Directly Affected vs. Indirectly Affected	
(1)	
General effect (rel. to -1), $\rho_k$	
<i>Intervals since emergency</i>	
0	-0.025*** (0.002)
1	-0.045*** (0.003)
2	-0.064*** (0.004)
3	-0.090*** (0.004)
4	-0.108*** (0.005)
5	-0.129*** (0.005)
6	-0.153*** (0.005)
7	-0.174*** (0.006)
8	-0.190*** (0.006)
9	-0.212*** (0.006)
10	-0.230*** (0.006)
11	-0.251*** (0.006)
Added effect of treatment (rel. to -1), $\beta_k$	
<i>Intervals since emergency</i>	
0	0.002** (0.001)
1	0.001 (0.001)
2	0.002 (0.001)
3	0.004*** (0.001)
4	0.004*** (0.002)
5	0.005*** (0.002)
6	0.006*** (0.002)
7	0.006*** (0.002)
8	0.006*** (0.002)
9	0.006*** (0.002)
10	0.005*** (0.002)
11	0.006*** (0.002)
Number of individuals	21,246

1: Data consists of medical claims data pooled from 2003-2019. The data is limited to treatment group individuals.

2: General effect estimates represent coefficient estimates of  $\rho$ , while Added effect of treatment estimates represent coefficient estimates of  $\beta$  from Equation (1). By construction, the difference in coverage is constrained to be 0 between comparison groups when  $k \in [-12, -1]$ . *Treatment* takes the value of one if an individual experiences the emergency themselves (i.e. the individual is affected) and is zero if they have a family member who experiences the emergency (i.e. the individual is unaffected). Each interval represents an approximately 30 day rolling window since the emergency.

3: Column 1 includes all demographic covariates. All results presented are OLS estimates.

4: Standard errors are clustered at the family-id level– 4,602 clusters.

5: Level of statistical significance: \*\*\*1%; \*\*5%; \*10%.

Table 4: Insurance Coverage Estimates Comparing Plans with HRA vs. HSA

	Full Interval Coverage	
	HRA vs. HSA (Treatment Group) (1)	HRA vs. HSA (Control Group) (2)
General effect (rel. to -1), $\rho_k$		
<i>Intervals since emergency</i>		
0	-0.022*** (0.003)	-0.025*** (0.001)
1	-0.051*** (0.004)	-0.045*** (0.001)
2	-0.065*** (0.004)	-0.066*** (0.001)
3	-0.087*** (0.005)	-0.088*** (0.001)
4	-0.097*** (0.005)	-0.110*** (0.001)
5	-0.113*** (0.005)	-0.132*** (0.001)
6	-0.142*** (0.006)	-0.152*** (0.001)
7	-0.155*** (0.006)	-0.174*** (0.001)
8	-0.171*** (0.006)	-0.192*** (0.001)
9	-0.195*** (0.007)	-0.212*** (0.001)
10	-0.212*** (0.007)	-0.231*** (0.001)
11	-0.232*** (0.007)	-0.251*** (0.002)
Added effect of treatment (rel. to -1), $\beta_k$		
<i>Intervals since emergency</i>		
0	0.015*** (0.003)	-0.002** (0.001)
1	0.030*** (0.005)	-0.003** (0.001)
2	0.030*** (0.007)	-0.003** (0.001)
3	0.030*** (0.008)	0.001 (0.002)
4	0.018** (0.009)	-0.002 (0.002)
5	0.019** (0.010)	0.002 (0.002)
6	0.024** (0.010)	-0.003 (0.002)
7	0.004 (0.011)	-0.001 (0.002)
8	0.012 (0.012)	-0.004* (0.002)
9	0.016 (0.012)	0.000 (0.002)
10	0.026** (0.013)	0.000 (0.003)
11	0.033** (0.013)	0.000 (0.003)
Number of individuals	4,774	122,717

1: Data consists of medical claims data pooled from 2003-2019 for treatment and control groups. Regressions are separately estimated by health plan type, prior to the emergency (placebo emergency).

2: General effect estimates represent coefficient estimates of  $\rho$ , while Added effect of treatment estimates represent coefficient estimates of  $\beta$  from Equation (1). By construction, the difference in coverage is constrained to be 0 between comparison groups when  $k \in [-12, -1]$ . The *Treatment* dummy takes the value of one if an individual belongs to a health plan paired with a Health Reimbursement Arrangement (HRA) and is zero if they belong to a health plan paired with a Health Savings Account (HSA). Estimates are performed separately for families exposed to the health shock and for families not exposed. Each interval represents an approximately 30 day rolling window since the emergency.

3: Standard errors are clustered at the family-id level— 1,026 and 28,558 clusters, respectively.

4: Level of statistical significance: \*\*\*1%; \*\*5%; \*10%.

Table 5: Main Regression Estimates - p5 – p95 Subgroup Effect of an Emergency on Insurance Coverage

Full Interval Coverage	
<u>Main</u>	
(1)	
General effect (rel. to -1), $\rho_k$	
<i>Intervals since emergency</i>	
0	-0.027*** (0.000)
1	-0.051*** (0.001)
2	-0.075*** (0.001)
3	-0.099*** (0.001)
4	-0.123*** (0.001)
5	-0.144*** (0.001)
6	-0.168*** (0.001)
7	-0.189*** (0.001)
8	-0.210*** (0.001)
9	-0.230*** (0.001)
10	-0.249*** (0.001)
11	-0.270*** (0.001)
Added effect of treatment (rel. to -1), $\beta_k$	
<i>Intervals since emergency</i>	
0	0.003 (0.002)
1	0.007** (0.003)
2	0.012*** (0.004)
3	0.010** (0.004)
4	0.016*** (0.005)
5	0.017*** (0.005)
6	0.017*** (0.006)
7	0.017*** (0.006)
8	0.022*** (0.006)
9	0.020*** (0.006)
10	0.021*** (0.007)
11	0.021*** (0.007)
Number of individuals	553,481

1: Data consists of medical claims data pooled from 2003-2019. The data is limited to individuals in treatment families whose number of matched control families fall between p5-p95 of available control families, as well as the the individuals associated with these control families.

2: General effect estimates represent coefficient estimates of  $\rho$ , while Added effect of treatment estimates represent coefficient estimates of  $\beta$  from Equation (1). By construction, the difference in coverage is constrained to be 0 between comparison groups when  $k \in [-12, -1]$ . *Treatment* takes the value of one if an individual belongs to a family exposed to appendicitis and zero if they belong to the control group. Each interval represents an approximately 30 day rolling window since the emergency.

3: Column 1 includes all demographic covariates. All results presented are OLS estimates.

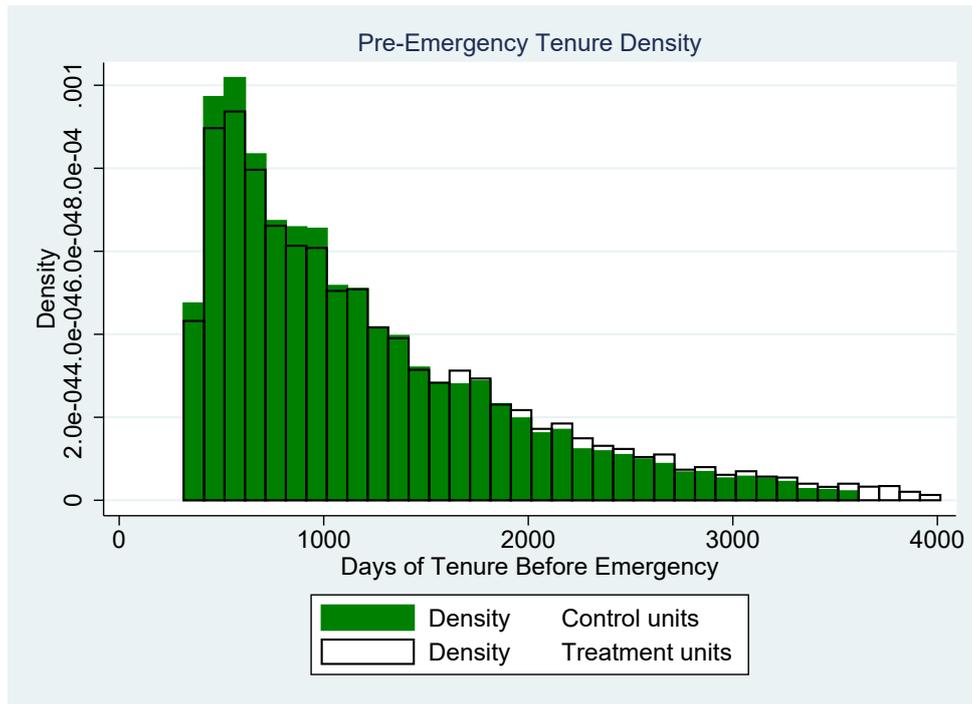
4: Standard errors are clustered at the family-id level– 129,780 clusters.

5: Level of statistical significance: \*\*\*1%; \*\*5%; \*10%.

Table 6: Data Summary by Health Plan Type

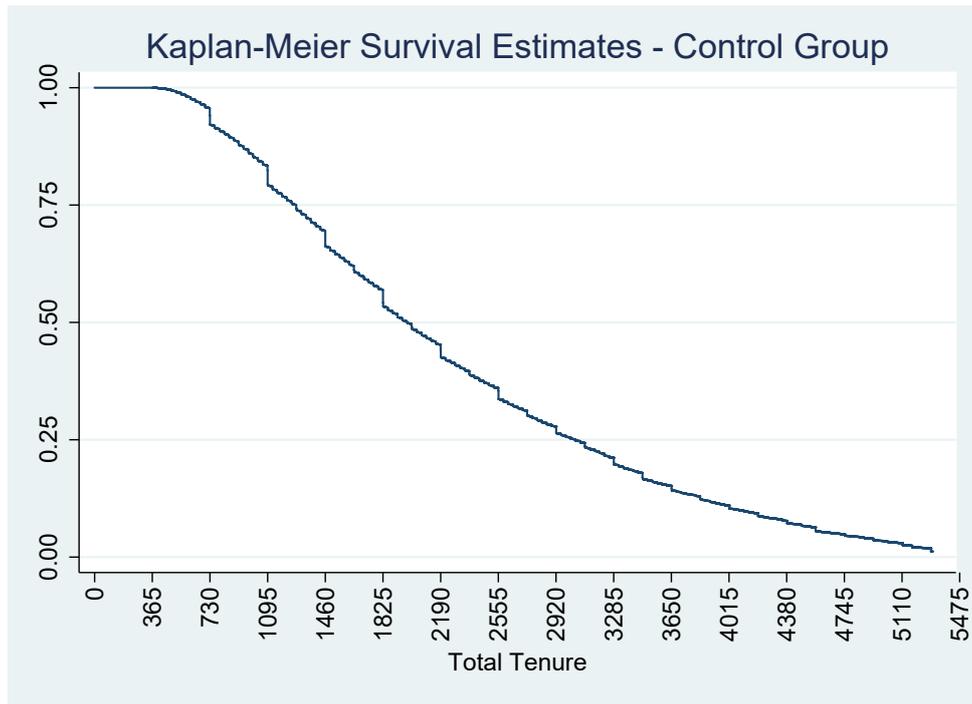
<b><i>HRA + health plan</i></b>	Treatment	Control
Average age	26.38	25.75
Average family size	4.70	4.71
Pre-emergency tenure	1395.81	1286.13
Share Male (%)	52.90	50.73
Share w/Charlson Comorbidity Score = 0	92.42	92.02
Share HMO (%)	0.00	0.31
Share PPO (%)	2.69	5.20
Share POS (%)	93.84	90.60
Share EPO (%)	3.47	3.89
Share East Coast (%)	9.14	7.39
Share Midwest (%)	25.57	26.14
Share South (%)	48.16	48.81
Share West Coast (%)	17.14	17.60
Share directly experiencing emergency (%)	22.24	-
Day-of-Emergency Spending	\$2556.40	-
Number of Individuals	1,412	43,981
<b><i>HSA + health plan</i></b>	Treatment	Control
Average age	25.39	25.23
Average family size	4.99	4.85
Pre-emergency tenure	1380.03	1270.68
Share Male (%)	52.29	50.60
Share w/Charlson Comorbidity Score = 0	94.59	94.16
Share HMO (%)	1.25	1.39
Share PPO (%)	1.10	1.76
Share POS (%)	94.62	94.22
Share EPO (%)	3.03	2.63
Share East Coast (%)	8.39	9.25
Share Midwest (%)	32.78	34.74
Share South (%)	30.55	32.62
Share West Coast (%)	28.29	23.34
Share directly experiencing emergency (%)	21.21	-
Day-of-Emergency Spending	\$2807.00	-
Number of Individuals	3,362	85,877
<b><i>Only health plan</i></b>	Treatment	Control
Average age	25.58	25.30
Average family size	4.80	4.73
Pre-emergency tenure	1253.60	1180.92
Share Male (%)	53.31	50.67
Share w/Charlson Comorbidity Score = 0	91.63	91.23
Share HMO (%)	9.95	12.19
Share PPO (%)	6.10	5.67
Share POS (%)	68.68	67.18
Share EPO (%)	15.24	14.93
Share East Coast (%)	13.06	12.15
Share Midwest (%)	20.09	20.89
Share South (%)	43.39	42.45
Share West Coast (%)	23.19	23.81
Share directly experiencing emergency (%)	21.89	-
Day-of-Emergency Spending	\$1266.57	-
Number of Individuals	15,598	450,426

Figure 1: Pre-emergency Tenure Distributions of Treatment and Control Groups



This figure presents the pre-emergency tenure distributions for the treatment and control groups. The distribution reflects the tenure for all family members belonging to the treatment and control group. Note, observations falling above the 99<sup>th</sup> percentile of tenure are dropped in order to preserve anonymity.

Figure 2: Kaplan-Meier Survival Curves



(b) Kaplan-Meier Survival Curve - by Start-Cohort

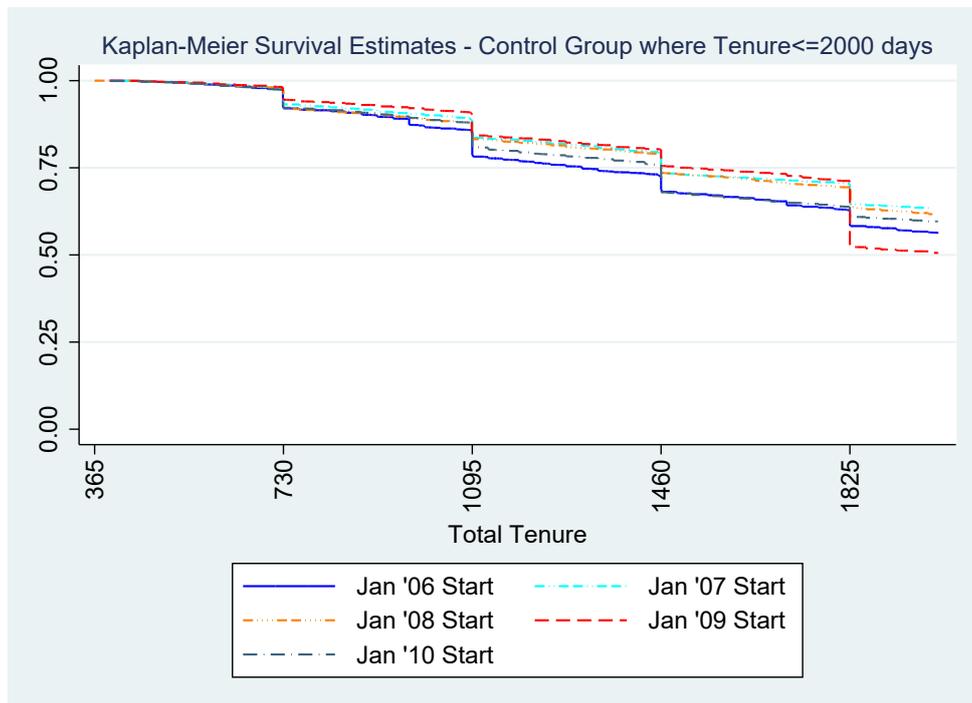
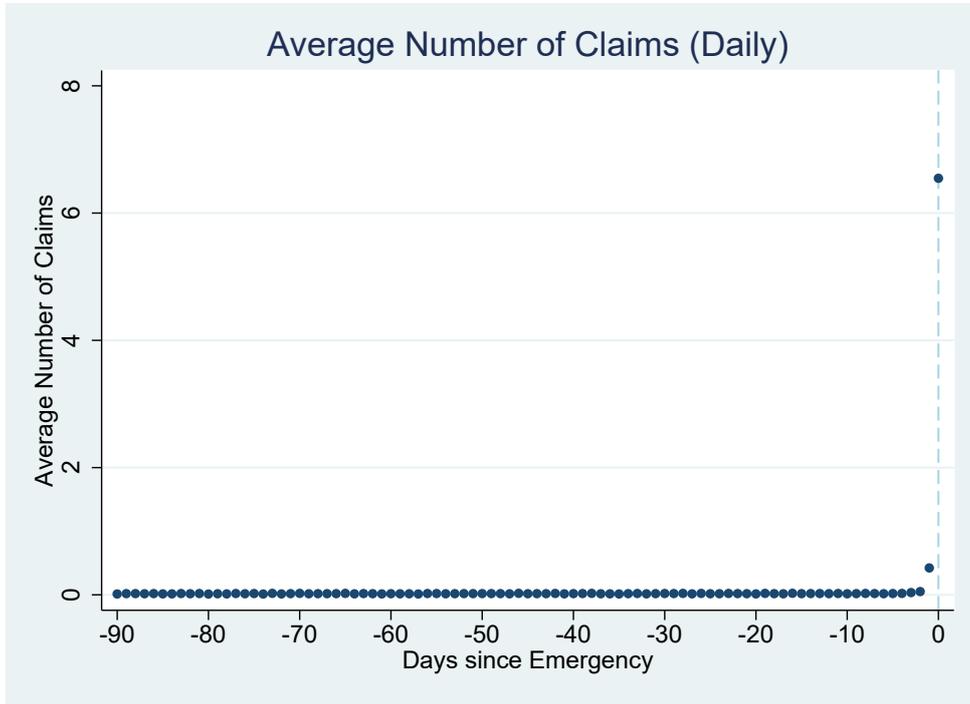


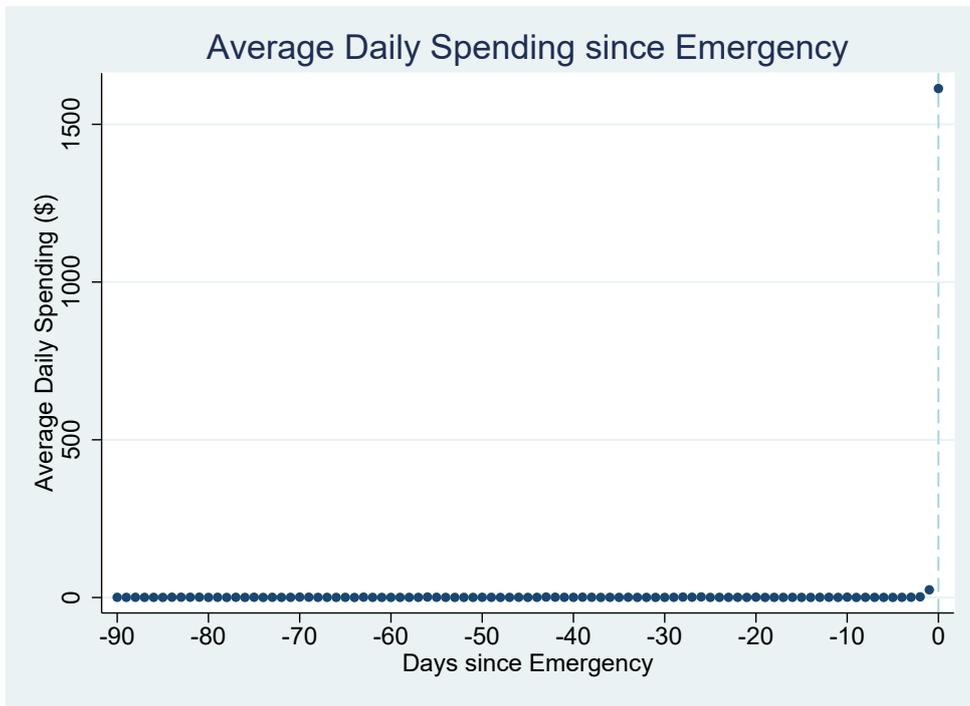
Figure 2a presents the Kaplan-Meier survival curves for all eligible controls. Figure 2b presents this curve for five distinct cohorts where the total tenure is less than or equal to 2000 days. Note, observations falling above the 99<sup>th</sup> percentile of tenure are dropped in order to preserve anonymity.

Figure 3: 90 Day Medical Outcomes for Directly Affected Individuals

(a) Number Claims: -90 to 0 Day Range



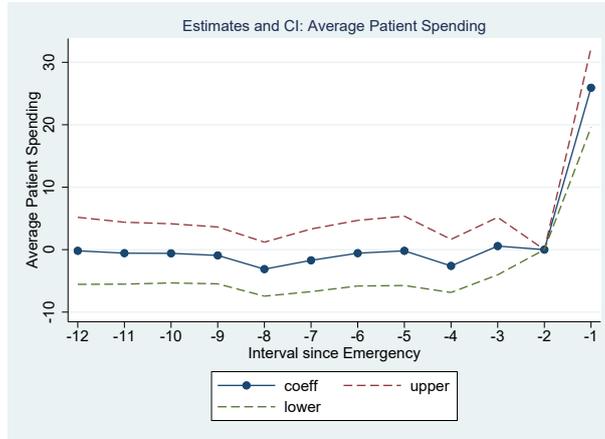
(b) Spending: -90 to 0 Day Range



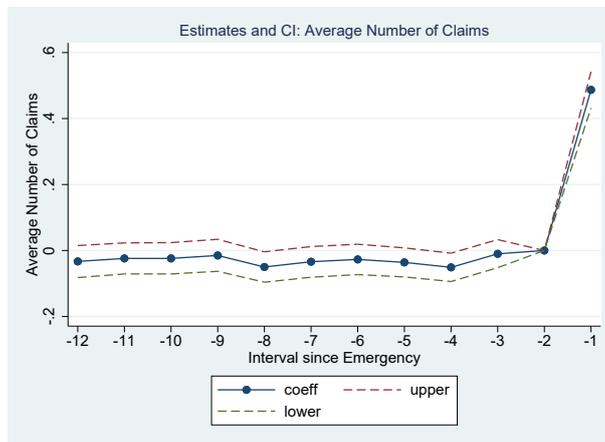
This figure presents the average daily medical spending and average number of medical claims made on a daily basis 90 days to 0 days before the appendicitis emergency. The sample is limited to individuals who directly experience the appendicitis emergency and who have insurance coverage for at least one year prior to the emergency.

Figure 4: Pre-trends in Medical Outcomes - Directly Affected Individuals

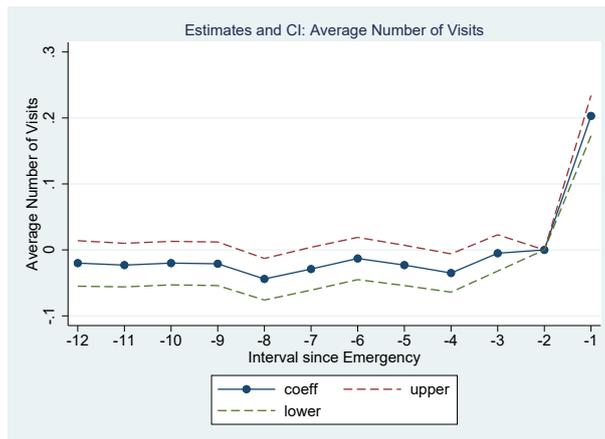
(a) Spending



(b) Number of Claims



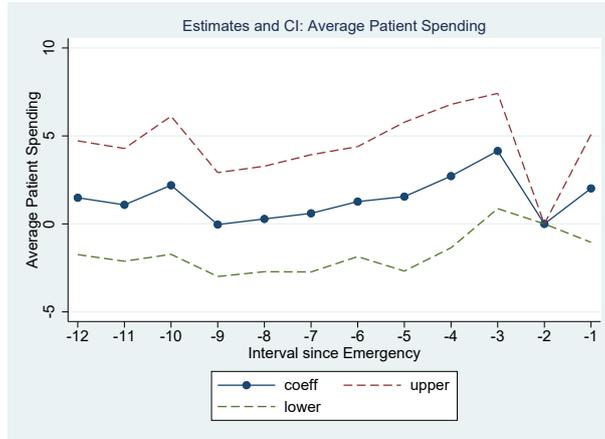
(c) Number of Visits



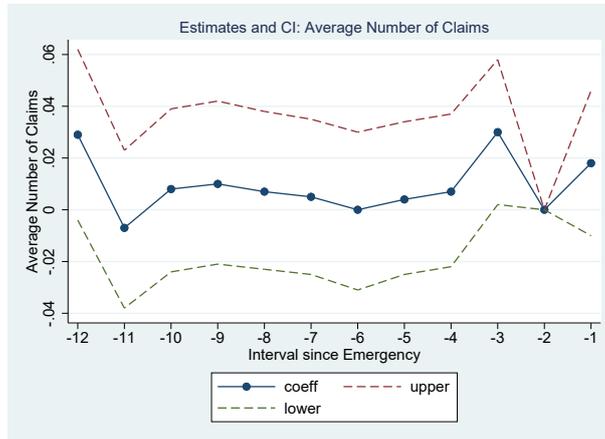
This figure presents estimates of  $\beta$  from the estimation of Equation 1 where the reference time period is  $t = -2$ , in the one year prior to the emergency. The outcomes are medical utilization outcomes for directly affected treatment individuals and all control group individuals who have at least one year of insurance coverage prior to an emergency. Each interval represents a roughly one-month period.

Figure 5: Pre-trends in Medical Outcomes - Indirectly Affected Individuals

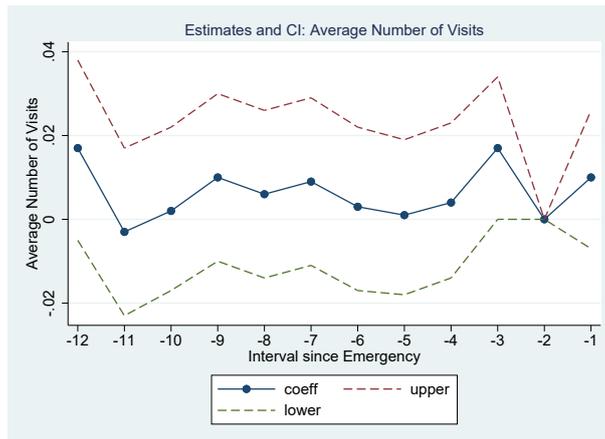
(a) Spending



(b) Number of Claims



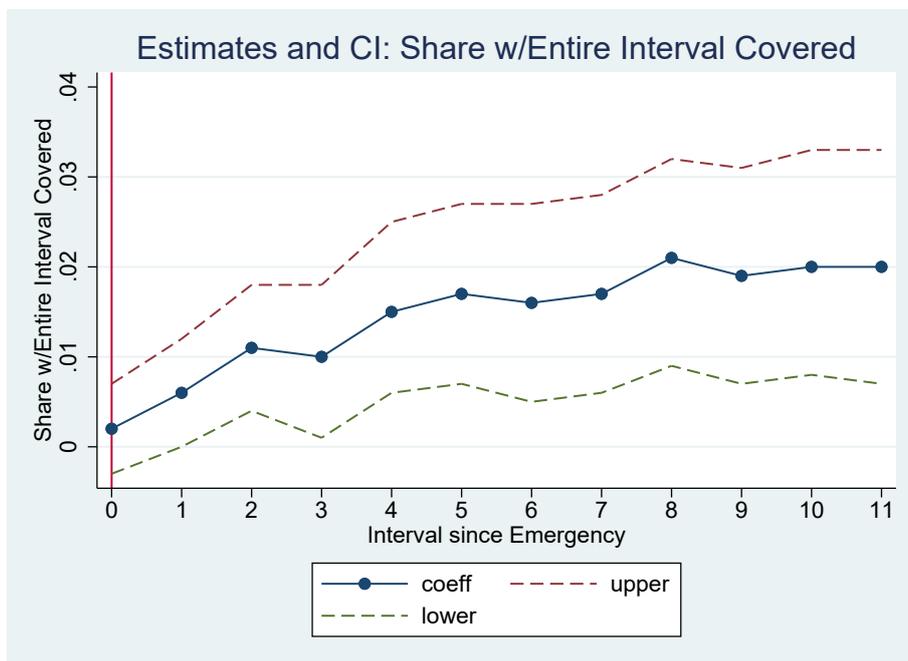
(c) Number of Visits



This figure presents estimates of  $\beta$  from the estimation of Equation 1 where the reference time period is  $t = -2$ , in the one year prior to the emergency. The outcomes are medical utilization outcomes for indirectly affected treatment individuals and all control group individuals who have at least one year of insurance coverage prior to an emergency. Each interval represents a roughly one-month period.

Figure 6: Estimates of  $\beta$

(a) Main Results: Estimates of  $\beta$



(b) Directly Affected vs. Indirectly Affected

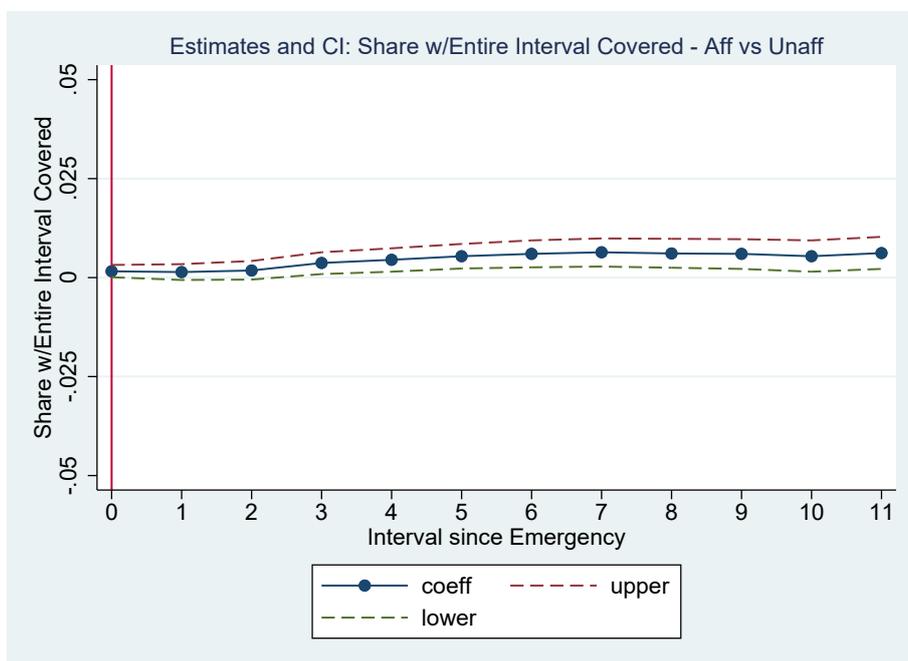
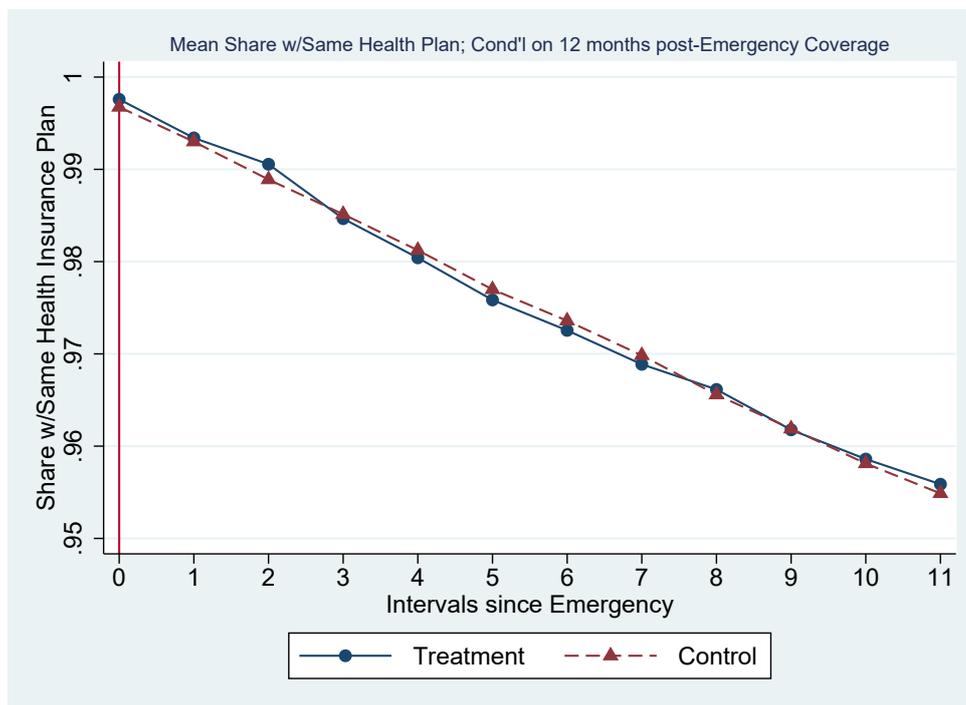


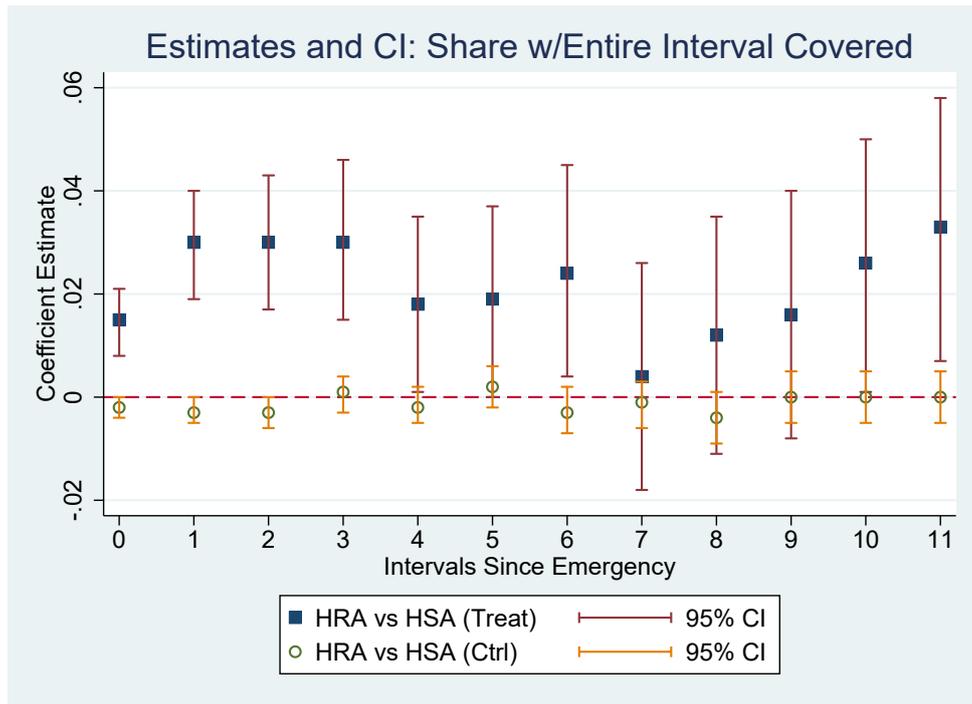
Figure 6b presents estimates of  $\beta$  from Equation (1) where treatment is defined as belonging to a family experiencing an appendicitis emergency. The examined outcome is a binary variable taking the value one if an individual has insurance coverage (through the insurance network) over the time interval considered and is zero, otherwise. The sample is limited to those with insurance coverage for at least one year prior to the emergency. Figure 6b presents estimates of  $\beta$  from Equation (1) where treatment takes the value one if an individual directly experiences the appendicitis emergency and zero if they are indirectly exposed through family affiliation. The sample is limited to treatment group individuals. Each interval represents a roughly one-month period.

Figure 7: Within-Insurer Network Health Plan Switching



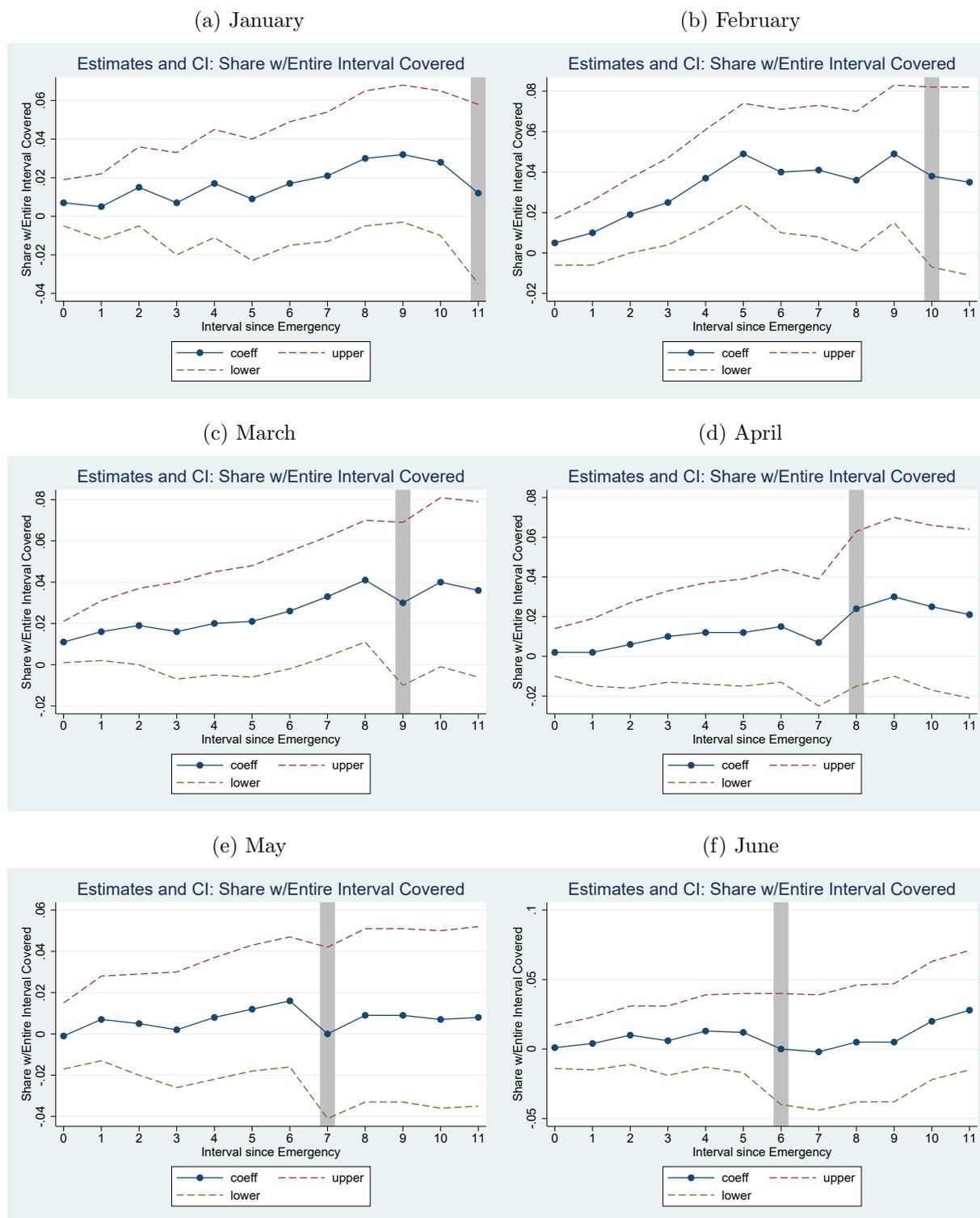
This figure presents the share of individuals who switch health insurance plans, conditional on remaining insured through the insurance network, at least 12 months after the emergency. Each interval represents a roughly one-month period.

Figure 8: Event Study Estimates of  $\beta$ : HRA vs. HSA



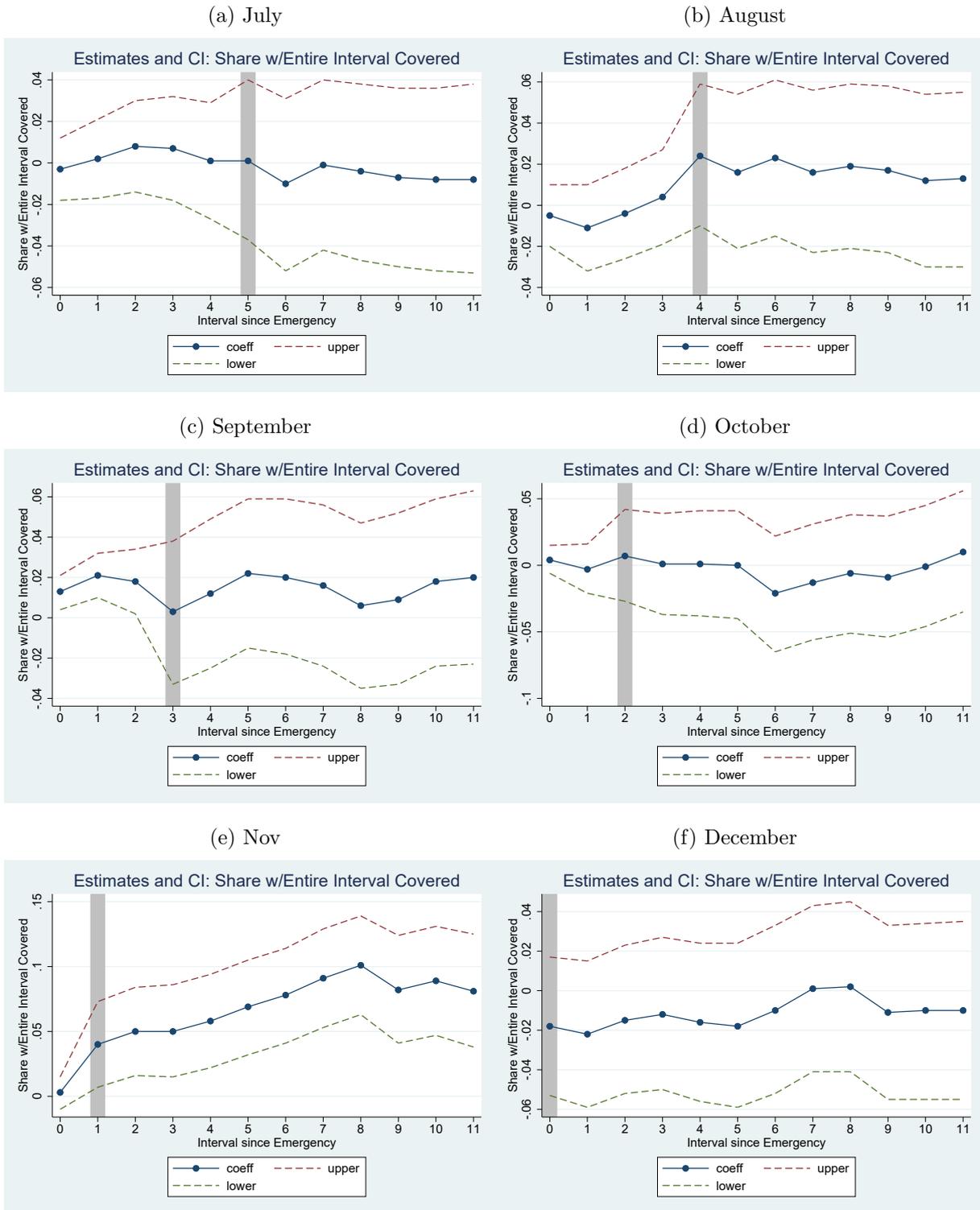
This figure presents estimates of  $\beta$  in each time period since an emergency, along with estimates of the 95 percent confidence interval, from Equation (1). Estimations are performed separately and compare families belonging to health plans with a Health Reimbursement Arrangement (HRA) vs. health plans with a Health Savings Account (HSA) at the time of the emergency. Each interval represents a roughly one-month period.

Figure 9: Estimates of  $\beta$  by Month of Emergency: January - June



This figure presents estimates of  $\beta$  in each interval since the emergency using Equation (1). Estimation is performed separately by the month of the emergency. Each interval is equivalent to roughly one month. The shaded grey bar denotes the January calendar month when open enrollment choices are typically realized. Treatment is defined as belonging to a family experiencing an appendicitis emergency. The examined outcome is a binary variable taking the value one if an individual has insurance coverage (through the insurance network) over the interval time period considered and is zero, otherwise. The sample is limited to individuals who have health insurance through the network for at least one year before an emergency.

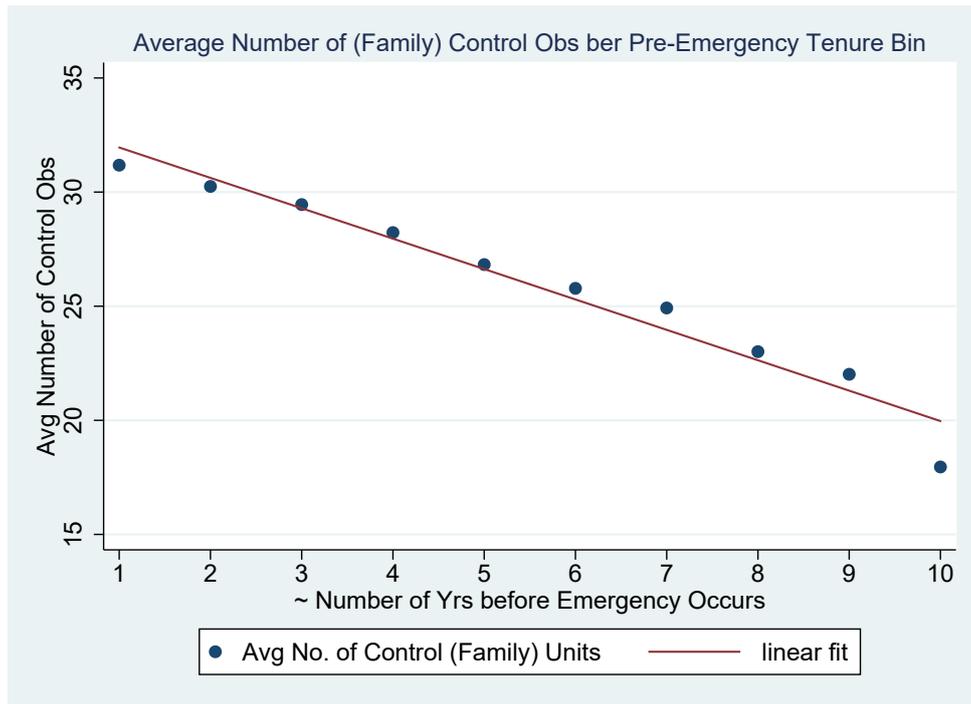
Figure 10: Estimates of  $\beta$  by Month in which Emergency Occurs: July - December



This figure presents estimates of  $\beta$  in each interval since the emergency using Equation (1). Estimation is performed separately by the month of the emergency. Each interval is equivalent to roughly one month. The shaded grey bar denotes the January calendar month when open enrollment choices are typically realized. Treatment is defined as belonging to a family experiencing an appendicitis emergency. The examined outcome is a binary variable taking the value one if an individual has insurance coverage (through the insurance network) over the interval time period considered and is zero, otherwise. The sample is limited to individuals who have health insurance through the network for at least one year before an emergency.

## 7 Appendix

Figure 11: Average Number of Available Controls by Treatment Family Tenure



This figure presents the average number of control families available to a treatment family, based on the number of days of pre-emergency tenure held by the treated families.