The Intracorrelation of Family Health Insurance and Job Lock

Marion Aouad*

March 2023

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.

JEL Classification: I10 I12 J10 J20 J22

*University of California Irvine, Department of Economics, 3151 Social Sciences Plaza Irvine, CA 92697. marion.aouad@uci.edu. Thank you to Casper Nordal Jørgensen, Todd Wagner, and Liam Rose for invaluable discussion, comments and advice. Thank you as well to members of Stanford University at S-SPIRE, PHS, SIEPR, Hannes Schwandt, Damon Clark, and seminar participants at Stanford, UC Irvine, Cornell PAM, the FTC, NYU Wagner, GWU Health Economics, University Duisburg-Essen, All California Labor Economics Conference and RAND. Data access for this project was provided by the Stanford Center for Population Health Sciences Data Core. The PHS Data Core is supported by a National Institutes of Health National Center for Advancing Translational Science Clinical and Translational Science Award (UL1 TR001085) and internal Stanford funding. The content is solely the responsibility of the authors and does not necessarily represent the official views of the NIH. Note: This paper was previously circulated under the titles, "The Intracorrelation of Family Health Insurance and Consumption" and "An Examination of the Intracorrelation of Family Health Insurance."

1 Introduction

Unexpected, individual-level health shocks can have far-reaching household implications (e.g. Dobkin et al. (2018) and 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.[1] As such, a health shock could result in the job lock of another family member, where that family member is non-optimally locked to a job in order to ensure continuous health insurance coverage, and continued access to a specific health insurance plan, for themselves and for other family members (Madrian (1994)).[2]

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 (unexpected, transitory 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, as of 2018. In particular, job changes are proxied for using health insurance network changes in response to an as-good-as-random appendicitis health shock (to be further discussed).

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 by children (Narsule et al. (2011)). Lastly, by examining the health shock of a child, who is neither a policy holder nor a financial contributor to the family, this study captures job lock tied to the need for health insurance/continued access to the same set of health care providers. This allows for a more precise capture of job lock that is tied

[1] 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.

[2] For example, a family health plan could increase the cost of switching jobs for a health plan's primary beneficiary beyond what it would be if every family member had their own health plan.

to the need for continuous access to health care rather than the need to smooth income (e.g. due to disruptions in labor supply) around the time of a health shock (Fadlon and Nielsen (2021)).[3]

To measure the rate of insurance network switching after the health shock, I exploit unique features of the data; namely that, dropping out of the data is due either to: i) switching to another insurance network (i.e. due to a job change or a change in the selected insurance network conditional on remaining at the same job) or ii) due to the complete loss of health insurance.[4] 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).

Furthermore, I exploit the fact that insurance network switching and the changing of health plans, outside of a Qualifying Life Event, is only traditionally allowed in certain months (i.e. during Open Enrollment periods).[5] Under certain assumptions, this allows for the construction of a measure of within-job network and health plan switching after the shock. 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 "placebo" emergency date and in doing so, creates a point-in-time benchmark to examine

[3]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 on 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, employment-sponsored health insurance must come from a family member/affiliate.

[4]Individuals who switch plans within the network will still be observed in the data.

[5]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 (2020). Open enrollment periods refer to periods where individuals can freely select a new health insurance network and/or new health plan (healthcare.gov (2020)).

the natural rate of "churn" out of the insurance network in the absence of a health shock.[6] 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 exit and (within-network) health plan changes for all other family members.[7] 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 job change rate of the primary insurance policy holder of approximately 13 percent, as compared to policy holders in families not exposed to the health shock. Such a finding demonstrates that job lock may occur because of a family's need to maintain continued access to an *employer-specific* health insurance plan.

Investigating possible mechanisms, I find evidence that health plan 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 arrangements (HRAs) 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 forefeit the money held in an 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 14 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. This finding does not preclude other pathways that may increase families' attachment to an employer-specific health insurance plan after a health shock, such as the desire to maintain access to the same set of health care providers, or the salience and recency of a health shock, which may cause families to over rely on the present health care expenses/frequency of medical interactions when determining future expenses/medical interactions (e.g. due to "availability bias" as discussed by Tversky and Kahneman (1974)); yet, it provides a testable pathway that helps explain the observed health plan and job

[6] This matching procedure is related to the matching techniques used by Miller (2017) and Fadlon and Nielsen (2019).

[7] The current health plan is defined as the health plan held at the time of the emergency.

lock.

Furthermore, the effect of the emergency on dropout rates is nearly identical across all family members one year after the emergency. This suggests that if families' health plan selection and employment decisions are influenced by the health status of the sickest family member, the health shock may not only affect the 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 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), Shi (2020), Bae and Meckel (2022)), 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)). However, as shown, transitory shocks can affect attachment to the current employer-specific health plan, which subsequently affects job lock. 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.[8] 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

[8]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)).

claims data. To the author's knowledge, this is the first study to use medical claims data to test for and estimate the magnitude of job lock occurring after a health shock. 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 (Madrian (1994)), and even after the expansion of public health insurance options, such as the Affordable Care Act (Bailey and Dave (2019)).[9]

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 insurance 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 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.

[9] For a more detailed discussion on the provisions of HIPAA, see Lewin (1999).

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 et al. (2017) and the *timing* of its onset occurs with few discernible predictable risk factors.[10] This implies that the onset of this disease can be considered as essentially random in its occurrence and timing. As such, families exposed to appendicitis and those who are not, should not systematically differ.

Furthermore, acute appendicitis is a condition that requires immediate medical attention. Thus, 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). Lastly, this disease has a low mortality rate, ranging from 0.1 to 1 percent (Craig, Sandy (2018)) and a two percent post-surgical complication rate among children ages one to seventeen years as found in a study by Rolle et al. (2021). Thus, the results of this analysis are more likely to extrapolate to a broader group given appendicitis's non-negligible rate of occurrence in the general population, as well as its lower mortality and smaller post-surgical complication rate. Further, disease severity is unlikely to drive the observed results.

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 or 2) switching health insurance plans, specifically into the health plan of a different

[10] 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) find geo-spatial correlation in the incidence for certain kinds of appendicitis (perforated appendicitis) in Washington state.

insurance network.[11] The analysis will assume that insurance network exit and health plan changes can largely be attributed to job changes based on evidence that individuals tend to be inert in changing health plans (Handel (2013)). Further, 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.

To achieve, ex-ante, comparability in the job switching rates, this analysis constructs a control group from individuals who join a network health plan in the same year and month as the appendicitis group, and 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). 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) and 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 1a depicts the Kaplan-Meier survival curve for a sample of individuals who are not exposed to appendicitis. Similarly, Figure 1b 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.[12] Both figures depict the share of the people who still belong to a health plan within the insurance network (i.e. those who have "survived" in the data) at any given point in time, t. Of interest is the slope of the graphs. The slope,

[11] 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. They are also a possible reason for insurance network and plan switching. This will be further discussed in Section 5.

[12] The join dates and days of total tenure are chosen for illustrative purposes; the general conclusions still hold if these are varied.

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 for 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 were not exposed to an appendicitis 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.

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.[13]

A cross-pairing for every possible appendicitis group member and eligible control group member 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 emer-

[13] In practice, the latter condition requires that $floor(\frac{T^C}{365}) \geq ceiling(\frac{T^T}{365})$, 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). Also, gaps in network coverage are possible since individuals can exit and re-enter the data at a later date. Thus, the focus is on spans of continuous, uninterrupted network coverage when examining tenure.

gency date of the treated reference individual is assigned to the paired control individual.[14] 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. An example of this match process using a hypothetical appendicitis emergency date is shown in the Appendix (Figure 10).

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 means of these characteristics. This is evident in the histogram displayed in Figure 2, which shows the very similar distributions of tenure held before the emergency date for the appendicitis and control groups, respectively.

Of note, there is heterogeneity in the number of corresponding matches for each treatment family that is correlated with the pre-emergency tenure of the treatment family. This is demonstrated in Figure 3, which shows that treated families with the highest pre-emergency tenure tend to have the fewest controls. This is a feature of the data, where the majority of individuals tend to remain in the network for shorter tenures (e.g. two years or fewer). Consequently, control families are allowed to be used as a match more than once across different emergency dates for different treatment families. This helps to ensure that treatment families with longer network tenures have control matches. Additionally, the number of controls used will be constrained to be similar across the treatment families, and its implications will be discussed later in the Robustness Checks Section.

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. *Optum* refers to Optum©'s de-identified Clinformatics® Data Mart Database. Optum©'s Clinformatics® 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.

[14] The number of cross-pairings matches used is varied and tested in the Robustness Checks Section.

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.[15] 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.[16] Note, while the primary policy holder cannot be identified in the data, it must be one of the two adults in the family unit.

Common restrictions are made for both the treatment and control groups. These restrictions include that families must consist of two adult heads and at least one child, and families must not experience an emergency admission or hospitalization within one year prior to the emergency date.[17] The former is done in order to narrow in on the possible

[15] 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.

[16] 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).

[17] 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 their appendicitis diagnosis.

mechanisms influencing the insurance decisions observed in the data.[18] 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 each observation.

Table 1 shows that the treatment and control groups have very similar demographic characteristics. There are 21,246 individuals in the treatment group (i.e. 4,634 families) and 605,590 individuals in the control group (i.e. 134,712 families). The average family consists of slightly fewer than five individuals, where the average age 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. Lastly, as previously mentioned, a control family can be used as a match more than once across different emergency dates for different treatment families. In practice, 89.4 percent of control families are used as a match once, 9.8 percent are used twice and 0.8 percent are used three or four times.

Importantly, general health, as proxied by the one-year Charlson comorbidity score (Charlson et al. (1987)) is highly similar across both groups.[19] Specifically, the share of individuals in both groups having zero comorbid conditions prior to the emergency is

[18]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.

[19]The look-back period used to compute the Charlson comorbidity score is one year before the emergency.

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 in the control group where the POS plans are the most prevalent, followed by HMO, EPO, 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 expense 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:

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

y_{it} measures insurance network exit/dropout for individual i at event-time $t \in [-12, 11]$. It takes the value of one if an individual maintains continuous insurance coverage through the insurance network over the entire interval (i.e. there is no dropout), where each interval is approximately 30 days long; it is zero, otherwise.[20] Since the primary policy

[20]The focus is on rolling 30 day windows since an emergency, with the last interval being 35 days long. Rolling windows are sequential month-long periods of time where the first month is initiated at the time of an emergency. This is done because people are unlikely to make job/health plan decisions based on

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, the main model examines the responses at the individual-level, rather than at the family-level.[21]

To further study the family-level response to the health shock, I also estimate Equation (1) at the family-level and examine two binary outcomes. The first outcome takes the value of one if all family members remain in the network over a given interval, and is zero otherwise. The second outcome takes the value of one if at least one family member remains in network over a given interval, and is zero otherwise.

D_{it}^{k} takes the value of one when an individual is observed k intervals since the emergency (placebo emergency); it is zero otherwise. 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 Abraham and Sun (2018)]."

The model focuses on a two year event window - one year before and one year after the emergency. 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 aggregated over roughly 30 day intervals and may be interpreted as *monthly* responses.

The parameters of interest in this model are the β_k. For each k, β_k gives the effect of exposure to an emergency on the probability of remaining in the network (as compared to the control group), in the k^{th} interval since the emergency, relative to the interval right before the emergency. The ρ_k parameter estimates provide a benchmark for the natural rate of continued coverage in the absence of an actual emergency. This benchmark is used to interpret the economic importance of the β estimates.

In order to interpret the β 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 is valid. In other words, in the absence of an emergency, the treatment group would have trended similarly in the outcome to the control group.

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.

[21] To account for the intracorrelation of family-level decisions, I cluster standard errors at the family level.

Given that the control group is also limited to having insurance for at least one 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 4 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 and on the day immediately preceding the emergency.

X represents a vector of covariates. This vector includes gender; 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 Arrangement 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, Z_{it} includes month dummies and year dummies. Note, given the match strategy, covariates are included for power, robustness and helping to ensure comparability across treatment and control groups.

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 across several medical utilization outcomes: medical spending/costs, number of claims made, and number of medical visits made over roughly 30 day periods.[22] Note, it is common to test the parallel trends assumption by examining value of pre-treatment

[22]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.

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

Given the increase in utilization in the day leading up to the emergency, as shown in Figure 4, medical claims/utilization from the two days prior to the emergency date are omitted from this analysis. Also, 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 5 and 6 presents these results. They show that across all outcomes, medical utilization trends quite similarly before the onset of an emergency across all groups, as expected.

5.2 Main Results - Insurance Network Changes

The main results are presented in Table 2, which reports the coefficient estimates of ρ and β from Equation (1). Estimates of ρ capture general trends in continued insurance coverage (i.e. staying in the health insurance network) while β captures the added effect of an appendicitis emergency on the likelihood of remaining in the insurance network. These results are also graphically presented in the Appendix (Figure 11a). By construction, in the intervals prior to the emergency, there is no difference in the exit rates across the treatment and control groups.

The coefficient estimates in Table 2 indicate that an appendicitis emergency results in an overall *increased* likelihood of remaining in 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.[23] 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. The effect also persists beyond one

[23]The rate of continued coverage for the control group in period k is ρ_k and the rate of continued coverage 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 percent change in the rate of continued coverage due to the appendicitis emergency. Since $\rho_k < 0$, this magnitude can be interpreted as the percent change in network exit due to the emergency.

year. Two years after the health shock, families exposed to the appendicitis emergency have an approximately 6 percent lower likelihood of exiting the insurance network.

Turning to the family-level estimation, the results are presented in Table 3. I find that families who are exposed to appendicitis are less likely to have the entire family-unit (i.e. all family members) exit the network, and are also less likely to have at least one family member exit the network. For example, one year after the health shock, appendicitis families are 8 percent less likely to have the entire family-unit exit the network and are 6.5 percent less likely to have at least one family member exit the network. These results support the main finding that the health shock reduces network switching of families exposed to appendicitis.

5.3 Health Plan Switching

I also examine the rate of within-insurance network health plan switching after the health shock. This is informative is because each health plan is employer-specific; thus, if there are reduced rates of employer-specific health plan switching after the shock, this suggests that there are reduced rates of job switching by the primary policy-holder. This will be further analyzed in Section 5.5.

Figure 7 shows the rate of health plan switching after the emergency across the treatment and control groups, conditional on remaining insured through the network for at least one year after the emergency. 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 one year after an emergency, 95.6 and 95.5 percent of the treatment and control groups maintain the same health insurance plan, respectively.[24] Taken together with the results presented in Section 5.2, these results show that treatment families are more likely to remain in the same plan (and network) one year after the health shock.[25]

[24]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 the treatment and control groups.

[25]Let A be the event that a family stays in the network for at least one year and let B be the event that a family maintains the same health plan within the network for at least one year. Given that $Pr(A \cap B) = Pr(A) \times Pr(B|A)$, then $Pr(A \cap B)^T > Pr(A \cap B)^C$ since $Pr(B|A)^T \approx Pr(B|A)^C$ and $Pr(A)^T > Pr(A)^C$, where T represents the treatment group and C represents the control group.

5.4 Correlation in Family Insurance Coverage

To better understand how individual health shocks may lock other family members on to 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 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 4 presents the coefficient estimates of β, which are also graphically displayed in Appendix Figure 11b. The results indicate that there is some initial within-family variability in dropout among family members exposed to appendicitis. However, this effect is not long lasting and diminishes within one year. Specifically, differential family retention is largest around the time of the shock – within one month of the shock, there is an approximately 6.5 percent lower rate of dropout for those directly experiencing the shock compared to those who do not, but within one year, this effect is much smaller at 2.5 percent. These estimates suggest that if there is within-family heterogeneity in network dropout, it occurs soon after the health shock and is not long lasting.

This finding illustrates that there is generally a high degree of correlation and persistence in health insurance network choice within a family unit over time. The results are meaningful because alternative familial responses are possible. For example, a subset of the family could decide to join the insurance plan of the other adult-head in the family unit. Thus, the results suggest that the health shock may also "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 or longer-run household welfare losses from reduced health plan switching if there are welfare-improving plans that are being passed over (Handel (2013)) as 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 healthier 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 provided by employers.

5.5 Job Lock Evidence

In this section, I discuss how the observed results can be explained by job lock. In particular, I discuss how the timing of network exit, where the month of exit is likely correlated with the reason for network exit, can be used to construct an estimate 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 the calender 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 determined by the Internal Revenue Service code (e.g. job changes, marriage, divorce, birth of a child, move to a different county).[26] 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.

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 childbirth 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 likely 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 relatively stable at 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 by 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 8 and 9 and trace out the effects of the health shock for emergencies occurring in calendar months January through November.[27] 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 zero to four percentage points lower among the families exposed to the appendicitis health shock, depending on the month of the

[26] More examples of Qualifying life events can be found in the Appendix (Section 7.1).

[27] 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.

emergency. For example, among families experiencing an emergency in February, within three to twelve months of the emergency, families exposed to the appendicitis health shock are two to four percent more likely to stay in the network. Of note, the standard errors for the β 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 demonstrate the relationship between the observed network/plan selections and job decisions, Appendix Figure 12 provides a schema of the network/plan $\times$ job outcomes that occur after the health shock, as well as the data identification strategy used to estimate the relevant shares (to be discussed below).

To construct the job lock estimate, I estimate the following equation for each calendar month in which an emergency occurs[28]:

$$\textit{Share with same employer 1-year after the emergency} = A \times (B + C) + (1 - A) \times D \tag{2}$$

Here, A represents the share of families who stay in the network for at least one year after the emergency; B represents the share of families who remain at the same job and with the same employer-specific health plan, conditional on remaining in the network, within one year of the emergency; C represents the share of families where the primary policy holder maintains the same job but selects a new health plan within the same network, within one year of the emergency; and D represents the share of families where the primary policy holder exits the network for a new health plan, conditional on remaining at the same job, within one year of the emergency.[29]

Both A and B can be directly calculated in the data. To calculate D, I examine the 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

[28]To get an aggregate, one-year estimate of job lock, I calculate the weighted sum of job lock across the emergency months.

[29]I focus on emergencies occurring in January through November here since emergencies that occur in these months allow for the post-health shock measurement of C and D.

of the calendar year, December 31, these individuals exit the network and join a new health insurance network and new 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).

Relatedly, C is estimated by calculating the share of families who remain in the network for one year after their emergency, but who have elected a new health plan as of January in the calendar year following the emergency. Specifically, to estimate C, I assume that individuals who change health plans during the Open Enrollment period, do so conditional on remaining at the same job. This implies that within-network plan switching can be identified by changes in the employer-specific health plan group number that occur in the January after the health shock. Similar to the calculation of D, this approach assumes that typical job separation dates are continuous around December 31.

Under these assumptions, 83.5 percent of the treatment group remains with their employer for at least one additional year. In contrast, 81.1 percent of the control group remains with their employer for at least one additional year after the emergency. This implies an estimated reduction in job mobility of approximately 13 percent.[30] 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. due to a 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 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)).

[30]The calculation for this is estimate is: $\frac{(100-83.5)-(100-81.1)}{(100-81.1)}$.

Lastly, while the job lock estimate is constructed under reasonable assumptions, the limitations of this estimate must be acknowledged. In particular, it is possible that the appendicitis emergency affects other - unobserved - family decisions, which may affect adult labor supply and job attachment. For example, there may be time costs associated with post-emergency care and follow-up. However, given that the post-surgical recovery time for an appendix removal is typically one to four weeks (Kaiser Permanente (2020)), this is likely a smaller concern.

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 versus a non-portable "savings" account.

Health insurance plans can have 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)).[31]

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.[32]

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,

[31]The characteristics of enrollees across the three health plans are shown in Table 5. 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.

[32]An Employee Benefits Research Institute (EBRI) study found that the average balance held in an HRA or HSA was approximately $2,100 in 2014 (Fronstin and Elmlinger (2015)).

the money held in the add-on accounts may be costly to forfeit).

The results are presented in Table 6 and in Appendix Figure 13. Among families exposed to the appendicitis health shock, those who belong to the HRA plan at the time of the 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 13. 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 \$2,500 - \$2,800). 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. Of note, this finding does not necessarily exclude other possible mechanisms. For example, the *salience* and recency of the health shock may "update" families' beliefs regarding future health care expenditures (e.g. see Gallagher (2014)); moreover, this process may lead families to over rely on the present health care expenses/frequency of medical interactions when determining future expenses/frequency of medical interactions (e.g. due to "availability bias" as discussed by Tversky and Kahneman (1974)). This may exacerbate existing switching costs associated with changing physicians, such as the search costs associated with finding a new physician or the time costs associated with establishing trust with a new physician, leading to lowered rates of network exit.

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 7. 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 network 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 experienced by 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 one year after the emergency. Overall, this result translates to a reduction in the one-year job change rate of approximately 13 percent for the health plan's primary policy holder. These findings suggest that characteristics of the current health insurance market contribute to both "health plan lock" and job lock.

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 on 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 health reimbursement arrangements (HRAs) may make health plan and job switching more costly at the time of an expensive health shock. 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 after an emergency. This examination is beyond the scope of this work but is a fruitful area for future research.

References

Akerlof, G. A. (1978). The market for "lemons": Quality uncertainty and the market mechanism. In *Uncertainty in economics*, pp. 235–251. Elsevier.

American-Fact-Finder (2019). US Census Bureau. `https://factfinder.census.gov/faces/tableservices/jsf/pages/productview.xhtml?pid=ACS_13_3YR_B27004&prodType=table.com` [Accessed: July 2019].

Bae, H. and K. Meckel (2022). Dependent coverage and parental" job lock": Evidence from the affordable care act. Technical report, National Bureau of Economic Research.

Bailey, J. and A. Chorniy (2016). Employer-provided health insurance and job mobility: did the affordable care act reduce job lock? *Contemporary Economic Policy 34*(1), 173–183.

Bailey, J. and D. Dave (2019). The effect of the affordable care act on entrepreneurship among older adults. *Eastern Economic Journal 45*(1), 141–159.

Baird, D. L., C. Simillis, C. Kontovounisios, S. Rasheed, and P. P. Tekkis (2017). Acute appendicitis. *Bmj 357*, j1703.

Bansak, C. and S. Raphael (2008). The state children's health insurance program and job mobility: identifying job lock among working parents in near-poor households. *ILR review 61*(4), 564–579.

Berger, M. C., D. A. Black, and F. A. Scott (2004). Is there job lock? evidence from the pre-hipaa era. *Southern Economic Journal*, 953–976.

Bhangu, A., K. Søreide, S. Di Saverio, J. H. Assarsson, and F. T. Drake (2015). Acute appendicitis: modern understanding of pathogenesis, diagnosis, and management. *The Lancet 386*(10000), 1278–1287.

Bradley, C. J., D. Neumark, and S. Barkowski (2013). Does employer-provided health insurance constrain labor supply adjustments to health shocks? new evidence on women diagnosed with breast cancer. *Journal of health economics 32*(5), 833–849.

Bradley, C. J., D. Neumark, Z. Luo, and H. L. Bednarek (2007). Employment-contingent health insurance, illness, and labor supply of women: evidence from married women with breast cancer. *Health economics 16*(7), 719–737.

Bundorf, M. K., J. Levin, and N. Mahoney (2012). Pricing and welfare in health plan choice. *American Economic Review 102*(7), 3214–48.

Cengiz, D., A. Dube, A. Lindner, and B. Zipperer (2019). The effect of minimum wages on low-wage jobs. *The Quarterly Journal of Economics 134*(3), 1405–1454.

Census Bureau (cited August 2020). Census regions and divisions of the united states. `https://www2.census.gov/geo/pdfs/maps-data/maps/reference/us_regdiv.pdf`.

Centers for Disease Control and Prevention (2021). National Center for Health Statistics -

Emergency Department Visits. www.cdc.gov/nchs/fastats/emergency-department [Accessed: June 2021].

Charlson, M. E., P. Pompei, K. L. Ales, and C. R. MacKenzie (1987). A New Method of Classifying Prognostic Comorbidity in Longitudinal Studies: Development and Validation. *Journal of Chronic Diseases 40*(5), 373–383.

Chatterji, P., P. Brandon, and S. Markowitz (2016). Job mobility among parents of children with chronic health conditions: Early effects of the 2010 affordable care act. *Journal of Health Economics 48*, 26–43.

Copeland, C. (2019). Trends in employee tenure, 1983–2018. *EBRI Issue Brief 474*, 4–16.

Craig, S. and B. E. Brenner (cited August 2020). Appendicitis. https://emedicine.medscape.com/article/773895-overview.pdf.

Craig, Sandy (2018). Medscape Drugs Diseases: What is the Mortality Rate associated with Appendicitis? . https://emedicine.medscape.com/article/773895-overview [Accessed: May 2022].

Cunningham, P. J. and L. Kohn (2000). Health plan switching: Choice or circumstance? *Health Affairs 19*(3), 158–164.

Currie, J. and B. C. Madrian (1999). Health, health insurance and the labor market. *Handbook of labor economics 3*, 3309–3416.

Cutler, D. M. and R. J. Zeckhauser (1998). Adverse selection in health insurance. In *Forum for Health Economics & Policy*, Volume 1. De Gruyter.

Dobkin, C., A. Finkelstein, R. Kluender, and M. J. Notowidigdo (2018). The Economic Consequences of Hospital Admissions. *American Economic Review 108*(2), 308–52.

Fadlon, I. and T. H. Nielsen (2019). Family health behaviors. *American Economic Review 109*(9), 3162–91.

Fadlon, I. and T. H. Nielsen (2021). Family labor supply responses to severe health shocks: Evidence from danish administrative records. *American Economic Journal: Applied Economics*.

Farrell, J. and P. Klemperer (2007). Coordination and lock-in: Competition with switching costs and network effects. *Handbook of industrial organization 3*, 1967–2072.

Fronstin, P. and A. Elmlinger (2015). Health savings accounts and health reimbursement arrangements: Assets, account balances, and rollovers, 2006-2014. *EBRI Issue Brief* (409).

Gallagher, J. (2014). Learning about an infrequent event: evidence from flood insurance take-up in the united states. *American Economic Journal: Applied Economics*, 206–233.

Garthwaite, C., T. Gross, and M. J. Notowidigdo (2014). Public health insurance, labor supply, and employment lock. *The Quarterly Journal of Economics 129*(2), 653–696.

Golz, R. A., D. R. Flum, S. E. Sanchez, X. Liu, C. Donovan, and F. T. Drake (2020). Geographic association between incidence of acute appendicitis and socioeconomic status. *JAMA surgery 155*(4), 330–338.

Gruber, J. and B. C. Madrian (2002). Health insurance, labor supply, and job mobility: A critical review of the literature.

Handel, B. R. (2013). Adverse selection and inertia in health insurance markets: When nudging hurts. *American Economic Review 103*(7), 2643–82.

Handel, B. R. and J. T. Kolstad (2015). Health insurance for" humans": Information frictions, plan choice, and consumer welfare. *American Economic Review 105*(8), 2449–2500.

healthcare.gov (2020). Qualifying Life Event. `https://www.healthcare.gov/glossary/qualifying-life-event/` [Accessed: January 2021].

healthcare.gov (2022). Qualifying life event. `https://www.healthcare.gov/glossary/qualifying-life-event/` [Accessed: June 2022].

Jeon, S.-H. and R. V. Pohl (2017). Health and work in the family: Evidence from spouses' cancer diagnoses. *Journal of health economics 52*, 1–18.

Kaiser Permanente (2020). Appendectomy: What to Expect at Home. `https://healthy.kaiserpermanente.org/health-wellness/health-encyclopedia/he.appendectomy-what-to-expect-at-home.ug3573` [Accessed: November 2020].

Kapur, K. (1998). The impact of health on job mobility: A measure of job lock. *ILR review 51*(2), 282–298.

Lamiraud, K. and P. Stadelmann (2020). Switching costs in competitive health insurance markets: The role of insurers' pricing strategies. *Health Economics 29*(9), 992–1012.

Lewin, R. (1999). Job lock: Will hipaa solve the job mobility problem. *U. Pa. J. Lab. & Emp. L. 2*, 507.

Madrian, B. C. (1994). Employment-based health insurance and job mobility: Is there evidence of job-lock? *The Quarterly Journal of Economics 109*(1), 27–54.

Miller, C. (2017). The persistent effect of temporary affirmative action. *American Economic Journal: Applied Economics 9*(3), 152–90.

Narsule, C. K., E. J. Kahle, D. S. Kim, A. C. Anderson, and F. I. Luks (2011). Effect of delay in presentation on rate of perforation in children with appendicitis. *The American journal of emergency medicine 29*(8), 890–893.

Oreopoulos, P., T. Von Wachter, and A. Heisz (2012). The short-and long-term career effects of graduating in a recession. *American Economic Journal: Applied Economics 4*(1), 1–29.

Polyakova, M. (2016). Regulation of insurance with adverse selection and switching costs: Evidence from medicare part d. *American Economic Journal: Applied Economics 8*(3), 165–95.

Rolle, U., C. Fahlenbach, C.-D. Heidecke, G. Heller, H.-J. Meyer, E. Schuler, B. Waibel, E. Jeschke, C. Günster, and M. Maneck (2021). Rates of complications after appendectomy in children and adolescents: pediatric surgical compared to general surgical hospitals. *Journal of Surgical Research 260*, 467–474.

Rothschild, M. and J. Stiglitz (1978). Equilibrium in competitive insurance markets: An essay on the economics of imperfect information. In *Uncertainty in economics*, pp. 257–280. Elsevier.

Royalty, A. B. and J. M. Abraham (2006). Health insurance and labor market outcomes: Joint decision-making within households. *Journal of Public Economics 90*(8-9), 1561–1577.

Shi, M. (2020). Job lock, retirement, and dependent health insurance: Evidence from the affordable care act.

Sinaiko, A. D., T. J. Layton, S. Rose, and T. G. McGuire (2017). Implications of family risk pooling for individual health insurance markets. *Health Services and Outcomes Research Methodology 17*(3-4), 219–236.

Stroupe, K. T., E. D. Kinney, and T. J. Kniesner (2001). Chronic illness and health insurance-related job lock. *Journal of Policy Analysis and Management: The Journal of the Association for Public Policy Analysis and Management 20*(3), 525–544.

Tax Policy Center (2020). Tax Policy Center Briefing Book: A Citizen's Guide to the Fascinating (though often Complex) Elements of the US Tax System. *Washington, DC: Urban Institute and The Brookings Institution.*

Tversky, A. and D. Kahneman (1974). Judgment under uncertainty: Heuristics and biases: Biases in judgments reveal some heuristics of thinking under uncertainty. *science 185*(4157), 1124–1131.

US Department of Labor, B. o. L. S. (2018). Employee tenure summary.

U.S. Government Accountability Office (2011). `https://www.gao.gov/products/gao-12-166r.pdf` [Accessed: October 2021].

Table 1: Summary Statistics

	Treatment (1)	*Control* (2)
Average age	25.60	25.31
Average family size	4.80	4.75
Pre-emergency tenure	1279.03	1197.87
Share Male (%)	53.13	50.63
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	605,590

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	No Cov	F.e.
	(1)	(2)	(3)
General effect (rel. to -1), ρ_k			
Intervals since emergency			
0	-0.027***	-0.027***	-0.027***
	(0.000)	(0.000)	(0.000)
1	-0.051***	-0.051***	-0.051***
	(0.001)	(0.001)	(0.001)
2	-0.075***	-0.075***	-0.075***
	(0.001)	(0.001)	(0.001)
3	-0.099***	-0.099***	-0.099***
	(0.001)	(0.001)	(0.001)
4	-0.123***	-0.123***	-0.123***
	(0.001)	(0.001)	(0.001)
5	-0.144***	-0.144***	-0.144***
	(0.001)	(0.001)	(0.001)
6	-0.168***	-0.168***	-0.168***
	(0.001)	(0.001)	(0.001)
7	-0.189***	-0.189***	-0.189***
	(0.001)	(0.001)	(0.001)
8	-0.210***	-0.210***	-0.210***
	(0.001)	(0.001)	(0.001)
9	-0.230***	-0.230***	-0.230***
	(0.001)	(0.001)	(0.001)
10	-0.249***	-0.249***	-0.249***
	(0.001)	(0.001)	(0.001)
11	-0.269***	-0.269***	-0.269***
	(0.001)	(0.001)	(0.001)
Added effect of treatment (rel. to -1), β_k			
Intervals since emergency			
0	0.002	0.002	0.002
	(0.002)	(0.002)	(0.002)
1	0.006*	0.006*	0.006*
	(0.003)	(0.003)	(0.003)
2	0.011***	0.011***	0.011***
	(0.004)	(0.004)	(0.004)
3	0.010**	0.010**	0.010**
	(0.004)	(0.004)	(0.004)
4	0.015***	0.015***	0.015***
	(0.005)	(0.005)	(0.005)
5	0.017***	0.017***	0.017***
	(0.005)	(0.005)	(0.005)
6	0.016***	0.016***	0.016***
	(0.005)	(0.005)	(0.005)
7	0.017***	0.017***	0.017***
	(0.006)	(0.006)	(0.006)
8	0.021***	0.021***	0.021***
	(0.006)	(0.006)	(0.006)
9	0.019***	0.019***	0.019***
	(0.006)	(0.006)	(0.006)
10	0.020***	0.020***	0.020***
	(0.006)	(0.006)	(0.006)
11	0.020***	0.020***	0.020***
	(0.007)	(0.007)	(0.007)
Number of individuals	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 ρ, while Added effect of treatment estimates represent coefficient estimates of β from Equation (1). By construction, the difference in coverage is constrained to be zero between comparison groups when $k \in [-12, -1]$. $Treatment$ takes the value of one if an individual belongs to families who are 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. Note: Estimates across models are identical up to the fourth decimal place.
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 - Family-level Effect of an Emergency on Insurance Coverage

	Coverage	
	All Family Members in network (1)	At least one Family Member in network (2)
General effect (rel. to -1), ρ_k		
Intervals since emergency		
0	-0.030***	-0.025***
	(0.000)	(0.000)
1	-0.058***	-0.047***
	(0.001)	(0.001)
2	-0.085***	-0.07***
	(0.001)	(0.001)
3	-0.112***	-0.092***
	(0.001)	(0.001)
4	-0.139***	-0.114***
	(0.001)	(0.001)
5	-0.163***	-0.133***
	(0.001)	(0.001)
6	-0.188***	-0.155***
	(0.001)	(0.001)
7	-0.212***	-0.175***
	(0.001)	(0.001)
8	-0.234***	-0.194***
	(0.001)	(0.001)
9	-0.256***	-0.212***
	(0.001)	(0.001)
10	-0.278***	-0.231***
	(0.001)	(0.001)
11	-0.301***	-0.250***
	(0.001)	(0.001)
Added effect of treatment (rel. to -1), β_k		
Intervals since emergency		
0	0.003	0.002
	(0.002)	(0.002)
1	0.008***	0.005*
	(0.003)	(0.003)
2	0.014***	0.009***
	(0.004)	(0.004)
3	0.013***	0.009**
	(0.004)	(0.004)
4	0.020***	0.015***
	(0.005)	(0.004)
5	0.022***	0.015***
	(0.005)	(0.005)
6	0.021***	0.014***
	(0.006)	(0.005)
7	0.022***	0.014***
	(0.006)	(0.005)
8	0.027***	0.017***
	(0.006)	(0.006)
9	0.025***	0.016***
	(0.006)	(0.006)
10	0.025***	0.017***
	(0.007)	(0.006)
11	0.025***	0.016**
	(0.007)	(0.006)
Number of families	139,029	139,029

1: Data consists of medical claims data pooled from 2003-2019.
2: General effect estimates represent coefficient estimates of ρ, while Added effect of treatment estimates represent coefficient estimates of β from Equation (1). By construction, the difference in coverage is constrained to be zero between comparison groups when $k \in [-12, -1]$. In column 1, the outcome variable takes the value of one if all family members remain in network in a given interval, and is zero otherwise. In column 2, the outcome variable takes the value of one if at least one family member remains in network in a given interval, and is zero otherwise. Each interval represents an approximately 30 day rolling window since the emergency.
3: Columns 1 and 2 include all demographic covariates. 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 4: 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), ρ_k	
Intervals since emergency	
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), β_k	
Intervals since emergency	
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 ρ, while Added effect of treatment estimates represent coefficient estimates of β from Equation (1). By construction, the difference in coverage is constrained to be zero 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 the individual belongs to a family exposed to an 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 5: Data Summary by Health Plan Type

HRA + health plan	Treatment (1)	Control (2)
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
HSA + health plan	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
Only health plan	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

Note: This table reports summary statistics for the individuals exposed to an appendicitis health shock (column 1) and the control group (column 2) by the type of health plan held 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 6: Insurance Coverage Estimates Comparing Plans with HRA vs. HSA

	Full Interval Coverage	
	HRA vs. HSA (Treatment Group)	HRA vs. HSA (Control Group)
	(1)	(2)
General effect (rel. to -1), ρ_k		
Intervals since emergency		
0	-0.022***	-0.025***
	(0.003)	(0.001)
1	-0.051***	-0.045***
	(0.004)	(0.001)
2	-0.065***	-0.066***
	(0.004)	(0.001)
3	-0.087***	-0.088***
	(0.005)	(0.001)
4	-0.097***	-0.110***
	(0.005)	(0.001)
5	-0.113***	-0.132***
	(0.005)	(0.001)
6	-0.142***	-0.152***
	(0.006)	(0.001)
7	-0.155***	-0.174***
	(0.006)	(0.001)
8	-0.171***	-0.192***
	(0.006)	(0.001)
9	-0.195***	-0.212***
	(0.007)	(0.001)
10	-0.212***	-0.231***
	(0.007)	(0.001)
11	-0.232***	-0.251***
	(0.007)	(0.002)
Added effect of treatment (rel. to -1), β_k		
Intervals since emergency		
0	0.015***	-0.002**
	(0.003)	(0.001)
1	0.030***	-0.003**
	(0.005)	(0.001)
2	0.030***	-0.003**
	(0.007)	(0.001)
3	0.030***	0.001
	(0.008)	(0.002)
4	0.018**	-0.002
	(0.009)	(0.002)
5	0.019**	0.002
	(0.010)	(0.002)
6	0.024**	-0.003
	(0.010)	(0.002)
7	0.004	-0.001
	(0.011)	(0.002)
8	0.012	-0.004*
	(0.012)	(0.002)
9	0.016	0.000
	(0.012)	(0.002)
10	0.026**	0.000
	(0.013)	(0.003)
11	0.033**	0.000
	(0.013)	(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 ρ, while Added effect of treatment estimates represent coefficient estimates of β from Equation (1). By construction, the difference in coverage is constrained to be zero 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 to a shock. 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%.

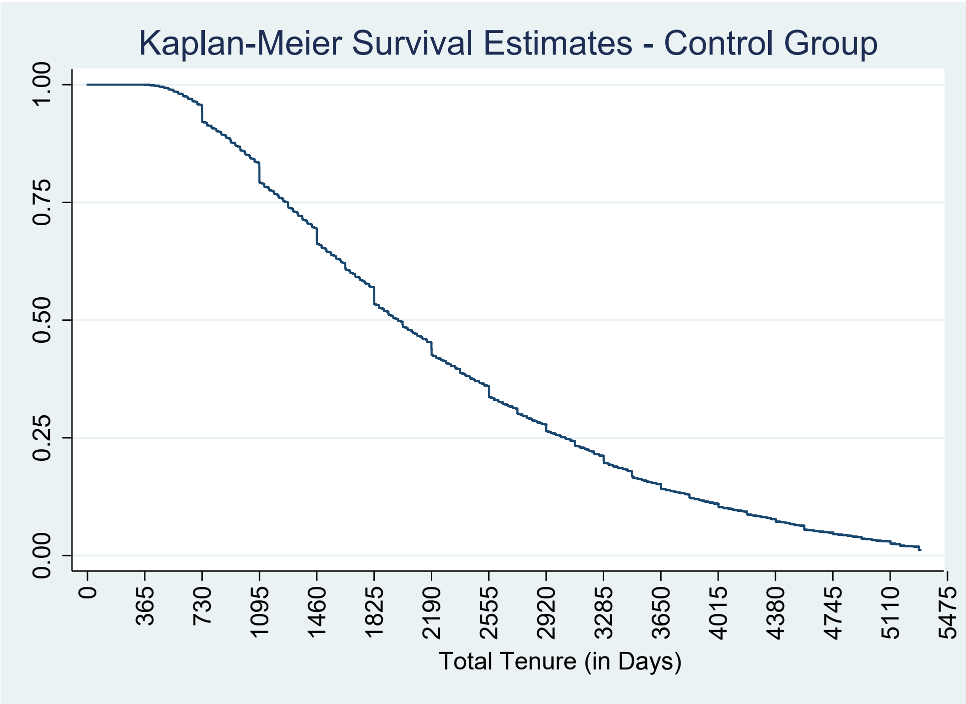
Table 7: Main Regression Estimates - p5 – p95 Subgroup Effect of an Emergency on Insurance Coverage

	Full Interval Coverage Main (1)
General effect (rel. to -1), ρ_k	
Intervals since emergency	
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), β_k	
Intervals since emergency	
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 their matched controls.
2: General effect estimates represent coefficient estimates of ρ, while Added effect of treatment estimates represent coefficient estimates of β from Equation (1). By construction, the difference in coverage is constrained to be zero 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%.

Figure 1: Kaplan-Meier Survival Curves

(a) Kaplan-Meier Survival Curve



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

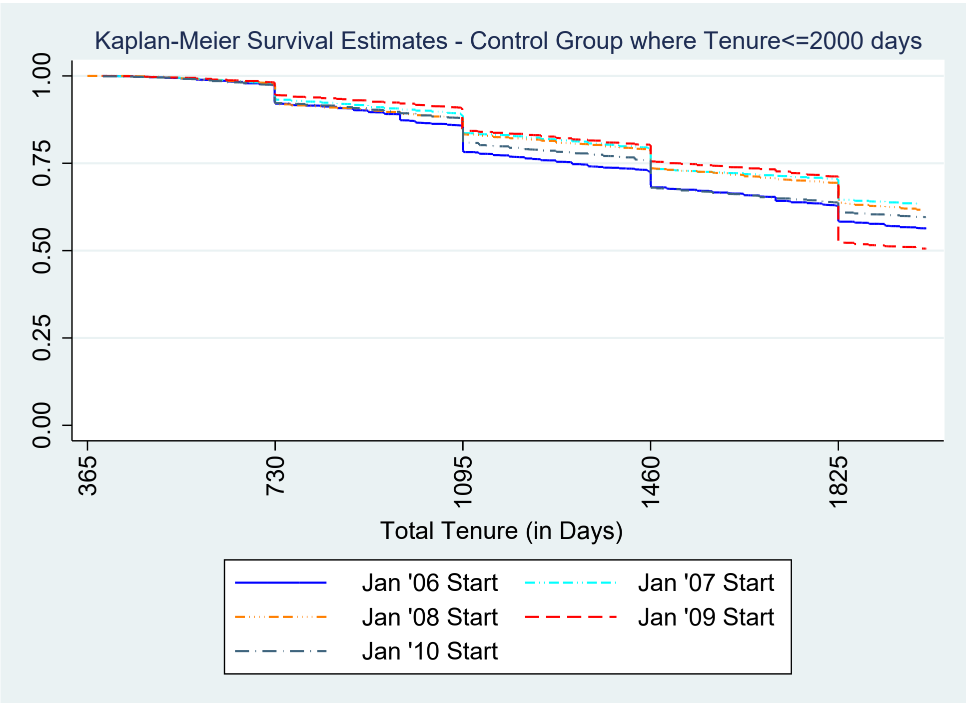
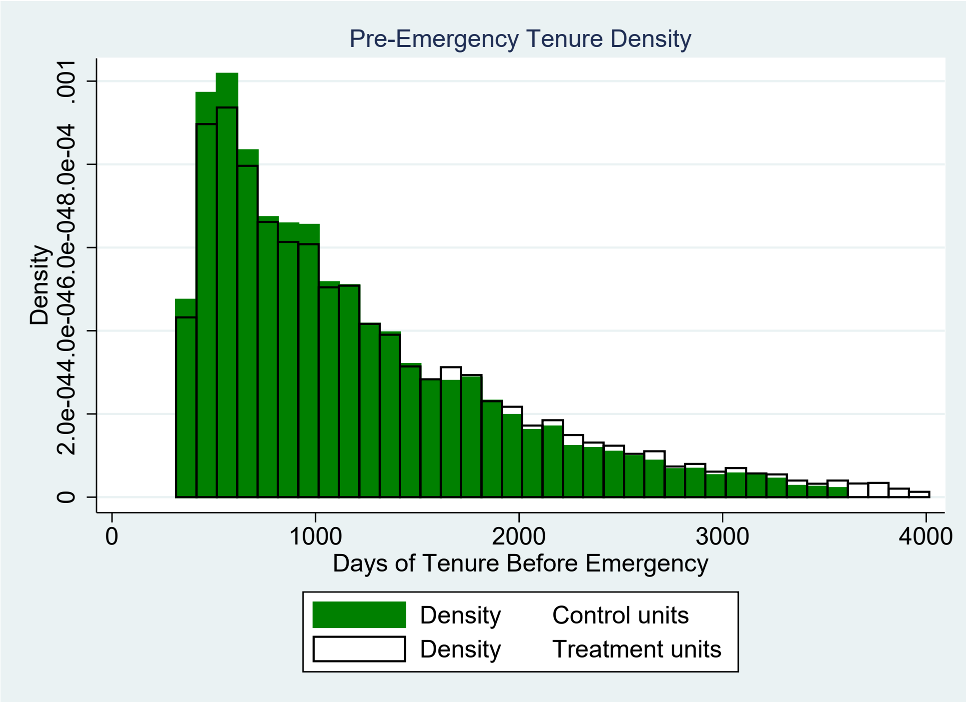


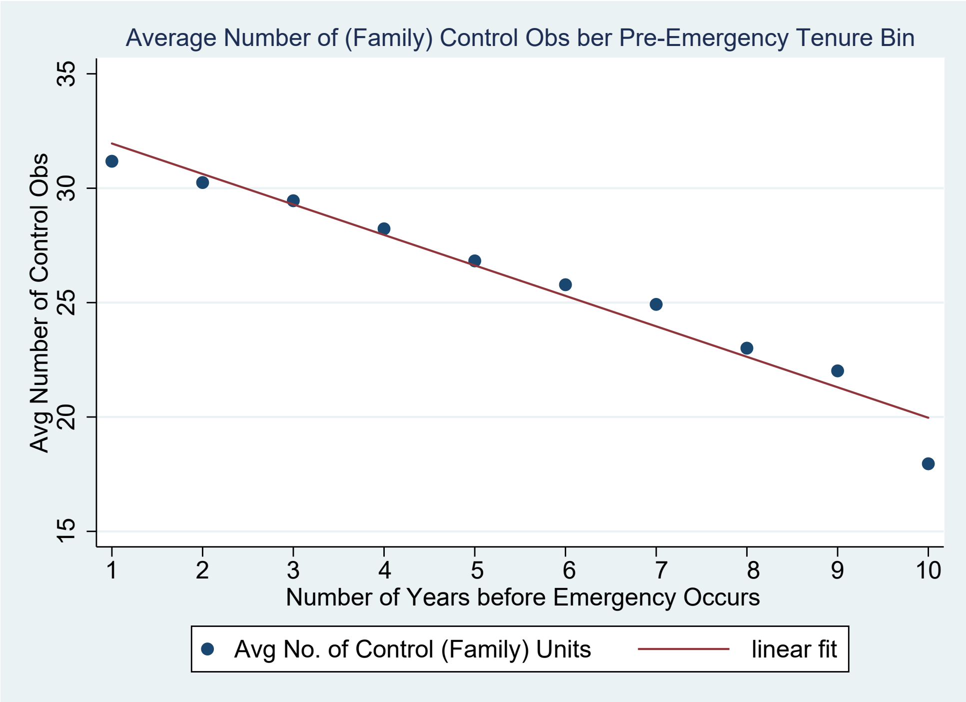
Figure 1a presents the Kaplan-Meier survival curves for all eligible controls. Figure 1b presents this curve for five distinct cohorts where the total tenure is less than or equal to 2000 days. Note, observations falling above the 99^{th} percentile of tenure are dropped in order to preserve anonymity.

Figure 2: Pre-emergency Tenure Distributions of Treatment and Control Groups



This figure presents the pre-emergency tenure distributions for all family members belonging to the treatment and control groups. Note, observations falling above the 99^{th} percentile of tenure are dropped in order to preserve anonymity.

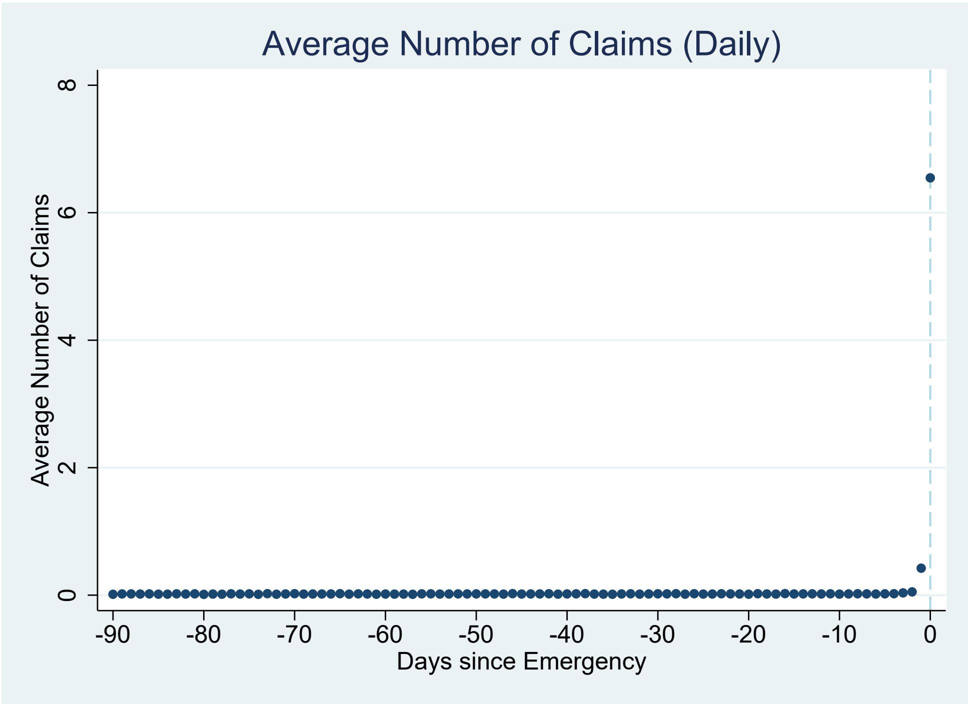
Figure 3: Average Number of Available Controls by Treatment Family Tenure



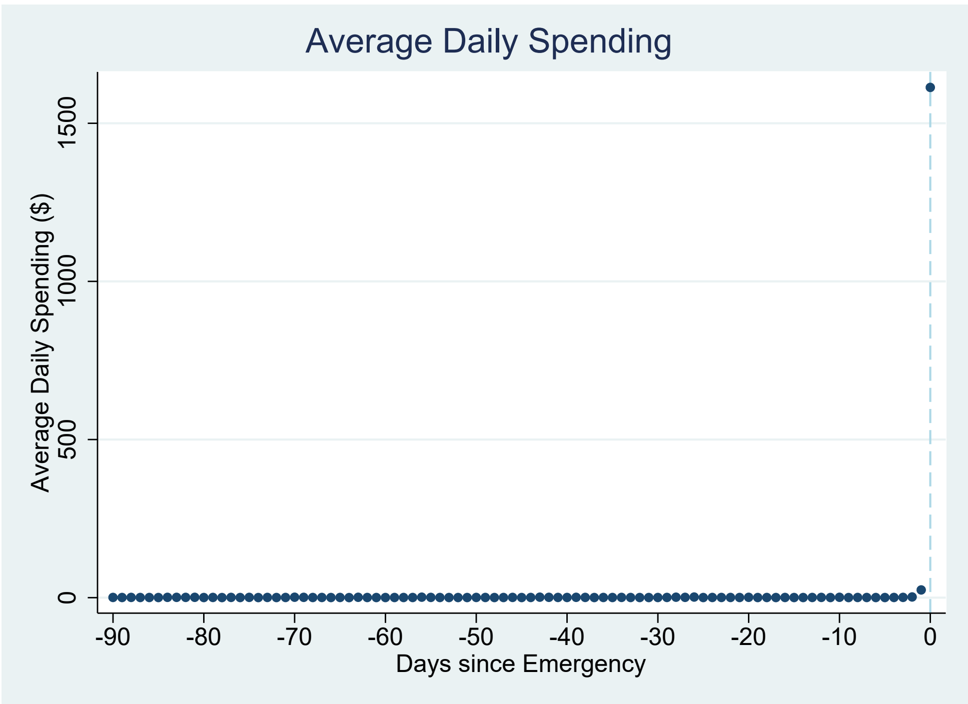
This figures presents the average number of control families available to treatment families based on the number of years of pre-emergency tenure held by treatment families.

Figure 4: 90 Day Medical Outcomes for Directly Affected Individuals

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



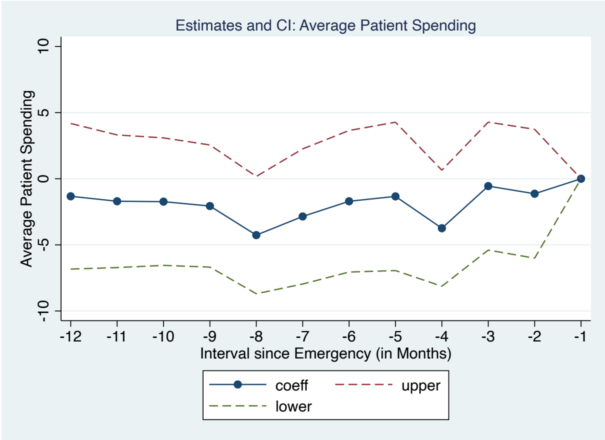
(b) Spending: -90 to 0 Day Range



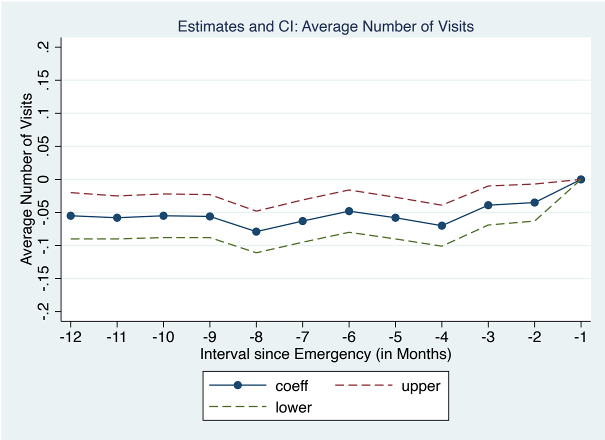
This figure presents the average daily number of medical claims made and the average daily medical spending ninety days to zero days before the appendicitis emergency. The sample is limited to individuals who directly experience the appendicitis emergency and who have insurance coverage for at least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

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

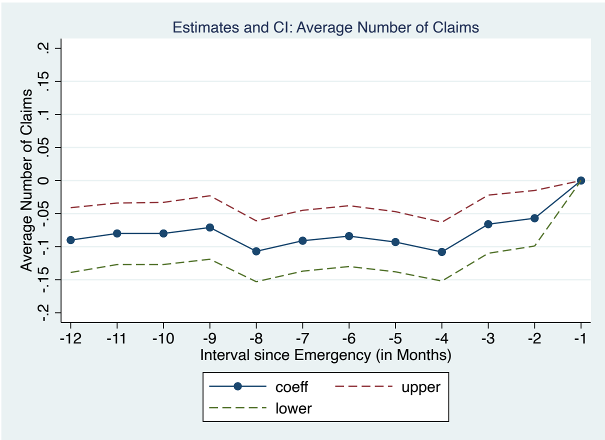
(a) Spending



(b) Number of Claims



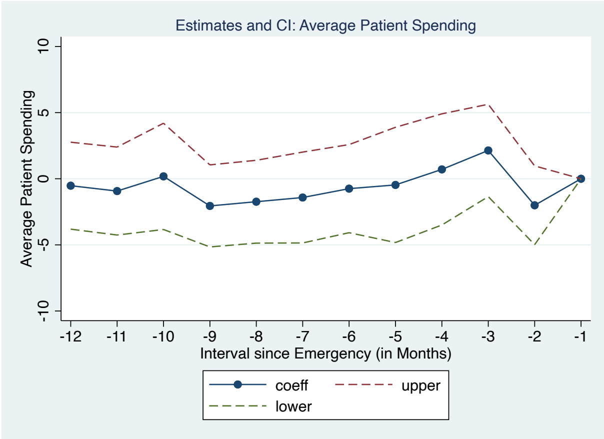
(c) Number of Visits



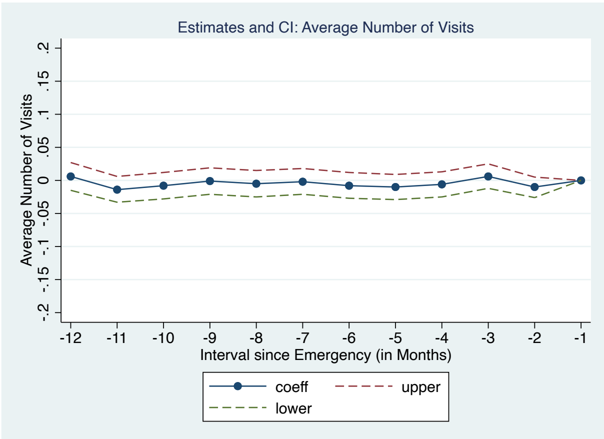
This figure presents estimates of β from the estimation of Equation (1). The outcomes are medical utilization outcomes for directly affected treatment individuals (i.e. the children) and all control group individuals who have at least one year of insurance coverage prior to an emergency. Each interval represents a roughly one-month period.

Figure 6: Pre-trends in Medical Outcomes - Indirectly Affected Individuals

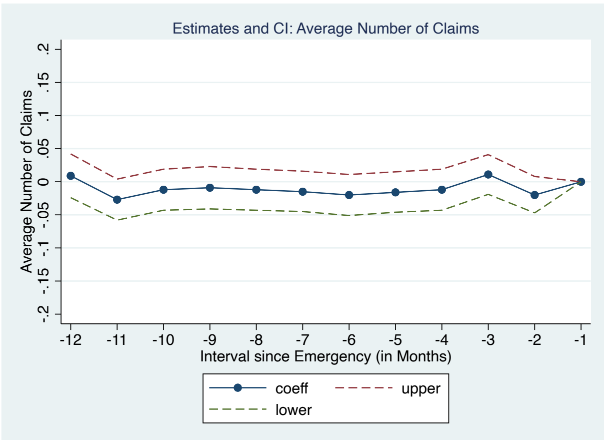
(a) Spending



(b) Number of Claims

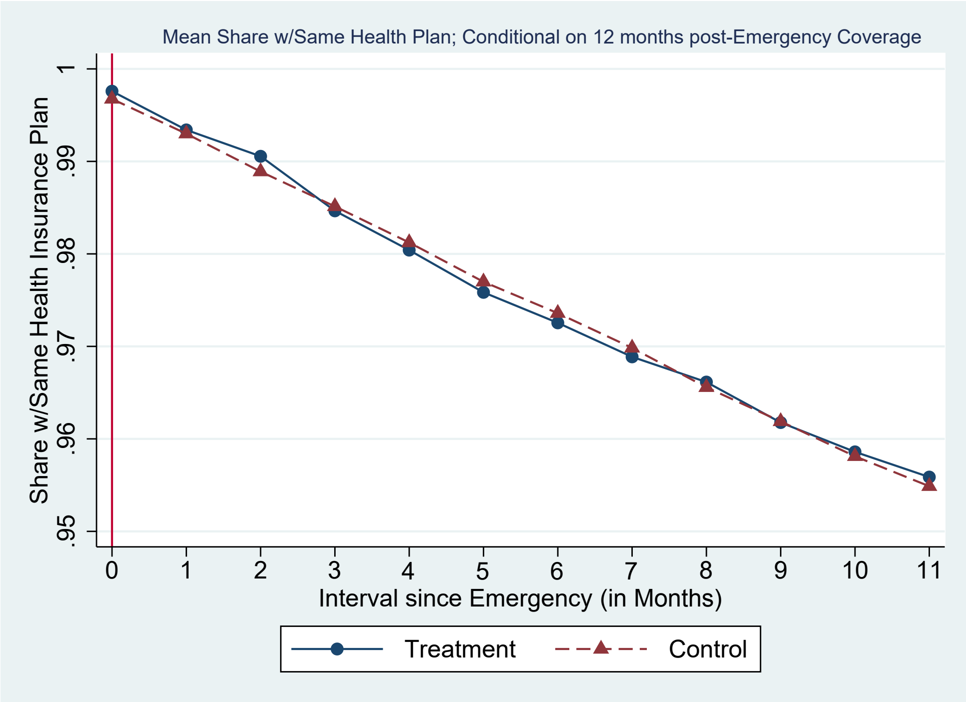


(c) Number of Visits



This figure presents estimates of β from the estimation of Equation (1). 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.

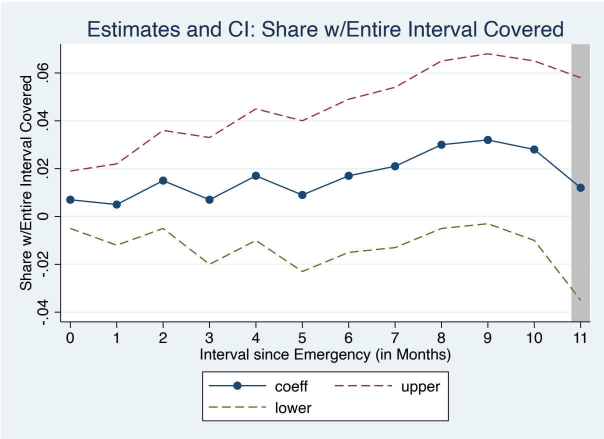
Figure 7: Within-Insurer Network Health Plan Switching



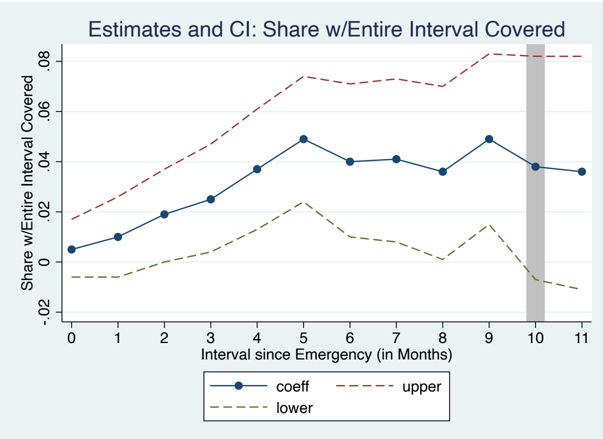
This figure presents the share of individuals who switch health insurance plans, conditional on remaining insured through the insurance network for at least 12 months after the emergency. Each interval represents a roughly one-month period.

Figure 8: Estimates of β by Month of Emergency: January - June

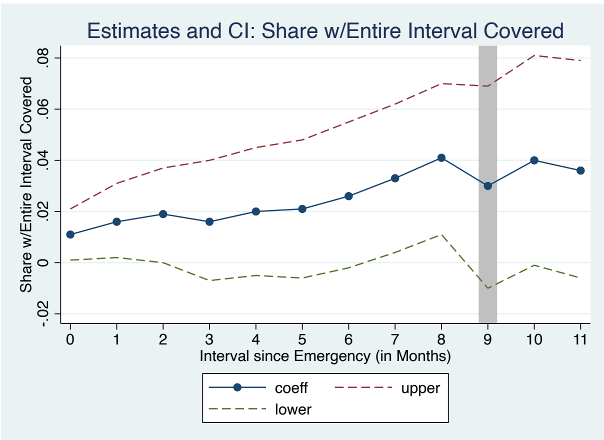
(a) January



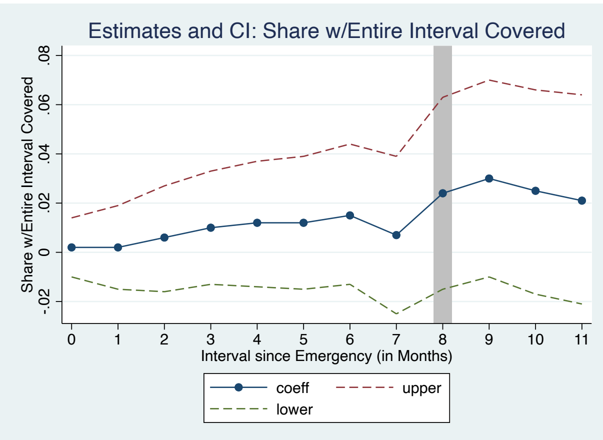
(b) February



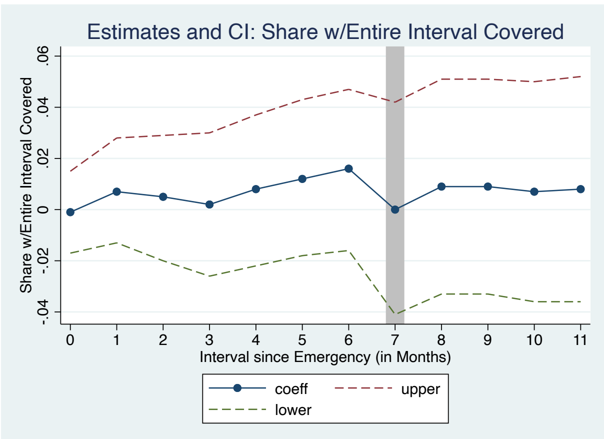
(c) March



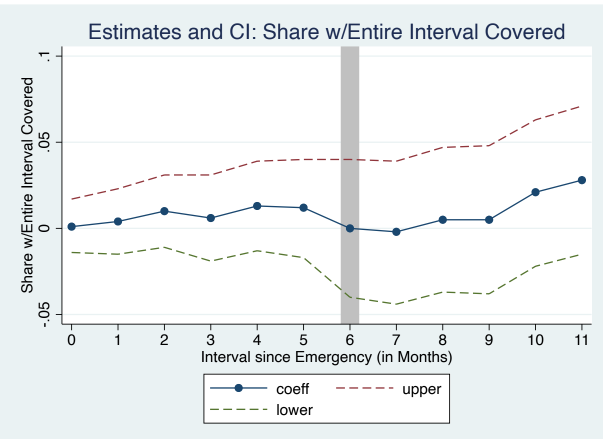
(d) April



(e) May



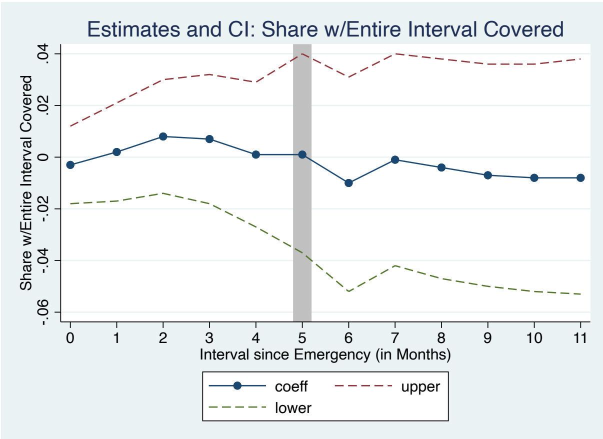
(f) June



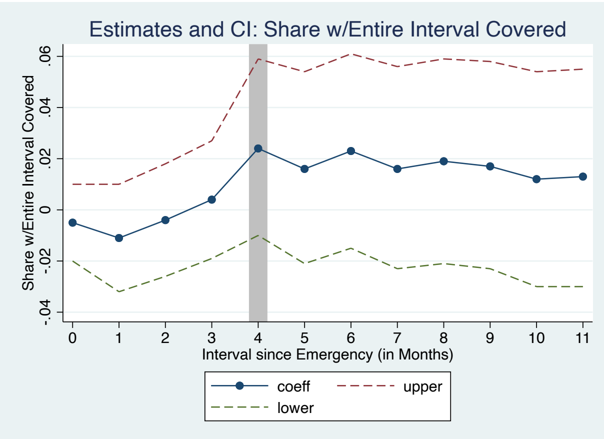
This figure presents estimates of β in each interval since the emergency using Equation (1). Estimation is performed separately by the month of the emergency. Each interval is equivalent to roughly one month. The shaded grey bar denotes the January calendar month when open enrollment choices are typically 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 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 9: Estimates of β by Month in which Emergency Occurs: July - November

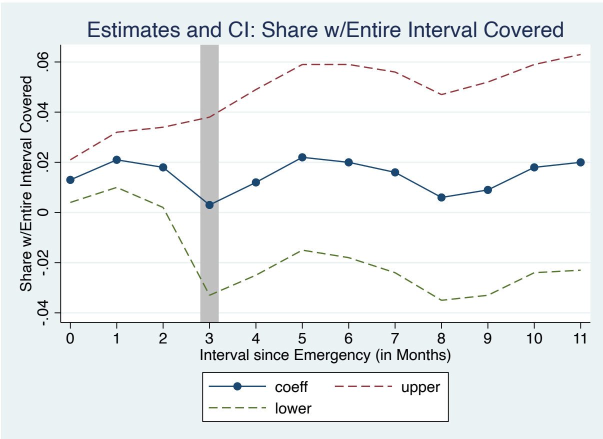
(a) July



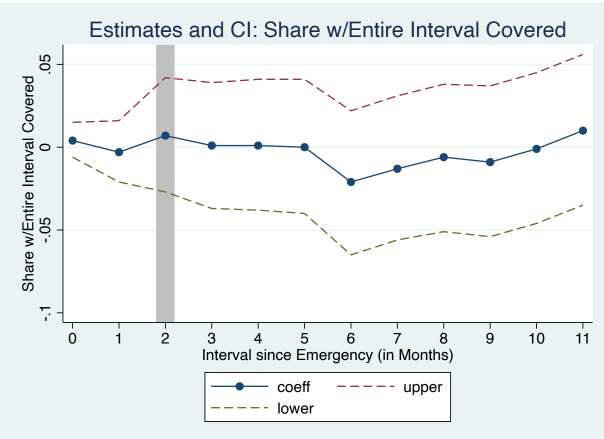
(b) August



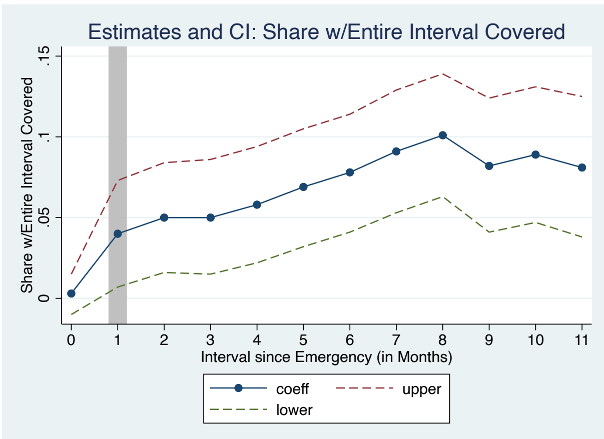
(c) September



(d) October



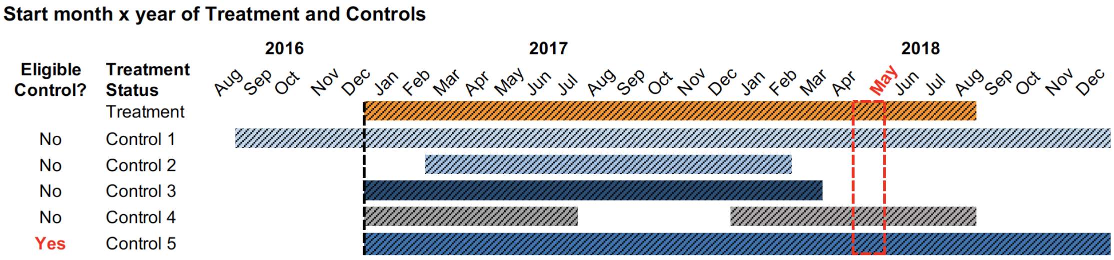
(e) November



This figure presents estimates of β 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 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

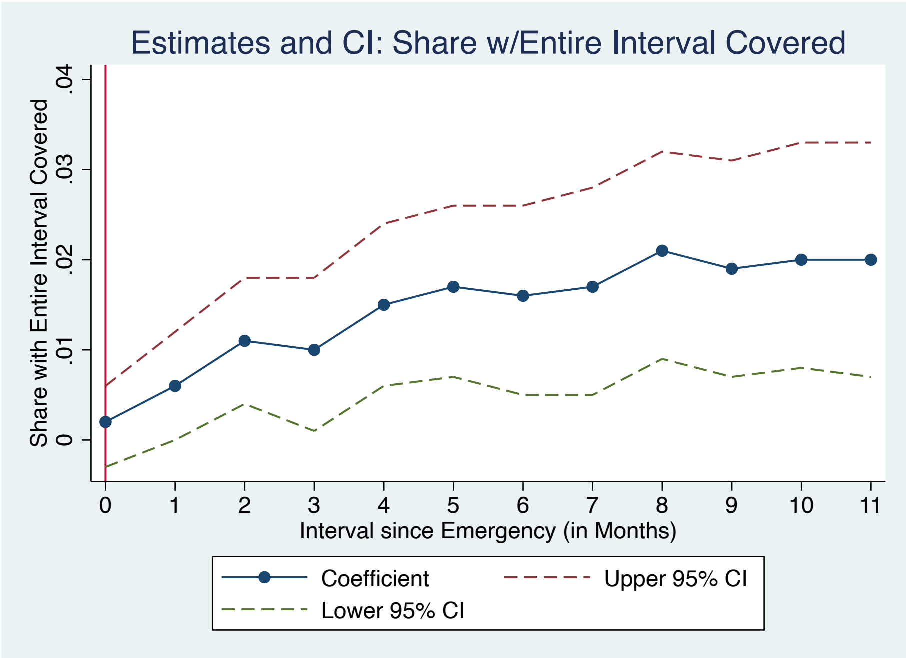
Figure 10: Match Process Example



This figure presents an example of the match process discussed in Section 2.3 using a hypothetical appendicitis emergency date (i.e. May 2018). Each shaded region represents a month of coverage.

Figure 11: Estimates of β

(a) Main Results: Estimates of β



(b) Directly Affected vs. Indirectly Affected

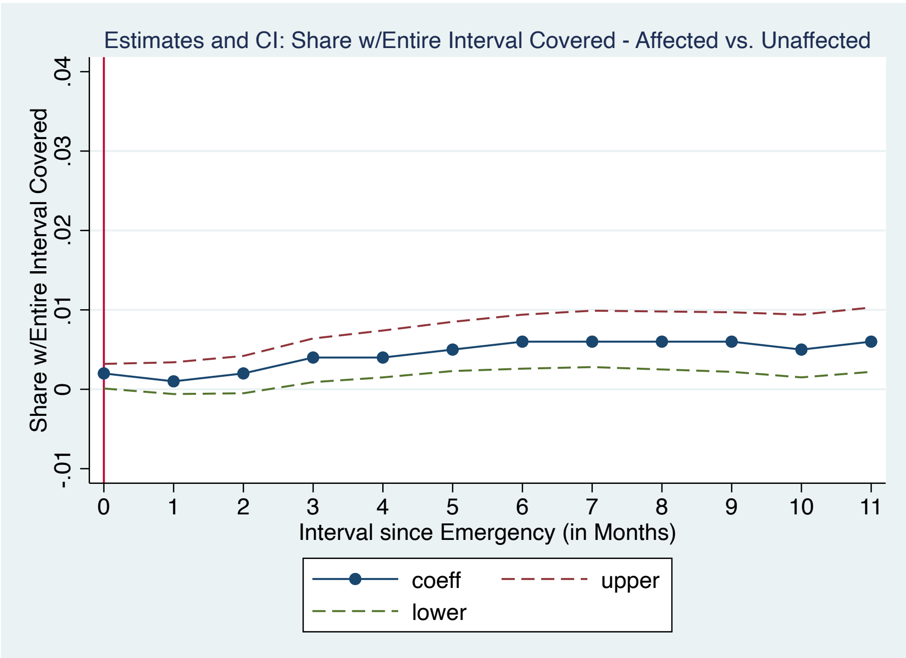
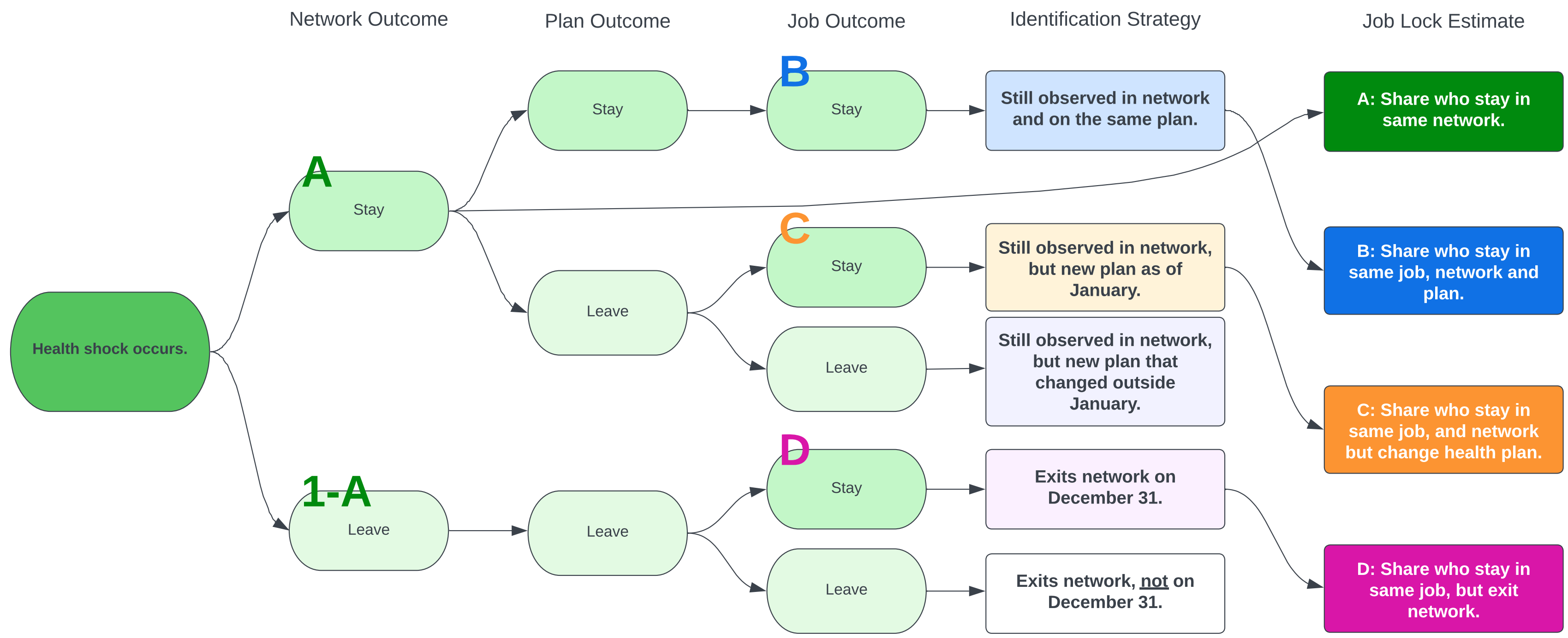


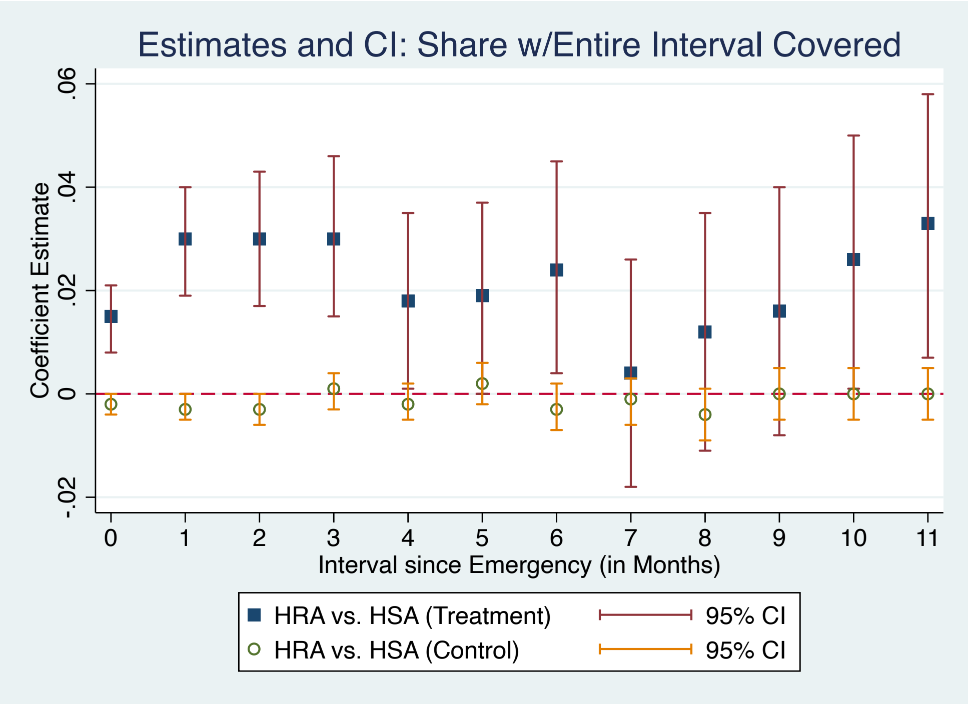
Figure 11a presents estimates of β from Equation (1) where treatment is defined as belonging to a family experiencing an appendicitis emergency. The outcome is a binary variable taking the value one if an individual has insurance coverage (through the insurance network) over the time interval considered and is zero, otherwise. The sample is limited to those with insurance coverage for at least one year prior to the emergency. Figure 11b presents estimates of β 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 12: Network and Job Outcomes after the Health Shock



This figure provides a schema of the network/plan $\times$ job outcomes that occur within one year of the health shock, as well as the data identification strategy used to estimate the relevant shares.

Figure 13: Estimates of β: HRA vs. HSA



This figure presents estimates of β 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.

7.1 List of Qualifying Life Events

The following list provides examples of Qualifying Life Events (QLE) listed on *www.healthcare.gov* (healthcare.gov (2022)).

- Loss of health coverage:
  - Losing existing health coverage, including job-based, individual, and student plans.
  - Losing eligibility for Medicare, Medicaid, or Children's Health Insurance Program (CHIP).
  - Turning 26 and losing coverage through a parent's plan.
- Changes in household:
  - Getting married or divorced.
  - Having a baby or adopting a child.
  - Death in the family.
- Changes in residence:
  - Moving to a different ZIP code or county.
  - A student moving to or from the place they attend school.
  - A seasonal worker moving to or from the place they both live and work.
  - Moving to or from a shelter or other transitional housing.
- Other qualifying events:
  - Changes in your income that affect the coverage you qualify for.
  - Gaining membership in a federally recognized tribe or status as an Alaska Native Claims Settlement Act (ANCSA) Corporation shareholder.
  - Becoming a U.S. citizen.
  - Leaving incarceration (jail or prison).
  - AmeriCorps members starting or ending their service.